American Political Science Review

MARCH 1997
VOLUME 91 NUMBER 1

EDITOR

Ada W. Finifter
Department of Political Science
303 South Kedzie Hall
Michigan State University
East Lansing, MI 48824-1032

BOOK REVIEW EDITOR

Mark I. Lichbach
Department of Political Science
Campus Box 333
University of Colorado at Boulder
Boulder, CO 80309-0333

EDITORIAL BOARD

Paul R. Abramson
Michigan State University
David Austen-Smith
Northwestern University
Lawrence Baum
Ohio State University
Nathaniel Beck
University of California, San Diego
Jonathan Bendor
Stanford University
W. James Booth
Vanderbilt University
Paul Brace
Rice University
Bruce Bueno de Mesquita
Stanford University
Robert Erikson
University of Houston
William Galston
University of Maryland
Barbara Geddes
University of California at Los Angeles
Micheal W. Giles
Emory University
Robert W. Jackman
University of California, Davis
Gary King
Harvard University
Richard Ned Lebow
The Mershon Center
Ohio State University
Robert Luskin
University of Texas at Austin
Paula D. McClain
University of Virginia
Kathleen McGraw
SUNY, Stony Brook
Walter Mebane, Jr.
Cornell University
Karen Remmer
University of New Mexico
Virginia Sapiro
University of Wisconsin-Madison
Arlene Saxonhouse
University of Michigan
Theda Skocpol
Harvard University
Steven B. Smith
Yale University
Laura Stoker
University of California at Berkeley
John L. Sullivan
University of Minnesota
Kaare Strøm
University of California at San Diego
University of Norway, Bergen
Michael Ward
University of Colorado at Boulder
Susan Welch
Pennsylvania State University
John R. Zaller
University of California at Los Angeles

EDITORIAL STAFF

Harriett Posner
Director of Manuscript Processing and Production
Elizabeth Johnston
Sandra Gross
Copy Editors
Sara C. Benesh
Christopher Butler
Kathleen Dowley
Mark S. Hurwitz
Elizabeth R. Kaufer
Matthew Kleiman
Brandon C. Prins
Jeff Reno
APSR Interns
Eric Hirsch
Robert R. Lopez
Dave Reilly
Assistants to the Book Review Editor

The *American Political Science Review* (ISSN-0003-0554) appears quarterly. It is published by the American Political Science Association, 1527 New Hampshire Ave., N.W., Washington, DC 20036, and sent to all members. Dues: Regular Members with gross calendar income: under $30,000, $65; $30,000–$39,999, $80; $40,000–$49,999, $95; $50,000–$69,999, $110; $70,000 and over, $125; Student Members (limited to 5 years), $30; Unemployed members, $30; Retired Members (who have been members 25 years) with gross calendar income under $25,000, $25; $25,000 and over, $45; Life Members, $3,000; Institutional Members: Domestic, $200; Foreign, $220. Dues allocated for a subscription to the *APSR:* $17 for individual members; $128 for institutional members. Changes of address sent to Membership Services APSA. Postmaster: Send address changes to APSR, 1527 New Hampshire Ave., N.W., Washington, DC 20036. Periodicals postage paid at Washington, DC, and at additional mailing offices.

American Political Science Review

Volume 91, Number 1, March 1997

Table of Contents

EDITOR'S NOTES vi

ARTICLES

Unequal Participation: Democracy's Unresolved Dilemma
Arend Lijphart 1

Capabilities, Perception, and Escalation
Bruce Bueno de Mesquita, James D. Morrow, and Ethan R. Zorick 15

Separation-of-Powers Games in the Positive Theory of Congress and Courts
Jeffrey A. Segal 28

Politics Matters: Political Events as Catalysts for Preadult Socialization
David O. Sears and Nicholas A. Valentino 45

Political Institutions and Satisfaction with Democracy: A Cross-National Analysis of Consensus and Majoritarian Systems
Christopher J. Anderson and Christine A. Guillory 66

Institutional Arrangements and the Creation of Social Capital: The Effects of Public School Choice
Mark Schneider, Paul Teske, Melissa Marschall, Michael Mintrom, and Christine Roch 82

Alliances, Preferential Trading Arrangements, and International Trade
Edward D. Mansfield and Rachel Bronson 94

Beyond Quid Pro Quo: What's Wrong with Private Gain from Public Office?
Andrew Stark 108

Legal Theory in the Collapse of Weimar: Contemporary Lessons?
David Dyzenhaus 121

Stability and Coordination in Duverger's Law: A Formal Model of Preelection Polls and Strategic Voting
Mark Fey 135

FORUM

Trends in the Partisan Composition of State Legislatures: A Response to Fiorina
Jeffrey M. Stonecash and Anna M. Agathangelou 148

Professionalism, Realignment, and Representation
Morris P. Fiorina 156

BOOK REVIEWS

Political Theory

Anderson, Kevin. *Lenin, Hegel and Western Marxism: A Critical Study* .... G. W. Smith 163

Barry, Andrew, Thomas Osborne, and Nikolas Rose, eds. *Foucault and Political Reason: Liberalism, Neo-Liberalism and the Rationalities of Government* .... Jodi Dean **164**
Brown, Courtney. *Serpents in the Sand: Essays on the Nonlinear Nature of Politics and Human Destiny* .... L. Douglas Kiel **165**
Carver, Terrell. *Gender Is Not a Synonym for Women* .... Christine Di Stefano **166**
Charvet, John. *The Idea of an Ethical Community* .... Steven Kautz **167**
Dunn, John. *The History of Political Theory and Other Essays* .... Stephen L. Esquith **168**
Haakonssen, Knud. *Natural Law and Moral Philosophy: From Grotius to the Scottish Enlightenment* .... Martha K. Zebrowski **169**
Heyd, David, ed. *Toleration: An Elusive Virtue.*
  Tinder, Glenn. *Tolerance and Community* .... Preston King **170**
Hollis, Martin. *Reason in Action: Essays in the Philosophy of the Social Sciences* .... Harold Kincaid **171**
Kemple, Thomas M. *Reading Marx Writing: Melodrama, the Market and the* Grundrisse .... Brian J. Shaw **171**
Manent, Pierre. *Tocqueville and the Nature of Democracy* .... Sanford Kessler **173**
Meadowcroft, James, ed. *The Liberal Political Tradition: Contemporary Reappraisals* .... Margaret Canovan **174**
Mendel-Reyes, Meta. *Reclaiming Democracy: The Sixties in Politics and Memory* .... Joseph M. Schwartz **175**
Miller, Fred D., Jr. *Nature, Justice, and Rights in Aristotle's* Politics .... Judith A. Swanson **176**
Ranieri, John J. *Eric Voegelin and the Good Society* .... Steven R. McCarl **177**
Rengger, N. J. *Political Theory, Modernity, and Postmodernity* .... Mark Reinhardt **178**
Smith, Gregory Bruce. *Nietzsche, Heidegger, and the Transition to Postmodernity* .... Ronald Beiner **179**
Sreenivasan, Gopal. *The Limits of Lockean Rights in Property* .... John W. Danford **180**
Tinder, Glenn. *Tolerance and Community.* See Heyd, David, above.

American Politics

Boyle, Kevin. *The UAW and the Heyday of American Liberalism, 1945–1968.*
  Green, William C., and Ernest J. Yanarella, eds. *North American Auto Unions in Crisis: Lean Production and Contested Terrain* .... Stephen Amberg **181**
Carter, Dan T. *The Politics of Rage: George Wallace, the Origins of the New Conservatism, and the Transformation of American Politics* .... Charles W. Dunn **183**
Costin, Lela B., Howard Jacob Karger, and David Stoesz. *The Politics of Child Abuse in America* .... Douglas J. Besharov, Jacob W. Dembosky **184**
Diamond, Sara. *Roads to Dominion: Right-wing Movements and Political Power in the United States* .... Michael P. Federici **185**
Flood, Lawrence, ed. *Unions and Public Policy: The New Economy, Law, and Democratic Policies.*
  Gross, James. *Broken Promise: The Subversion of U.S. Labor Relations Policy, 1947–1994* .... Ruth O'Brien **186**
Green, William J., and Ernest J. Yanarella, eds. *North American Auto Unions in Crisis: Lean Production as Contested Terrain.* See Boyle, Kevin, above.
Gross, James. *Broken Promise: The Subversion of U.S. Labor Relations Policy, 1947–1994.* See Flood, Lawrence, above.
Halpern, Stephen C. *On the Limits of the Law: The Ironic Legacy of Title VI of the 1964 Civil Rights Act* .... Stephen L. Wasby **187**
Huddleston, Mark W., and William W. Boyer. *The Higher Civil Service in the United States: Quest for Reform* .... B. Guy Peters **188**
Jacobsohn, Gary Jeffrey, and Susan Dunn, eds. *Diversity and Citizenship: Rediscovering American Nationhood* .... Richard C. Sinopoli **189**
Klinkner, Philip A., ed. *Midterm: The Elections of 1994 in Context* .... Herbert F. Weisberg **190**
Knott, Stephen F. *Secret and Sanctioned: Covert Operations and the American Presidency* .... Loch K. Johnson **191**
Lipset, Seymour Martin. *American Exceptionalism: A Double-Edged Sword* .... Sidney Verba **192**
Mann, Thomas E., and Norman J. Ornstein, eds. *Intensive Care: How Congress Shapes Health Policy* .... Mark Carl Rom **193**

Mezey, Susan Gluck. *Children in Court: Public Policymaking and Federal Court Decisions* . . . . . Susan E. Lawrence **194**
Nelson, Robert H. *Public Lands and Private Rights: The Failure of Scientific Management* . . . . . Paul J. Culhane **195**
Peterson, Paul E., ed. *Classifying by Race* . . . . . Charles S. Bullock III **196**
Rozell, Mark J., and Clyde Wilcox, eds. *God at the Grassroots: The Christian Right in the 1994 Elections* . . . . . Lyman A. Kellstedt **197**
Skrentny, John David. *The Ironies of Affirmative Action: Politics, Culture and Justice in America* . . . . . Kate Greene **197**
Souder, Jon A., and Sally K. Fairfax. *State Trust Lands: History, Management, and Sustainable Use* . . . . . Charles Davis **199**
Stuckey, Mary E., ed. *Theory and Practice of Political Communication Research* . . . . . Doris A. Graber **199**
Tarr, G. Alan, ed. *Constitutional Politics in the States: Contemporary Controversies and Historical Patterns* . . . . . Bradley C. Canon **200**
Thurber, James A., ed. *Rivals for Power: Presidential-Congressional Relations* . . . . . Robert J. Spitzer **201**
Warshaw, Shirley Anne. *Powersharing: White House–Cabinet Relations in the Modern American Presidency* . . . . . Thomas J. Weko **202**
Wasby, Stephen L. *Race Relations Litigation in an Age of Complexity* . . . . . Susan M. Olson **203**
Westerfield, Donald L. *War Powers: The Preisdent, the Congress and the Question of War* . . . . . Louis Fisher **204**
Wetstein, Matthew E. *Abortion Rates in the United States: The Influence of Opinion and Policy* . . . . . Ted G. Jelen **205**

Comparative Politics

Buchanan, Paul G. *State, Labor, Capital: Democratizing Class Relations in the Southern Cone* . . . . . Ben Ross Schneider **206**
Callon, Scott. *Divided Sun: MITI and the Breakdown of Japanese High-Tech Industrial Policy, 1975–1993* . . . . . Maurice Wright **207**
Chang, Ha-Joon, and Robert Rowthorn, eds. *The Role of the State in Economic Change* . . . . . Chung H. Lee **208**
Chen, Feng. *Economic Transition and Political Legitimacy in Post-Mao China.*
  Naughton, Barry. *Growing Out of the Plan: Chinese Economic Reform, 1978–1993.*
  Sun, Yan. *The Chinese Reassessment of Socialism, 1979–1992* . . . . . Mark Blecher **209**
Friedman, Francine. *The Bosnian Muslims: Denial of a Nation* . . . . . Zachary Irwin **211**
Goldhagen, Daniel Jonah. *Hitler's Willing Executioners: Ordinary Germans and the Holocaust* . . . . . Kristen R. Monroe **212**
Hahn, Jeffrey W., ed. *Democratization in Russia: The Development of Legislative Institutions* . . . . . Terry D. Clark **213**
Hancock, Ralph C., and L. Gary Lambert, eds. *The Legacy of the French Revolution* . . . . . John A. Rohr **214**
Healey, John, and William Tordoff, eds. *Votes and Budgets: Comparative Studies in Accountable Governance in the South* . . . . . Goran Hyden **215**
Hesse, Joachim Jens, and Nevil Johnson, eds. *Constitutional Policy and Change in Europe* . . . . . Michel Rosenfield **215**
Jacob, Herbert, Erhard Blankenburg, Herbert M. Kritzer, Doris Marie Provine, and Joseph Sanders. *Courts, Law, and Politics in Comparative Perspective* . . . . . Martin Shapiro **216**
Kandiyoti, Deniz, ed. *Gendering the Middle East: Emerging Perspectives* . . . . . As'ad AbuKhalil **217**
King, Desmond. *Actively Seeking Work? The Politics of Unemployment and Welfare Policy in the United States and Great Britain* . . . . . Sanford F. Schram **218**
Naughton, Barry. *Growing Out of the Plan: Chinese Economic Reform, 1978–1993.* See Chen, Feng, above.
O'Brien, David M., with Yasuo Ohkoshi. *To Dream of Dreams: Religious Freedom and Constitutional Politics in Postwar Japan* . . . . . Daniel A. Metraux **219**
Peleg, Ilan. *Human Rights in the West Bank and Gaza: Legacy and Politics* . . . . . David P. Forsythe **220**
Pierson, Christopher. *Socialism After Communism: The New Market Socialism* . . . . . David Schweickart **221**
Singerman, Diane. *Avenues of Participation: Family, Politics, and Networks in Urban Quarters of Cairo* . . . . . Robert Bianchi **222**

Skocpol, Theda. *Social Revolutions in the Modern World* .... John Foran **222**
Sun, Yan. *The Chinese Reassessment of Socialism, 1979–1992.* See Chen, Feng, above.
Verdery, Katherine. *What Was Socialism, and What Comes Next?* .... Sabrina P. Ramet **223**
Weiqun, Gu. *Conflicts of Divided Nations: The Cases of China and Korea* .... George T. Crane **224**
Wiseman, John A. *The New Struggle for Democracy in Africa* .... Matthew J. Costello **224**
Wou, Odoric Y. K. *Mobilizing the Masses: Building Revolution in Henan* .... Tang Tsou **225**

International Relations

Anaya, S. James. *Indigenous Peoples in International Law* .... Alison Brysk **227**
Bartelson, Jens. *A Genealogy of Sovereignty* .... Cynthia Weber **228**
Elkind, David. *Beyond Sovereignty: Territory and Political Economy in the Twenty-First Century* .... Saul H. Mendlovitz **229**
Foot, Rosemary. *The Practice of Power: U.S. Relations with China since 1949* .... Samuel S. Kim **230**
Guelke, Adrian. *The Age of Terrorism and the International Political System* .... Martha Crenshaw **231**
Henderson, Errol Anthony. *Afrocentrism and World Politics: Towards a New Paradigm* .... Molefi Kete Asante **232**
Hey, Jeanne A. K. *Theories of Dependent Foreign Policy and the Case of Ecuador in the 1980s* .... Carl Bromley, Howard J. Wiarda **233**
Hopmann, P. Terrence. *The Negotiation Process and the Resolution of International Conflicts* .... Russell Leigh Moses **234**
Hunt, Michael H. *The Genesis of Chinese Communist Foreign Policy* .... Xiaobo Hu **235**
Keohane, Robert O., and Elinor Ostrom, eds. *Local Commons and Global Interdependence: Heterogeneity and Cooperation in Two Domains* .... Barry B. Hughes **236**
Kirshner, Jonathan, *Currency and Coercion: The Political Economy of International Monetary Power* .... Philip G. Cerny **236**
Knox, Paul L., and Peter J. Taylor, eds. *World Cities in a World System* .... Susan Clarke **237**
Lapid, Yosef, and Friedrich Kratochwil, eds. *The Return of Culture and Identity in IR Theory* .... Mark Neufeld **238**
Larkin, Bruce D. *Nuclear Designs: Great Britain, France and China in the Global Governance of Nuclear Arms* .... Weixing Hu **239**
Mercer, Jonathan. *Reputation and International Politics* .... Stephen Peter Rosen **240**
Merritt, Richard L. *Democracy Imposed: U.S. Occupation Policy and the German Public, 1945–1949* .... Gerald Braunthal **241**
Rhodes, Carolyn, and Sonia Mazey, eds. *The State of the European Union, Volume 3: Building a European Polity?* .... Michael Baun **242**
Sagan, Scott, and Kenneth Waltz. *The Spread of Nuclear Weapons: A Debate* .... Richard Rosecrance **243**
Wiarda, Howard J. *Democracy and Its Discontents: Development, Interdependence, and U.S. Policy in Latin America* .... John D. Martz **244**

FORTHCOMING ARTICLES

The Evolutionary Stability of Cooperation, **Jonathan Bendor and Piotr Swistak**
The Strategic Timing of Position Taking in Congress: A Study of the North American Free Trade Agreement, **Janet M. Box-Steffensmeier, Laura W. Arnold, and Christopher J. Zorn**
The Public Consequences of Private Inequality: Family Life and Citizen Participation, **Nancy Burns, Key Lehman Schlozman, and Sidney Verba**
Crime and Punishment: The Role of Norms in Crisis Bargaining, **Christopher Gelpi**
The Quixotic Search for Consensus on the U.S. Supreme Court: a Cross-Judicial Empirical Analysis of the Rehnquist Court Justices, **Scott D. Gerber and Keeok Park**
Private Property Versus Markets: Democratic and Communitarian Critiques of Capitalism, **Claudio J. Katz**
The Private Production of Public Goods: Organizational Maintenance, Managers' Objectives, and Collective Goals, **Robert C. Lowry**
Citizenship and Ethnicity: An Examination of Two Transition Moments in Kenyan Politics, **Stephen N. Ndegwa**
Aristotle and the Recovery of Political Judgment, **Richard S. Ruderman**
The Political Foundations of Democracy and the Rule of the Law, **Barry R. Weingast**
Distance Versus Direction: The Illusory Defeat of Proximity Theory of Electoral Choice, **Anders Westholm**

American Political Science Association

1527 New Hampshire Avenue,
Washington, D.C. 20036

PRESIDENT:
Elinor Ostrom
Indiana University

PRESIDENT ELECT:
M. Kent Jennings
University of California, Santa Barbara

VICE PRESIDENTS:
Suzanne Berger
Massachusetts Institute of Technology

William E. Nelson, Jr.
The Ohio State University

G. Bingham Powell
University of Rochester

SECRETARY:
Virginia Sapiro
University of Wisconsin, Madison

TREASURER:
Gary C. Jacobson
University of California, San Diego

EXECUTIVE DIRECTOR:
Catherine E. Rudder

PROGRAM CO-CHAIRS:
James Alt
Harvard University

Margaret Levi
University of Washington

EDITOR, *APSR*
Ada W. Finifter
Michigan State University

COUNCIL, 1995–97:
Ruth Berins Collier
University of California, Berkeley

Micheal W. Giles
Emory University

Rodney E. Hero
University of Colorado, Boulder

Pamela K. Jensen
Kenyon College

Bruce W. Jentleson
University of California, Davis

Richard J. Payne
Illinois State University

Ian Shapiro
Yale University

Paul Sniderman
Stanford University

COUNCIL, 1996–98:
Martha Ackelsberg
Smith College

E. J. Dionne
The Washington Post

Kathie Stromlie Golden
Morris Brown College

John E. Jackson
University of Michigan

Michael Laver
Trinity College, Dublin, Ireland

David D. Laitin
University of Chicago

John G. Ruggie
Columbia University

Joseph Stewart, Jr.
University of New Mexico

Editor's Notes

During summer 1997, a number of political science journals, including the *Review*, will not process new submissions from July 15 to August 15. In the last year, the *Review* received 550 manuscripts, but there is a noticeable periodicity to the flow. Our months of heaviest submission are June, at the end of the American academic year, and October, following the annual meeting of the American Political Science Association. From mid-July to mid-August we receive relatively fewer manuscripts but, paradoxically, we are frequently less able to put them into the review process as quickly because many members of the Editorial Board (who are consulted on all new submissions) and likely reviewers are traveling for research purposes, moving, or on vacation, and our very small *APSR* staff may also be short-handed. At the same time, the constant pressure of the daily routine leaves academic journal editors with little time (usually none) for long-range planning or catch-up on the inevitable backlogs that develop.

In response to these problems, and in collaboration with the editors of the American regional political science journals, we are experimenting with a new policy on summer submissions and will not process *new* manuscripts between July 15 and August 15, 1997. We will continue to accept invited resubmissions and reviews during this period and will process them normally. The editors involved hope that this policy will allow the journals to schedule vacation time for their own staffs, provide a time for long-range planning, and avoid disappointing authors who hopefully but incorrectly think that submitting during a slow time in the academic year will result in shorter turnaround time. Not least important, we want to be able to be completely up to date in processing the previous year's manuscripts before the inevitable beginning in mid-September of the incoming tide of manuscripts associated with the annual meeting.

The editors involved in this submission experiment do not expect or desire that this marginal restructuring of the submission process will lessen the number of manuscripts that come to their journals. Rather our objective is to process a full year's worth of manuscripts at a more efficient level in eleven months. We therefore ask that authors submit new manuscripts to arrive either before or after the July 15–August 15 period. New manuscripts that arrive during this period will be held until normal processing resumes. For further information on the journals participating in this experiment, see the announcement in the March 1997 issue of *PS*.

INSTRUCTIONS TO CONTRIBUTORS

General Considerations

The *American Political Science Review* strives to publish scholarly research of exceptional merit, demonstrating the highest standards of excellence in conceptualization, exposition, methodology, and craftsmanship. Because the *Review* reaches a diverse audience of scholars and practitioners, contributors should demonstrate how their analysis illuminates a significant research problem, or answers an important research question, of general interest in political science. For the same reason, authors should strive for a presentation that will be understandable to as many scholars as possible, consistent with the nature of their material.

Manuscripts being submitted for publication should be sent to Professor Ada W. Finifter, Editor, *American Political Science Review*, Department of Political Science, Michigan State University, 303 South Kedzie Hall, East Lansing, MI 48824-1032. Correspondence concerning manuscripts under review may be sent to the same address or, preferably, by e-mail to apsr@ssc.msu.edu. Please note that the *APSR* will not process *new* manuscripts between July 15 and August 15, 1997. *New manuscripts* should be mailed to arrive either before or after this period. *Invited revisions and reviews* may be submitted at any time and will be processed upon arrival.

The *APSR* does not consider papers currently under review at other journals or that duplicate or overlap with parts of larger manuscripts that have been submitted to publishers. Moreover, since space is at a premium, submission of articles substantially similar to articles submitted or published elsewhere, or to part of a book or other larger work, is strongly discouraged. Authors should discuss any such related publications in a cover letter to the Editor.

For manuscripts containing quantitative evidence and analysis, authors should describe their empirical procedures in sufficient detail to permit reviewers to understand and evaluate what has been done and, in the event the article is accepted for publication, to permit other scholars to carry out similar analyses on other data sets. It is desirable for articles to be self-contained and authors should not refer readers to other publications for descriptions of basic research procedures such as sampling methods, question wordings, or experimental protocols. As part of the review process, authors may be asked to submit additional documentation if procedures are not sufficiently clear. If readers are advised that additional information is available from the author, printed copies of that information should be submitted with the manuscript. If the amount of this supplementary information is extensive, please inquire about alternate procedures.

Manuscripts that are largely or entirely critiques or commentaries on previously published articles will be reviewed using the same general procedures as for regular articles with one exception. In addition to the usual number of reviewers, such manuscripts will also be sent to the scholar(s) whose work is being critiqued, in the same anonymous form as they are sent to other reviewers. Comments from the original author(s) to the editor will be invited as a supplement to the advice of regular reviewers. There are several purposes for this notice to the original author(s): (1) to encourage review of the details of analyses or research procedures that might occasionally escape the notice of disinterested reviewers; (2) to enable prompt publication of critiques by supplying critiqued authors with early notice of their existence and, therefore, more adequate time to reply; (3) as a courtesy to critiqued authors. Authors of critiques should therefore send as many additional copies of their manuscripts as will be required for this purpose.

Manuscripts must not be longer than 45 pages including text, all tables and figures, notes, references, and appendices. Font sizes smaller than 10 point should not be used for any part of the paper nor should there be more than 300 words per page. The entire paper must be double-spaced, with the sole exception of tables for which double-spacing would require a second page otherwise not needed. All pages should be numbered in one sequence and text should be formatted using a normal single column no wider than 6½ inches, as is typical for manuscripts (rather than the double-column format of the published version of the *Review*), and printed on one side of the page only. Include an abstract of no more than 150 words. The *American Political Science Review* style of embedded citations should be used, and there should be a separate list of references at the end of the article. Please do not use footnotes for simple citations. Since these specifications are designed to make it easier for reviewers to read and evaluate papers, manuscripts not adhering to these guidelines are subject to being rejected without review.

For submission purposes, authors who wish to do so may place footnotes at the bottom of the pages instead of using endnotes and may locate tables and figures (on separate pages and only one to a page) approximately where they fall in the text. However, manuscripts accepted for publication must be submitted with endnotes, and with tables and figures on separate pages at the back of the manuscript with standard indications of text placement, e.g., [Table 3 about here]. In deciding how to format your initial submission, please consider the necessity of making these changes if your paper is accepted. Authors of accepted manuscripts are also required to submit camera-ready copy of graphs or other types of figures. Instructions will be provided.

The *American Political Science Review* citation format requires inclusion of month, season, or issue number (in that order of preference if a cited journal provides more than one) as part of the volume and page identification. For example, a reference to an article in the *Review* would be given as 90(September): 475–87, rather than 90:475–87. On other questions of form, please follow the example of a recent issue of the *Review* or refer to the *Style Manual for Political Science*, a publication of the American Political Science Association's Committee on Publications. It is available from the Association's office (see "Other Correspondence" below for address). For unusual style or formatting issues, authors may refer to *The Chicago Manual of Style*, 14th ed.

Specific Procedures

Please follow these specific procedures for submission:

1. Submit *five* copies of manuscripts. Please review all pages of all copies to make sure copies are complete. To comply with the *Review*'s policy of double-blind peer reviews, four of the submitted manuscripts should have no identifying references. The first page of the four anonymous copies should contain only the title and an abstract of no more than 150 words. If it is important to the development of the paper that an author's previous publications be cited, whenever possible this should be done in a way that does not make the authorship of the submitted paper obvious. This is usually most easily accomplished by referring to oneself in the third person. In such cases, normal references to the work cited should appear in the list of references. If numerous references that would suggest authorship are required, authors may delete their names from embedded references and substitute "Author [date]" (use actual date of other work). When attributing work to "Author," *references to the works in question should be completely omitted from the list of references.* Do not leave blank lines where the author's references would otherwise appear.
2. The first page of the identified copy should contain the name, academic rank, institutional affiliation, and contact information (address, telephone, fax, e-mail) for all authors and, in the case of multiple authors, an indication of the author who will receive correspondence. The identified copy should contain any relevant citations to the author's previous work. If there are any, please mark this copy with "Contains author citations" at the top of the first page.
3. Please also send a loose copy of a page containing the full title of the article, the names of all authors, and the abstract.
4. Authors having access to e-mail should send a copy of the abstract to apsr@ssc.msu.edu, when the submission is mailed. The subject line should contain the first listed author's last name followed by the word *Abstract*. The body of the message should contain the title of the paper, the names of all authors, and the abstract (as on the loose page described above). E-mail submission of the abstract is not required but will help us process your manuscript more efficiently.
5. Authors should not fax abstracts, send any part of the paper itself by e-mail, or send the abstract by e-mail before their submission packet is in the mail.

No copies of submitted manuscripts can be returned. Since submission procedures may be modified in the future, authors are encouraged to check for current instructions in the most recent issue of the *Review* or at our Internet web site, http://www.ssc.msu.edu/~apsr/.

BOOK REVIEWS

All book review correspondence should be sent to the Book Review Editor, Professor Mark Lichbach, American Political

Science Review Book Review, Department of Political Science, University of Colorado, Box 333, Boulder, CO 80309. (E-mail: apsrbook@colorado.edu.) Authors wishing their book to be considered for review should ask their publisher to send a copy to the Book Review Editor. Those interested in reviewing books should send their vita to the Book Review Editor.

OTHER CORRESPONDENCE

The American Political Science Association's address, telephone and fax are: 1527 New Hampshire Avenue, NW, Washington, D.C. 20036, (202)483-2512, and fax (202)483-2657. E-mail: apsa@apsa.com. Please direct correspondence as follows:

Information, including news and notes, for *PS*
Dr. Robert J-P. Hauck, Editor, *PS*
E-mail: rhauck@apsa.com

Circulation and subscription correspondence (domestic claims for nonreceipt of issues must be made within four months of the month of publication; overseas claims, eight months)
Helen Cook, Director of Members Service
E-mail: membership@apsa.com

Reprint permissions
James McCartin
E-mail: jmccartin@apsa.com

Advertising information and rates
Lauren Harris, Advertising Manager
E-mail: lharris@apsa.com

EXPEDITING REQUESTS FOR COPYING *APSR* AND *PS* ARTICLES FOR CLASS USE

The APSA already has an agreement with the Copyright Clearance Center (CCC) whereby bookstores and copy centers can receive expedited clearance to copy articles from the *APSR* and *PS* in compliance with the Association's policies and applicable fees. In regard to these permission fees, there is no charge for the first ten copies of an article. Any additional copies are 75 cents each. The Copyright Clearance Center's address, telephone, and fax are: 222 Rosewood Drive, Danvers, MA 01923, (508)750-8400, and fax (508)750-4744.

The Association of American Publishers (AAP) has created a standardized form for college faculty to submit to a copy center or bookstore in order to request copyrighted material for course packs. The form is available through the CCC, which will handle copyright permissions. Authors of *APSR* articles may use their own articles in course packs or other printed materials without payment of royalty fees.

INDEXING

Articles and notes appearing in the *Review* before the June 1953 issue were indexed in *The Reader's Guide to Periodical Literature.* Current issues are indexed by *International Political Science Abstracts*, *United States Political Science Documents*, and *Social Sciences Index.* Microfilm of the *Review*, beginning with Volume 1, may be obtained from University Microfilms, 300 North Zeeb Road, Ann Arbor, MI 48106. A *Cumulative Index of the Review*, Volumes 1–62, 1906–1968, also may be obtained from University Microfilms. Articles appearing in the *Review* are listed in *ABC Pol Sci* and *Current Contents: Behavioral, Social and Management Sciences.* Book Reviews are indexed in *Book Review Index.*

American Political Science Review Vol. 91, No. 1 March 1997

Unequal Participation: Democracy's Unresolved Dilemma

Presidential Address, American Political Science Association, 1996

AREND LIJPHART *University of California, San Diego*

Low voter turnout is a serious democratic problem for five reasons: (1) It means unequal turnout that is systematically biased against less well-to-do citizens. (2) Unequal turnout spells unequal political influence. (3) U.S. voter turnout is especially low, but, measured as percent of voting-age population, it is also relatively low in most other countries. (4) Turnout in midterm, regional, local, and supranational elections—less salient but by no means unimportant elections—tends to be especially poor. (5) Turnout appears to be declining everywhere. The problem of inequality can be solved by institutional mechanisms that maximize turnout. One option is the combination of voter-friendly registration rules, proportional representation, infrequent elections, weekend voting, and holding less salient elections concurrently with the most important national elections. The other option, which can maximize turnout by itself, is compulsory voting. Its advantages far outweigh the normative and practical objections to it.

EQUALITY VERSUS PARTICIPATION

Political equality and political participation are both basic democratic ideals. In principle, they are perfectly compatible. In practice, however, as political scientists have known for a long time, participation is highly unequal. And unequal participation spells unequal influence—a major dilemma for representative democracy in which the "democratic responsiveness [of elected officials] depends on citizen participation" (Verba 1996, 2), and a serious problem even if participation is not regarded mainly as a representational instrument but as an intrinsic democratic good (Arendt 1958, Barber 1984, Pateman 1970). Moreover, as political scientists have also known for a long time, the inequality of representation and influence are not randomly distributed but systematically biased in favor of more privileged citizens—those with higher incomes, greater wealth, and better education—and against less advantaged citizens.

This systematic class bias applies with special force to the more intensive and time-consuming forms of participation. Steven J. Rosenstone and John Mark Hansen (1993, 238) found that, in the United States, the smaller the number of participants in political activity, the greater the inequality in participation. In other countries, too, it is especially the more advantaged citizens who engage in these intensive modes of participation—both conventional activities such as working in election campaigns, contacting government officials, contributing money to parties or candidates, and working informally in the community (Verba, Nie, and Kim 1978, 286–95) and unconventional activities like participation in demonstrations, boycotts, rent and tax strikes, occupying buildings, and blocking traffic (Marsh and Kaase 1979, 100, 112–26).

Voting is less unequal than other forms of participation, but it is far from unbiased. The bias is especially strong in the United States where "no matter which form citizen participation takes, the pattern of class equality is unbroken," and where, over time, the level of voting participation and class inequality are strongly and negatively linked: "When [relatively] many citizens turn out to vote, they are more representative of the electorate than when fewer people vote.... Class inequality in participation was greatest in the high-turnout elections of the 1960s and least in the low-turnout elections of the 1980s. As turnout declined between 1960 and 1988, class inequalities multiplied" (Rosenstone and Hansen 1993, 238, 241; see also Burnham 1980, 1987). Although generally not as strong, the same pattern of inequality can be seen in other democracies.

It is interesting to note that, at the end of the nineteenth and the beginning of the twentieth century, when universal suffrage was being adopted in many countries, political analysts tended to assume that it would be the better educated and more prosperous who would make the rational choice not to bother to vote. As a French observer put it in 1896, "The intellectual elite of the people asks itself whether it is worthwhile to cast a vote which is doomed to drown among the votes of the great crowd" (cited in Tingsten 1937, 184). But empirical studies soon showed that socioeconomic status and voting were positively, not negatively, linked. In his study of voting in the 1924 presidential election in the city of Chicago, Harold F.

Arend Lijphart is Research Professor of Political Science, University of California, San Diego, La Jolla, CA 92093-0521.

Earlier versions of this address were delivered to the annual meetings of the Southern, Northeastern, Western, and Southwestern Political Science Associations on November 3 and 9, 1995, and March 15 and 22, 1996, respectively. I am very grateful for the comments that I received from participants in these meetings. I also thank the many other scholars who have generously responded to my requests for information and advice as I was preparing my address: Stephen Ansolabehere, Ken Coghill, Michael J. Coppedge, Robert Darcy, Hans Daalder, Wilfried Dewachter, Lieven De Winter, Panayote E. Dimitras, Zachary Elkins, Peter Farago, Mark N. Franklin, Richard L. Hasen, Wolfgang Hirczy, Colin A. Hughes, Luc Huyse, Galen A. Irwin, Robert W. Jackman, Gary C. Jacobson, Mark P. Jones, Richard S. Katz, Hans-Dieter Klingemann, Sanford A. Lakoff, Malcolm Mackerras, Ian McAllister, José E. Molina Vega, Kenneth Newton, Dieter Nohlen, Anton Pelinka, Samuel L. Popkin, Timothy J. Power, Benjamin Radcliff, Robert Richie, Fred W. Riggs, Ronald Rogowski, W. P. Secker, Matthew S. Shugart, Jürg Steiner, Alan Wertheimer, and Raymond E. Wolfinger.

Gosnell (1927, 98) found that turnout increased with economic status and that "the more schooling the individual has the more likely he [or she] is to register and vote in presidential elections." In an article in the *American Political Science Review* two years earlier, the same clear pattern was reported on the basis of a voting study in the small Ohio town of Delaware (Arneson 1925). Herbert Tingsten (1937, 155) reviewed a large number of voting studies in Switzerland, Germany, Denmark, Austria, the United States, and Sweden, conducted between 1907 and 1933, and formulated "the general rule that the voting frequency rises with rising social standard."

Can the democratic dilemma of unequal participation be resolved? With the possible exception of financial contributions,[1] little can be done to equalize participation in the more intensive activities; mobilizing more people to participate appears to be of little help because, as Verba (1996, 7) laments, "for most activity, the forces of mobilization bring in the same people who would be active spontaneously." But a partial solution to the dilemma is to make the most basic form of participation, namely voting, as equal as possible—especially important as a "democratic counterweight" (Teixeira 1992, 4) to other forms of participation which are bound to remain unequal. And the obvious way to make voting more equal is to maximize voting turnout. The democratic goal should be not just universal *suffrage* but universal or near-universal *turnout*—in line with Tingsten's (1937, 230) "law of dispersion," which states that the probability of differences in voting turnout "is smaller the higher the general participation is.... The chances of dispersion... are inversely proportional to the electoral participation."[2]

On the basis of studies from the 1930s (Gosnell 1930, Tingsten 1937) to the 1980s and 1990s (Franklin 1996; Franklin, van der Eijk, and Oppenhuis 1996; Jackman 1987; Jackman and Miller 1995; Powell 1980, 1986), we know a great deal about the institutional mechanisms that can increase turnout, such as user-friendly registration rules, proportional election formulas, relatively infrequent elections, weekend voting, and compulsory voting. And all of these studies, from the 1930s on, have found that compulsory voting is a particularly effective method to achieve high turnout—in spite of generally low penalties (comparable to a fine for parking violations), lax enforcement (more lenient than the enforcement of parking rules), and the secrecy of the ballot which means that an actual vote cannot be compelled in the first place.

Compulsory "voting" is therefore a misnomer: All that can be required in practice is attendance at the polls; hence the least intrusive, but sufficient, form of compulsory voting is the requirement to appear at the polling station on election day without any further duty to mark a ballot or even to accept a ballot. This was the rule in the Netherlands from 1917 until the abolition of compulsory voting in 1970 (Adviescommissie Opkomstplicht 1967; Irwin 1974, 313).[3] More democracies have used the compulsory vote than is commonly recognized: Australia, Italy, Greece, Belgium, the Netherlands, Luxembourg, Austria (several *Länder*), Switzerland (a few cantons), and most Latin American countries (Fernández Baeza forthcoming; Fornos 1996; Hirczy 1994; Ochoa 1987, 866–7).[4]

In addition to being an effective enhancer of turnout in practice, the basic logic of compulsory voting as an egalitarian instrument is also strong. As Sidney Verba, Norman H. Nie, and Jae-On Kim (1978, 6) argue, to make political participation perfectly equal, one needs both a "ceiling"—a prescribed maximum—and a "floor"—a prescribed minimum—for activities of various kinds. For voting participation this means that "each citizen is allowed one and only one vote.... Such a *ceiling* goes a long way toward equalizing political participation, but it does not eliminate the possibility that citizens will differ in their use of the franchise. Turnout is usually related to socioeconomic status. Thus it may be necessary to place a *floor* under political activity as well, *to make it compulsory*" (emphasis added).

UNEQUAL TURNOUT AND UNEQUAL INFLUENCE

Before turning to the various institutional methods for raising turnout, including compulsory voting, let me first review the empirical evidence and theoretical arguments concerning the problems of low voter turnout and class bias. There are several serious reasons why democrats should worry about these problems.

First of all, as already indicated, low voter turnout means unequal and socioeconomically biased turnout. This pattern is so clear, strong, and well known in the United States that it does not need to be belabored further. Compared with the United States, the class bias in other democracies tends to be weaker—leading some analysts to regard it as an almost unique American phenomenon (Abramson 1995, 918; Piven and Cloward 1988a, 117–9). There is, however, abundant evidence of the same class bias, albeit usually not as strong, in other democracies. In Switzerland, the other major example of a Western democracy with low levels of turnout, the participation gap between the least and most highly educated citizens in the March 1991 referendum was 37 percentage points; Wolf Linder (1994, 95–6) calls this a "typical profile of a popular vote," and concludes that "especially when participation is low, the choir of Swiss direct democracy sings in upper- or middle-class tones." In survey data covering referenda between 1981 and 1991, the gap was almost 25 percentage points (Mottier 1993, 134). The class bias in turnout also affects Swiss parliamentary elections (Farago 1996, 11–2; Sidjanski 1983, 107).

---

[1] Making financial contributions to campaigns, parties, and candidates is an exceptional activity in two respects. One is that it is characterized by an income bias that is greater than in all other modes of participation (Verba, Schlozman, and Brady 1995, 516–7). The other is that, in principle, it can be equalized by complete and exclusive public financing of political parties and campaigns—a policy that, however, is more difficult to apply in countries like the United States with its "candidate-centered politics" (Wattenberg 1991) than in countries with strong and disciplined parties.

[2] Of course, another crucially important reason to aim for maximum turnout is democratic legitimacy (Hasen 1996, 2165–6; Teixeira 1992, 3, 101–2).

[3] Even in Australia, where the voter is actually obligated to deposit a ballot in the ballot box, compulsory "voting" is still a misnomer. In the words of a former Australian senator and proponent of compulsory voting: "What the law requires is that [electors] turn up at a polling booth and take a ballot paper. They are not compelled to fill in that ballot paper and have an absolute right not to vote by placing a blank or spoiled ballot paper in the ballot box. That is their unqualified right which only a small number choose to exercise" (Puplick 1995, 3–4).

[4] Some Latin American democracies exempt large groups such as illiterates and people over age 70 from the obligation to vote (Nohlen 1993). The exclusion of illiterate citizens, in particular, reintroduces a significant class bias in voting.

In countries with higher turnout, as expected, the link between socioeconomic status and turnout tends to be less strong, often not strong enough to be statistically significant and sometimes even negative. However, G. Bingham Powell, Jr. (1986, 27–8) combined data for seven European nations and Canada and found a consistent effect of the level of education on turnout: a difference of 10 percentage points between the lowest and highest of five education levels and a consistent increase of 2 to 3 percentage points at each higher level in the averages of eight nations. A similar study of six Central American countries also reports mixed results, but these averages show similar turnout increases at higher educational levels and a difference of 12 percentage points between the highest and lowest levels, with the "more dramatic differences . . . found in countries with lower turnout rates" (Seligson et al. 1995, 166–71).

Richard Topf (1995, 48–9), who surveys data from 16 European countries in six periods since 1960, finds several instances in which the least educated cohorts actually have slightly higher turnouts than the most highly educated—contrary to the expected pattern—and concludes that there is "no generalized education effect for voting." His own figures, however, show that the instances of the expected positive link between educational level and turnout are four times more numerous than the deviant instances; without the countries with compulsory voting the ratio is almost five to one. Similarly, a study of the 1989 European Parliament elections in the 12 member countries finds several negative correlations between levels of education, income, and social class on the one hand and voting turnout on the other, but positive correlations prevail by a better than two-to-one ratio; without the four countries with compulsory voting, the ratio is higher than three to one (Oppenhuis 1995, 186–90). The same expected, but not huge, class bias is also the usual finding in Russell B. Dalton's (1996, 57–8) comparative analysis of the United Kingdom, France, and Germany, as well as in single-country studies of these countries plus Spain and the Netherlands (Denters 1995; Denver 1995; Font and Virós 1995; Justel 1995; Särlvik and Crewe 1983, 79; Schultze 1995).

A slight class bias sometimes still turns up even in countries with compulsory voting, and hence high turnout. For instance, even in Australia where about 95% of the registered voters usually vote, Ian McAllister (1986) finds that slightly higher turnouts give a perceptible boost to the Labor Party and that slightly lower turnouts benefit the parties of the right; he also estimates that the hypothetical abolition of compulsory voting would strengthen this pattern and would give the political right "an inbuilt advantage." In the well-known graph in the first chapter of their *Participation and Political Equality*, Verba, Nie, and Kim (1978, 7) strikingly illustrate the increase in class bias that resulted from the abolition of compulsory voting in the Netherlands in 1970. For five educational groups, the reported turnout rates varied between 66 and 87%. Compared with these unequal turnouts, the last parliamentary election that was still conducted under compulsory voting, in 1967, showed turnouts for all groups above 90%—but there was still a slight class bias: turnouts increased gradually from 93% in the lowest educational group to 98 and 97% in the two groups with the most education.

In Belgium, surveys have found little or no relationship between educational level and voting participation. However, they have also discovered that, if compulsory voting were abolished, turnout would drop from well over 90% to about 60%, resulting in a strong class bias from which the more conservative parties would benefit (Ackaert and De Winter 1993, 77–9; 1996; De Winter and Ackaert 1994, 87–9). Similarly, Venezuela had high turnouts in its elections under compulsory voting until the mid-1980s and, like Belgium, relatively little class bias in turnout. Here, too, however, a survey found that, under voluntary voting, turnout would decline dramatically, to 48%, and that "electoral demobilization would introduce socioeconomic distinctions in voting turnout" (Baloyra and Martz 1979, 71; see also Molina Vega 1991).

In the early 1960s, two authoritative volumes summarized the most important findings of political scientists and sociologists. On the subject of voter turnout, Seymour Martin Lipset (1960, 182) stated that "patterns of voting participation are strikingly the same in various countries: Germany, Sweden, America, Norway, Finland, and many others for which we have data. . . . The better educated [vote] more than the less educated; . . . higher-status persons, more than lower." Similarly, one of the findings in Bernard Berelson and Gary A. Steiner's (1964, 423) *Inventory of Scientific Findings* was that "the higher a person's socioeconomic and educational level—especially the latter—the higher his [or her] political interest, participation, and voting turnout." More than three decades later, these conclusions are clearly still valid.[5]

The second reason why low and unequal voting turnout should be a serious concern is that who votes, and who doesn't, has important consequences for who gets elected and for the content of public policies. What is the significance, V. O. Key (1949, 527) asked, of group differences in voting and nonvoting? And he answered: "The blunt truth is that politicians and officials are under no compulsion to pay much heed to classes and groups of citizens that do not vote." More recently, Walter Dean Burnham (1987, 99) emphasized again that "the old saw remains profoundly true: if you don't vote, you don't count." Voice and exit are often alternative ways of exerting influence (Hirschman 1970), but with regard to voting the exit option spells no influence; only voice can have an effect.

---

[5] The one serious doubt about the practical significance of these findings is that measures to increase turnout in the United States, such as easier registration and absentee voting rules, do not necessarily increase the proportion of the less privileged among the voters. For instance, being allowed to register as late as election day "rather than goading the disadvantaged to the polls, appears to simply provide a further convenience for those already inclined to vote by virtue of their social class position" (Calvert and Gilchrist 1993, 699; see also Oliver 1996; Wolfinger and Rosenstone 1980, 82–8). One has to keep in mind, however, that such measures result in relatively small turnout increments; more substantial increases in voting participation, in line with Tingsten's law of dispersion, are much more

In addition to the clear connection between socioeconomic status and turnout, there are two further important links. One is the clear nexus between socioeconomic status on the one hand and party choice and the outcome of elections on the other; in Lipset's (1960, 220) famous formulation, elections are "the expression of the democratic class struggle." The second crucial link is that between types of parties, especially progressive versus conservative parties, and the policies that these parties pursue when they are in power. There is an extensive comparative literature about welfare, redistribution, full employment, social security, and overall government spending policies that is unanimous in its conclusion that political parties do matter (Blais, Blake, and Dion 1996; Castles 1982; Castles and McKinlay 1979; King 1981; Klingemann, Hofferbert, and Budge 1994; Tufte 1978). Douglas A. Hibbs's (1977, 1467) conclusion represents the broad consensus very well: "Governments pursue . . . policies broadly in accordance with the objective economic interests and subjective preferences of their class-defined core political constituencies."

Skeptics have raised two critical questions about the strength of the above links. One has to do with the supposed decline in class voting. Even Lipset (1960, 220) who originally proclaimed that "on a world scale, the principal generalization which can be made is that parties are primarily based on either the lower classes or the middle and upper classes," retreated from this conclusion in the updated version of *Political Man* (Lipset 1981, 503): on the basis of American, British, German, and Swedish data, he concluded that his original generalization "has become less valid" (see also Dogan 1995, Franklin 1992). Other analysts have argued, however, that class voting is changing—especially from a dichotomous working versus middle-class contrast to more complex and multifaceted class differences—instead of declining (Andersen 1984; Hout, Brooks, and Manza 1995; Manza, Hout, and Brooks 1995). These authors also emphasize, and the supporters of the thesis of the decline in class voting admit, that this decline does not mean that class voting has vanished. This is also the conclusion of a study of class voting in 20 democracies from 1945 to 1990 by Paul Nieuwbeerta (1995, esp. 46–51). He finds a "substantial decline" in class voting in many countries, but the decline is strong enough to be statistically significant in only about half of his countries. In about a third of the countries he finds an opposite trend or no trend. Most important, in none of the countries has class voting disappeared altogether.

The second doubt about the nexus between social class, voting turnout, party choice, and public policy is raised by studies that show nonvoters not to be different from voters, especially in the United States, regarding policy preferences and candidate and party preferences. Ruy A. Teixeira (1992, 100) sums up the conclusions of a large number of studies in the following words: They "all tell a similar story: nonvoters are somewhat more liberal than voters on policy issues concerning the economic role of government . . . and all agree that the magnitude of these differences is not large and that therefore the absence of nonvoters from the voting pool probably has little immediate effect on the policy output of government" (see also Gant and Lyons 1993, Shaffer 1982, and, for a similar British study, Studlar and Welch 1986).[6] For election outcomes, the story is basically the same. For instance, if all nonvoters had voted in the 1980 presidential election, Reagan would have received only 2% fewer votes and would still have won the election; in 1984 and 1988, winners Reagan and Bush would actually have received a *higher* vote percentage (Bennett and Resnick 1990, 795; see also Petrocik 1987).

There are, however, several problems with Teixeira's (1992, 96–7) conclusion, based on the above studies, that "most electoral outcomes are not determined in any meaningful sense by turnout." Nonvoters who are asked their opinions on policy and partisan preferences in surveys are typically citizens who have not given these questions much thought, who have not been politically mobilized, and who, in terms of social class, have not developed class consciousness. It is highly likely that, if they were mobilized to vote, their votes would be quite different from their responses in opinion polls. The usual surveys, while "more representative than any of the modes of citizen activity" and hence "rigorously egalitarian" (Verba 1996, 3–4), fall short of discovering people's true opinions and preferences; only James S. Fishkin's (1991, 1995) "deliberative opinion polls" and Robert A. Dahl's (1989, 340; 1970, 149–50) randomly selected "minipopulus" of about one thousand citizens, who would meet and deliberate for an extended period of time, combine representativeness with well-formed policy and political preferences.[7]

Furthermore, the few studies that attempt the diffi-

---

likely to reduce class bias. Moreover, Teixeira (1992, 112–5) presents data that directly contradict Calvert and Gilchrist's conclusion.

[6] It is worth noting, however, that the usual finding is that there are only *small* differences instead of *no* differences, and that these small differences usually indicate, as expected, that less privileged citizens have more leftist opinions.

[7] Teixeira (1992, 102) appears to agree at least in part with this interpretation when he argues that, in the long run, low voter turnout "may contribute to the problem of an unrepresentative policy agenda, because nonvoters and voters do tend to differ systematically from one another in attributes that reflect individual *needs and interests*, even if their specific policy preferences within a given agenda generally do not" (emphasis added).

cult task of directly testing the link between voter turnout, on the one hand, and tax and welfare policies, on the other, all find compelling evidence that unequal voting participation is associated with policies that favor privileged voters over underprivileged nonvoters (Hicks and Swank 1992; Hill and Leighley 1992; Leighley 1995, 195–6; Mebane 1994). Finally, perhaps the most persuasive evidence is the strong and direct link between turnout and support for left-of-center parties found by Alexander Pacek and Benjamin Radcliff (1995). They analyzed all national elections in 19 industrial democracies from 1950 to 1990 and found that, as hypothesized, the vote for left parties varied directly with turnout: The left share of the total vote increases by almost one-third of a percentage point for every percentage point increase in turnout.[8] In short, the overall weight of the evidence strongly supports the view that who votes and how people vote matter a great deal. Indeed, any other conclusion would be extremely damaging for the very concept of representative democracy.

LOW AND DECLINING VOTER TURNOUT

Additional reasons for serious worry are the low levels of electoral participation in almost all democracies—even in national elections but especially in lower-level elections—and the downward trend in turnout in most countries. That the United States ranks near the bottom of voting participation in comparative perspective is well-known, and this high degree of nonvoting is often contrasted with "nonvoting levels as low as 5 percent in other democracies" (Teixeira 1992, 21). Voter turnout, however, tends to be lower in other countries than is commonly recognized. Powell's (1980, 6–8) turnout figures for 30 democracies in the 1960s and 1970s—all of the democratic countries with populations over one million during this period—show that not a single country had a turnout rate as high as 95%. The highest percentage is that of Italy, a country with compulsory voting—94%; the lowest percentage is that of Switzerland—53%. And the median turnout rate is only 76%.

The main reason for the exaggeration of voter turnout in other democracies is that their turnout rates are usually calculated as percentages of registered voters rather than percentage of voting-age population. For the United States, the latter figure is almost always used since the former would be extremely misleading, given the large numbers of eligible voters who are not registered. For most other democracies, which have automatic registration or where it is the government's responsibility to register voters, turnout percentages based on registered voters are more nearly correct—but far from completely accurate: Voter registers everywhere may fail to include all eligible voters or may include names of voters who have moved or died. Therefore, the only proper turnout percentages both in absolute terms and for comparative purposes are those based on voting-age populations.[9] Powell's percentages, cited above, are the optimally accurate figures based on voting-age population. The median of only 76% that he reports means that in half of the countries—including most of the most populous countries such as India, Japan, Britain, France, and, of course, the United States—fewer than about three out of every four citizens turn out to vote.[10]

All of the unimpressive turnout figures that I have mentioned so far are still deceptively favorable because they are the turnout percentages in the most salient national elections and hence the elections with the highest turnout: national parliamentary elections in parliamentary systems and presidential elections in presidential and semipresidential systems. The vast majority of elections, however, are elections with lower salience—local, state, provincial, and off-year congressional elections, as well as the elections to the European Parliament—which are characterized by considerably lower turnout. The U.S. off-year election turnout has only been around 35%, and turnout in local elections only about 25% in recent years (Ansolabehere and Iyengar 1995, 145–6; Teixeira 1992, 7). When lower-level elections are on the same ballot as presidential elections, voting participation improves, but there also tends to be considerable roll-off, that is, voters casting their votes for president but not for less prestigious offices. Moreover, as turnout decreases, roll-off tends to increase (Burnham 1965, 13–4), and roll-off, like nonvoting, is inversely correlated with socioeconomic status (Darcy and Schneider 1989, 360–2).[11]

In other democracies, too, lower-level elections attract fewer voters than national elections. In his classic *Why Europe Votes*, Gosnell (1930, 142–76) devoted an entire chapter to local elections in European countries and found that, in the 1920s, Europeans were more faithful voters than Americans but considerably less so in lower-level than in national elections. Average turnout rates in local elections in France and Spain, in German state elections, and in elections to the parliament of autonomous Catalonia in the 1980s and 1990s have been between 60 and 70%, but these averages conceal much lower turnouts in particular states and cities such as the 54.8% turnout in the German state of Sachsen-Anhalt in 1994 and the 45.6% turnout in the French city of Saint-Martin-d'Hères in 1983 (Botella 1994, Font and Virós 1995, Hoffmann-Martinot 1994, López Nieto 1994, Schultze 1995). Average turnout rates in the English-speaking democracies tend to be much lower still: 53% in New Zealand; 40% in Great Britain, but well below 40% in the major urban areas; 33% in Canada; and about 35% in Australia, where at the local level there is no compulsory voting (Denver 1995; Goldsmith and Newton 1986, 145–7; Miller 1994; Rallings and Thrasher 1990). In the 1994 European Parliament elections, the average turnout in the 12 member countries was 58.3% but in three countries only slightly more than a third of the registered voters participated: 36.4% in the United Kingdom and 35.6% in the Netherlands and Portugal (Smith 1995, 210). Turnout in the first European Parliament election in newly admitted Sweden in 1995 was a mere 41.6% (Widfeldt 1996).

---

[8] In a more controversial analysis, challenged by Erikson (1995), Radcliff (1994, 1995) found a strikingly similar pattern in the United States on the basis of state-level data from 1928 to 1980. Another similar finding is that, in New Zealand between 1928 and 1988, Labour's share of the vote increased by about a third of a percentage point for every percentage point increase in turnout (Nagel 1988, 25–9). In the United Kingdom, high turnout has meant a consistent disadvantage for the Conservatives, a modest gain for the Liberals, and no appreciable advantage for Labour—but, of course, a relative advantage for Labour as a result of the Conservatives' disadvantage (McAllister and Mughan 1986).

[9] Nevertheless, in the remainder of this paper, I shall often have to cite turnout figures based on registered voters because these may be the only figures that are available. It should also be noted that percentages based on voting-age population may still contain two types of inaccuracy. One is that the voting-age population includes noncitizens, which means that turnout rates in countries with relatively large numbers of resident aliens such as the United States, Switzerland, France, Germany, and Belgium are understated (Powell 1986, 40; Teixeira 1992, 9–10). The other is that, in most countries but not in the United States, the "voters" that are counted include those who cast blank and invalid ballots (Crewe 1981, 238; Wolfinger, Glass, and Squire 1990, 570). However, these inaccuracies are not likely to affect turnout figures by more than a couple of percentage points.

[10] Mark N. Franklin (1996, 218) reports turnout figures for 37 countries in the 1960–95 period with a much higher median—83%—but these use registered voters as the denominator.

[11] One recent example is the 1990 election in Oklahoma in which 39.5% of the voting-age population voted for governor, but only 38.3% and 37.1% in the U.S. senatorial and congressional races, and an average of 31.6% in the judicial retention choices—roll-offs of 2.9%, and 20%, respectively (calculated from data in Darcy and Vanderleeuw 1993, 3–4). Gosnell (1930, 209–10) reports that in the 1920 election in Kansas "35% of those who voted for president did not vote for state printer."

All of these elections have been called "second-order elections" in which less is at stake than who will control national executive power (Reif and Schmitt 1980). But while second-order elections may be less important elections, they are not entirely unimportant, even in unitary and centralized systems of government. In decentralized and federal systems such as the United States and Germany, state elections are obviously of great importance and, similarly, congressional elections should rank close to presidential ones in democracies in which the executive and legislature are coequal branches of the government. From the perspective of rational choice, it is to be expected that carefully reasoning voters will vote less in most second-order than in first-order elections, but the magnitude of the difference between the two is more difficult to explain (Feeley 1974, 241). In any case, when considering the general problem of low voter turnout, second-order elections with their often striking lower voter participation cannot be ignored.

Finally, voter turnout is not only low but also declining in most countries. In the United States, participation in presidential elections has declined from 60–65% in the 1950s and 1960s to 50–55% in the 1980s and 1990s; in Teixeira's (1992, 6) words, "a *low* turnout society . . . has been turned into an *even lower* turnout society." In other industrialized democracies, the decline is also unmistakable although not as dramatic. Average turnout in 20 of these countries declined from 83% in the 1950s to 78% in the 1990s, with 17 countries showing a lower and only 3 a higher turnout in the latter period (Dalton 1996, 44–5). For 18 industrialized democracies in the shorter time span from the 1960s to the 1980s—but based on more accurate turnout rates as percentages of voting-age population—average turnout went down from 80 to 78%, with ten countries showing lower, four higher, and four about the same turnout in the most recent period (Jackman 1987, 420; Jackman and Miller 1995, 485). For the European democracies, the *Beliefs in Government* study reports "a decline in average participation levels over the postwar period as a whole" (Borg 1995, 441) and a drop from 85% in 1960–64 to 80% in 1985–89 (Topf 1995, 40–1; see also Flickinger and Studlar 1992).[12] In Switzerland, the European country with a long record of poor voter participation, the 42.3% turnout in 1995 was a new all-time low in legislative elections (Farago 1996, 11).

The pattern is similar for second-order elections. Rainer-Olaf Schultze (1995, 91–4) reports declining turnout in Germany, especially since the mid-1980s, at all four levels: local, state, national, and European Parliament elections. For all of the member countries, average turnout in the elections to the European Parliament has gone down steadily from 65.9% in the first elections held in 1979 to 63.8%, 62.8%, and 58.3% in the next three elections (Smith 1995, 210).[13]

These drops in turnout are not as drastic as in the United States, but they are especially disturbing because they have occurred in spite of dramatic increases in levels of education and economic well-being and the rise of postmaterialist values (Inglehart 1990) in all industrialized countries—factors that, at the individual level, are known to increase rather than decrease the probability of voting. Moreover, the decline in turnout has been accompanied by a "participatory revolution" in Western Europe with regard to more intensive forms of political participation in which class bias is very strong; hence, as Max Kaase (1996, 36) points out, serious concerns about political equality arise because of the skewed nature of the "active partial publics."

Frances Fox Piven and Richard A. Cloward (1988b, 869) have argued that, in the United States, "restrictive registration procedures are the functional equivalents of earlier property and literacy qualifications." Similarly, it can be argued that the logical and empirical link between low voter turnout and unequal turnout is the functional equivalent of such discriminatory qualifications—as well as the functional equivalent of two earlier proposals and practices that systematically give well-to-do and educated citizens greater voting rights than their less privileged co-citizens. One is Aristotle's suggestion that "equal blocks of property carry equal weights, though the number of persons in each block is different" (Barker 1958, 262); a version of this was Prussia's three-class system from 1849 to 1918 which entailed having each of the three classes elect one-third of the deputies, but the top class consisted of only 4% of the voters, the middle class 16%, and the bottom class 80% (Urwin 1974, 116). The other is Mill's ([1861] 1958, 138) proposal of plural voting: "two or more votes might be allowed" on the basis of occupational status and educational qualifications. Such a system, with a maximum of three votes per voter, operated in Belgium from 1893 to 1919 (Gosnell, 1930, 98–9).

---

[12] Richard Topf (1995, 40), however, belittles this decline by comparing the most recent 80% turnout, not with the high of 85%, but with the overall mean of 83% in the postwar period, and by arguing that "a decline of some 3 percentage points is a very small change indeed." My interpretation of the findings of the *Beliefs in Government* project also obviously differs from that of its three coordinators who conclude that "voting turnout [in Western Europe] has remained remarkably stable in the postwar period" (Kaase, Newton, and Scarbrough 1996, 226).

[13] The number of member countries increased from 9 in 1979 to 10 in 1984 and 12 in 1989 and 1994. It may therefore be more appropriate to examine the averages for the original 9 members only: 65.9% (1979), 62.3% (1984), 63.1% (1989), and 59.3% (1994). The slight boost in 1989 can be explained in terms of the concurrence of that year's election in Ireland with a national parliamentary election (van der Eijk, Franklin, and Marsh 1996, 154) that raised turnout by an estimated 20 percentage points—and which therefore raised the average turnout for the 9 countries by about 2 percentage points.

All of these discriminatory rules are now universally rejected as undemocratic. Why then do many democrats tolerate the systematic pattern of low and unequal turnout that is the functional equivalent of such rules?

INSTITUTIONAL REMEDIES

Voting participation depends on many factors, including the salience of the issues—note, for instance, the 93.5% turnout in Quebec's 1995 referendum on independence (Kennedy 1996) and the high turnouts in the final years of the Weimar Republic—the attractiveness of parties and candidates, and political culture and attitudes. When we look for remedies for nonvoting, however, institutional factors are especially important. For one thing, when we compare turnout variations among countries and across social characteristics of individuals, "the most striking message is that turnout varies much more from country to country than it does between different types of individuals" (Franklin 1996, 217–8), which suggests very strongly that in order to expand voting in a country with low turnout it is much more promising to improve the institutional context than to raise levels of education and political interest. For another, rules and institutions are, at least in principle, more amenable to manipulation than individual attitudes. Fortunately, we know a great deal about the effect of institutions on turnout, especially thanks to the impressive early studies by Harold F. Gosnell (1930) and Herbert Tingsten (1937) and the outstanding recent work of G. Bingham Powell (1986), Robert W. Jackman (1987), and Mark N. Franklin (1996).

In the United States, burdensome registration requirements have long been recognized as a major institutional deterrent to voting (Gosnell 1927, 1930, 203–5; Rosenstone and Hansen 1993, 230). Voting presents a problem of collective action that becomes more serious as the costs increase, and the costs of registration are often higher than the cost of voting itself (Wolfinger 1994, 81–3). Raymond E. Wolfinger and Steven J. Rosenstone (1980, 73, 88) found that turnout would increase by 9.1 percentage points if all states adopted completely liberalized registration rules, but they also argued that turnout could be raised substantially more by a European-style system in which registration is automatic or the government's responsibility. On the basis of his comparative analysis, Powell (1986, 36) concludes that automatic registration could boost turnout by up to 14 percentage points. Comparisons between nationwide turnout and turnout in the few states with either no registration requirement at all or same-day and same-place registration—that is, the possibility of registering at the polls on election day—show differences of about 15 percentage points (Abramson 1995, 916; Wolfinger, Glass, and Squire 1990, 564–5). Other estimates have been somewhat lower; for instance, Burnham's (1987, 108) is about 10%. After an extensive review of all of the evidence, Teixeira (1992, 122) concludes that the increase would be somewhere between 8 and 15 percentage points.

Fifteen percentage points appears to be the maximum benefit that thorough registration reform could achieve, and it would be only a partial remedy that would still leave the United States well below the median turnout of 76% in contemporary democracies. Also, it is unclear how much registration reform would contribute to turnout in off-year, state, local, and primary elections; even if the increase were as much as 15 percentage points in these elections, it would still leave turnout well below 50% in most. Registration reform is irrelevant for most other Western democracies where registration is not a big problem.

Another important institutional mechanism that affects turnout is the electoral system. Proportional representation (PR) tends to stimulate voter participation by giving the voters more choices and by eliminating the problem of wasted votes—votes cast for losing candidates or for candidates that win with big majorities—from which systems using single-member districts suffer; this makes it more attractive for individuals to cast their votes and for parties to mobilize voters even in areas of the country in which they are weak. This phenomenon was already highlighted by both Gosnell (1930, 201–3) and Tingsten (1937, 223–5). Recent comparative studies have estimated that the turnout boost from PR is somewhere between 9 and 12% (Blais and Carty 1990, 174; Burnham 1987, 106–7; Franklin 1996, 226; Lijphart 1994, 5–7; see also Amy 1993, 140–52).[14]

These estimates of PR's beneficial effect are all based on the most salient national elections. In contrast, in second-order elections using PR, the level of voter participation tends to be much less impressive. The European Parliament elections provide a striking example: Turnouts have been low even though 11 of the 12 member countries choose their representatives by PR. In the 1995 provincial elections, by PR, in the Netherlands, turnout was only 50%. A recent American example is the 1996 New York City school board election, one of the rare cases of PR in the United States: Turnout was a mere 5% (Steinberg 1996).

---

[14] The difference between PR and single-member-district systems is roughly the same as the variable that Powell (1986) and Jackman (1987) call "nationally competitive districts," with two exceptions. One is that the latter takes into consideration three categories of proportionality in PR systems, based on the number of representatives elected per district. The other concerns presidential elections: The direct presidential elections in France, in which each vote counts nationwide, are placed in the same category as the most proportional parliamentary elections, whereas the American electoral-college system of presidential elections is scored on a par with single-member-district systems. Jackman (1987) and Jackman and Miller (1995) also find that multipartism, which is strongly associated with PR, depresses turnout—thus undoing some of PR's beneficial influence—and that bicameralism lowers turnout as well.

The frequency of elections has a strongly negative influence on turnout. Boyd (1981, 1986, 1989) has convincingly demonstrated this effect for the United States, in which he estimates that, on average, voters are asked to come to the polls between two and three times each year—much more often than in all except one other democracy. The one country with even more frequent dates on which elections and referenda are conducted—about six or seven times per year—is Switzerland (Farago 1995, 121; Franklin 1996, 225, 234; Sidjanski 1983, 109). The United States and Switzerland are also the two Western democracies with by far the lowest levels of turnout. The most plausible explanation is voter fatigue (Jackman and Miller 1995, 482–3) or, in terms of rational choice, the fact that frequent elections increase the cost of voting. If frequent elections depress turnout in first-order elections, it is logical to expect that they hurt turnout in second-order elections even more. This may be the explanation for the wide gap in the United States between the first-order presidential elections, on the one hand, and the second-order—but in a system of separation and division of powers still very important—midterm congressional as well as state executive and legislative elections on the other.

Rational-choice theory also leads us to expect that concurrent elections will increase turnout since the benefit of voting now increases while the cost remains almost the same (Aldrich 1993, 261; Wolfinger 1994, 76–8). In particular, second-order elections should have better turnout when combined with first-order elections. The available evidence shows this hypothesis to be correct. The European Parliament elections in Portugal and Ireland held at the same time as national parliamentary elections, in 1987 and 1989, respectively, yielded turnouts more than 20% higher than the preceding and/or next separate European Parliament election in these countries (Niedermayer 1990, 47–8). The 1979 local elections in England and Wales were conducted simultaneously with House of Commons elections, and, as a result, "local election turnout soared up to parliamentary levels" (Miller 1994, 69). Combining first-order and second-order elections may even help the former to some extent: In the United States, the inclusion of a gubernatorial race can increase turnout in presidential elections by about 6 percentage points (Boyd 1989, 735–6).

In contrast, the daunting accumulation of very many elections and referendum questions on one long ballot—a phenomenon unique to the United States with its extremely large number of elective offices and primary elections (Crewe 1981, 225–32)—is generally regarded as a deterrent to turnout, although the benefits of voting would appear to keep increasing with increasing ballot length. Gosnell (1930, 186, 209) emphasizes "the old lesson of the need for a shorter ballot," and comments that European voters are "not given an impossible task to perform on election day. [They are] not presented with a huge . . . ballot as are the voters in many of the American states."

Minor measures to facilitate voting, such as the availability of mail ballots and the scheduling of elections on weekends instead of weekdays, can also be a small but distinct stimulus to turnout. On the basis of a multivariate analysis of turnout in 29 countries, Franklin (1996, 226–30) finds that, other factors being equal, weekend voting increases turnout by 5 to 6 percentage points and that mail ballots are worth another 4% in first-order elections. In the second-order European Parliament elections, weekend voting adds more than 9 percentage points to turnout.

COMPULSORY VOTING

The strongest of all the institutional factors is compulsory voting, particularly with regard to second-order elections; but let us first take a look at the most salient national elections. Gosnell (1930, 184) took special pains to examine two of the European cases of compulsory voting, and his conclusion was: "There is no doubt that compulsory voting has had a sustained stimulating effect upon voting in Belgium and in the Swiss cantons where it is used. In Belgium it has maintained the highest voting records found in Europe." Tingsten (1937, 205) gathered evidence from several additional countries—Austria, Bulgaria, Czechoslovakia, the Netherlands, Romania, and Australia—and, like Gosnell, he concluded "that popular participation in elections is very high in countries with compulsory voting, that the introduction of compulsory voting everywhere has been accompanied by a remarkable rise in participation, and that in countries where compulsory voting has been enacted in certain regions, these display more intense participation than the regions without compulsory voting."

In comparative multivariate analyses, compulsory voting has been found to raise turnout by 7 to 16 percentage points. Powell (1980, 9–10) finds a difference of about 10% in his study of 30 democracies. The figures reported by Jackman (1987, 412, 415–6) and Jackman and Miller (1995, 474) for the industrialized democracies in three successive decades from 1960 to 1990 are 15.0, 13.1, and 12.2%. Franklin's (1996, 227) finding of a 7.3 percentage point difference is the lowest that has been reported. In a study of Latin American turnout in the 1980s and early 1990s, replicating Jackman's analysis, Carolina A. Fornos (1996, 34–5) finds that compulsory voting boosted turnout by 11.4 percentage points in presidential elections and 16.5 percentage points in congressional elections.[15]

---

[15] Enrique C. Ochoa (1987, 867) also notes that the Latin American countries with compulsory-voting laws "tend to have a higher participation rate. The countries with the highest voter turnout during the

The most persuasive results are in Wolfgang Hirczy's (1994) systematic study of within-country differences—both variations over time and variations among different areas in the same country—in Australia, Austria, and the Netherlands. He concludes, in line with previous findings, that compulsory voting effectively and consistently raises turnout. His more striking conclusion, however, is that the increase in turnout depends a great deal on the baseline of participation *without* compulsory voting. Mean turnout in all three countries under mandatory voting was higher than 90%, but the increment due to mandatory voting in Austria was only about 3 percentage points, because turnout even under conditions of voluntary voting was well above 90%. In the Netherlands, the abolition of compulsory voting in 1970 caused a larger drop of about 10 percentage points to the average voluntary-voting baseline of around 84%. And in Australia, the mean turnout difference was even larger—more than 28%—because the average turnout under voluntary voting before 1925 was only about 62%.

Brazil and Venezuela are additional examples of low baselines and hence high turnout boosts due to compulsory voting. Average official turnout in Venezuela from 1958 to 1988 was 90.2% but, after the abolition of mandatory voting in 1993, turnout fell to 60.2% (Molina Vega 1995, 164).[16] A public opinion poll in Brazil in 1990 found that, under hypothetical conditions of voluntary voting, turnout would undergo a similar drop of about 30 percentage points from the 85% turnout in that year's election to 55% (Power and Roberts 1995, 796, 819). These examples lend further support to Hirczy's (1994, 74) observation that "the impact of mandatory voting laws should be particularly pronounced in low-turnout environments."

Hirczy's conclusion also has special significance for second-order elections because these tend to be elections with low turnout. Here, indeed, compulsory voting is strikingly effective. Franklin's (1996, 227, 230) finding of a modest 7.3% boost from compulsory voting in national elections, mentioned above, contrasts with a 26.1% increase in turnout in a similar multivariate analysis of the 1989 European Parliament elections. In all four of the European Parliament elections from 1979 to 1994, the mean turnout was 84.2% in the countries with compulsory voting but only 46.4% in those with voluntary voting—a difference of almost 38 percentage points (based on data in Smith 1995, 210).

Gosnell (1930, 155) was greatly impressed with the level of turnout in provincial and local elections in Belgium in the 1920s, which was practically the same, well above 90%, as in the national elections: "The device of compulsory voting in Belgium overcame that indifference toward local elections which is so marked in countries with a free voting system." The same pattern can still be seen today: Belgian local elections from 1976 to 1994 had an average turnout of 93.7%—almost identical with the average 93.8% turnout in parliamentary elections during this period (based on data in Ackaert and De Winter 1996). In Italy from 1968 to 1994, mean turnout in local elections was 84.4% compared with 86.2% in national parliamentary elections—a difference of less than 2 percentage points (Corbetta and Parisi 1995, 171). In Dutch provincial and municipal elections from 1946 until the abandonment of mandatory voting in 1970, turnout was almost always well above 90%, often close to 95%, and usually only a bit lower than that in parliamentary elections. In 1970, turnout dropped to 68.1% in provincial and 67.2% in municipal elections. After a brief improvement in turnout levels later in the 1970s, they declined even further. The 1994 and 1995 figures are 65.3% in municipal, 50% in provincial, and 35.6% in European elections.[17]

Students of compulsory voting have not only been impressed but also often surprised by the strong effect of the obligation to vote, especially in view of the generally low penalties for noncompliance and generally lax enforcement: "Even when the penalties for non-voting are very small, and where law and practice prescribe very wide acceptance of excuses, the growth of the poll has been perceptible" (Tingsten 1937, 205–6). In rational-choice terms, however, this phenomenon can be explained easily. Turnout is a problem of collective action, but an unusual one, because turnout entails both low costs and low benefits (Aldrich 1993); this means that the inducement of compulsory voting, small as it is, can still neutralize a large part of the cost of voting.[18]

Rational-choice theory also provides the basic normative justification for compulsory voting. The general remedy for problems of collective action is to counteract free riding by means of legal sanctions and enforcement. For the collective-action problem of turnout, this means that citizens should not be allowed to be free riders—that is, that they should be obligated to turn out to vote (Feeley 1974, Wertheimer 1975).

Compulsory voting is not the only method for assuring high voter turnout. If all the other institutional variables are favorable—automatic registration, a highly proportional electoral system, infrequent elec-

---

most recent presidential elections in the 1980s . . . all have mandatory voting laws."

[16] Molina Vega's (1995, 163) own, more realistic, estimates of turnout are a bit lower—a mean of 82.8% before and 54% after the abolition of compulsory voting—but the difference of almost 29 percentage points between the two is roughly similar to that between the before and after official percentages. While the obligation to vote remained formally in force in 1993, compulsory voting was effectively eliminated because all penalties for nonvoting were removed.

[17] I am indebted to Galen A. Irwin for providing me with these data (personal correspondence, May 5, 1996). See also Andeweg and Irwin 1993, 83–5; Denters 1995, 118–21, 137; and Irwin 1974.

[18] Some compulsory-voting laws do prescribe heavy penalties, such as up to a year's imprisonment in Greece, but this kind of sanction is never imposed. The typical penalty is a relatively small fine, similar to a fine for a parking violation, but even these are imposed on only a small fraction of the nonvoters: 4 to 5% in Australia, less than 1% in the Netherlands when it had compulsory voting, and less than one-fourth of a percent in Belgium (Adviescommissie Opkomstplicht 1967, 28; Hasen 1996, 2169–70; Mackerras and McAllister 1996). In Italy, the only penalty is the "innocuous sanction"—but still effective sanction—of noting "did not vote" on the citizen's certificate of good conduct (Corbetta and Parisi 1995, 150; but see also Lombardo 1996).

tions, and weekend voting—and in a highly politicized environment, it may be possible to have near-universal turnout without compulsory voting, as Hirczy (1995) has shown for the case of Malta. Second-order elections can have high turnout if they are conducted concurrently with first-order elections in which all the major institutional mechanisms are conducive to turnout. Compulsory voting is the only institutional mechanism, however, that can assure high turnout virtually by itself.

VOTING AS A DUTY: PROS AND CONS

The most important argument in favor of compulsory voting is its contribution to high and relatively equal voter turnout. Three additional, more speculative, advantages of compulsory voting, however, are worth mentioning. One is that the increase in voting participation may stimulate stronger participation and interest in other political activities: "People who participate in politics in one way are likely to do so in another" (Berelson and Steiner 1964, 422). Considerable evidence exists of a spillover effect from participation in the workplace, churches, and voluntary organizations to political participation (Almond and Verba 1963, 300–74; Greenberg 1986; Lafferty 1989; Peterson 1992; Sobel 1993; Verba, Schlozman, and Brady 1995, 304–68; but see also Greenberg, Grunberg, and Daniel 1996; Schweizer 1995).

Second, compulsory voting may have the beneficial effect of reducing the role of money in politics. When almost everybody votes, no large campaign funds are needed to goad voters to the polls, and, in Gosnell's (1930, 185) words, "elections are therefore less costly, more honest, and more representative." Third, mandatory voting may discourage attack advertising—and hence may lessen the cynicism and distrust that it engenders. Stephen Ansolabehere and Shanto Iyengar (1995) have found that attack ads work mainly by selectively depressing turnout among those not likely to vote for the attacker. When almost everybody votes, attack tactics lose most of their lure.[19]

Having emphasized the advantages of compulsory voting so far, I must also deal with the most important arguments that have been raised against it. One criticism has been that the compulsory vote forces to the polls people who have little political interest and knowledge and who are unlikely to cast a well-considered vote: "An unwilling or indifferent vote is a thoughtless one" (Abraham 1955, 21). What this objection overlooks is that mandatory voting may serve as an incentive to become better informed. An indirect bit of evidence supporting this possibility is that, in American and European election studies, respondents interviewed prior to elections were found to vote in considerably greater numbers than expected due to the stimulation of these interviews (Popkin 1991, 235; Smeets 1995, 311–2). Warren E. Miller's comment on this phenomenon is that such interviews are "the most expensive form of adult civic education known to mankind"![20] Compulsory voting may be able to serve as an equivalent, but much less expensive, form of civic education and political stimulation. This was an important objective when compulsory voting was introduced in both the Netherlands in 1917 and in Australia in 1924; at that time, one of its Australian proponents argued, in a highly optimistic vein, that "by compelling people to vote we are likely to arouse in them an intelligent interest and to give them a political knowledge that they do not at present possess" (cited in Morris Jones 1954, 32; see also Verplanke 1965, 81–3). Moreover, under compulsory voting, parties and candidates have a strong incentive to pay more attention and work harder to get information to previous nonvoters.

Another criticism, based especially on the experience of the last years of the Weimar Republic in which increasing turnout coincided with the growth of the Nazi vote, is that high turnout may be undesirable and even dangerous. Tingsten (1937, 225; see also Lipset 1960, 140–52, 218–9) already used the Weimar example to warn that "exceptionally high voting frequency may indicate an intensification" of political conflict that may foreshadow the fall of democracy. The danger is that, in periods of crisis, sudden jumps in turnout mean that many previously uninterested and uninvolved citizens will come to the polls and will support extremist parties. This, however, is an argument for, not against, compulsory voting: Instead of trying to keep turnout at steady *low* levels, it is better to safeguard against the danger of sudden sharp increases by maintaining steady *high* levels, unaffected by crises and charismatic leaders. Additional evidence that the Weimar precedent should not discourage efforts to increase turnout is Powell's (1982, 206) comparative study of 29 democracies in which he found a strong association between higher voter turnout and less citizen turmoil and violence: "The data favor the theorists who believe that citizen involvement enhances legitimacy" instead of producing democratic breakdown.[21]

Compulsory voting has also been disparaged, even by those who support it in principle, on the practical grounds that the possibility of it being adopted in democracies that do not already have it are very small, that one especially big obstacle to its adoption is the opposition of conservative parties, and that, particularly in the United States—where arguably it is needed more than in most democracies given its low voter turnout at all levels—its chances of being accepted are nil. Alan Wertheimer (1975, 293) argues that mandatory voting is "a good idea whose time is either past or has not yet come. It is certainly not a good idea whose time is at hand." And Richard L. Hasen (1996, 2173) favors compulsory voting in American federal elections but concludes that it "has virtually no chance of enactment in the United States."

---

[19] For countries with proportional representation, a fourth argument in favor of compulsory voting is that it is illogical to want votes to be converted proportionally into seats, but to be satisfied with a situation in which only a biased sample of the eligible electorate actually votes—which necessarily introduces considerable disproportionality after all. This was an important part of the reasoning behind the simultaneous adoption of compulsory voting and proportional representation in the Netherlands in 1917 (Andeweg and Irwin 1993, 81, 84; Daalder 1975, 228).

[20] Personal correspondence, July 2, 1995. The expense of this kind of civic education is, of course, not just the cost of conducting the interviews but also the fact that it is unnecessary for those who will vote anyway and far from 100% effective for those less likely to vote.

[21] Because Powell's conclusion is based on a number of presidential as well as parliamentary systems, his finding also assuages, at least partly, Fred W. Riggs's (1988, 263–4) fear that high turnout is a special danger in presidential regimes; Riggs regards presidentialism as inherently weak and unstable—and capable of survival only when conservative forces have predominant power.

The very fact, however, that so many democracies do have compulsory voting, and have had it for a long time, shows that, while it may be difficult, it is clearly not an impossible task to introduce it. It is also worth noting that, in compulsory-voting countries, there is no strong trend in favor of abandoning it; the Netherlands and Venezuela are the only major examples of countries that abolished compulsory voting in recent decades. It will indeed not be easy to overcome the opposition of conservative parties in whose self-interest it is to keep turnout as low and class-biased as possible. Universal suffrage was also initially opposed by most of these parties—but eventually accepted. Like universal suffrage, mandatory voting is a moral issue not just a political and partisan one. Indeed, compulsory voting can be regarded as a natural extension of universal suffrage.

A special impediment to mandatory voting in the United States is that it may be unconstitutional. Henry J. Abraham (1955, 31) takes this position and, in support of it, cites an 1896 opinion by the Supreme Court of Missouri that "voting is not such a duty as may be enforced by compulsory legislation, that it is distinctly not within the power of any legislative authority . . . to compel the citizen to exercise it." However, Hasen (1996, 2176) strongly disagrees. He argues that the only plausible constitutional objection to compulsory voting would be on the First Amendment ground of a violation of freedom of speech and that the U.S. Supreme Court has explicitly rejected the argument that the vote may be regarded as a form of speech; moreover, he points out that the Missouri Supreme Court's 1896 decision failed to mention any particular constitutional violations. And, of course, even the courts' possible finding of unconstitutionality would not be a permanent and unsurmountable obstacle; as Gosnell (1930, 207) observes, "if the courts should interfere with the adoption of a system of compulsory voting, then the state and federal constitutions could be amended."[22] It is not entirely without precedent in the United States either: In the eighteenth century, Georgia and Virginia experimented with mandatory voting laws (Hasen 1996, 2173–4), and constitutional provisions adopted in North Dakota in 1898 and in Massachusetts in 1918 authorized their state legislatures to institute compulsory voting—but no legislative action was taken (Gosnell 1930, 206–7).

The danger of too much pessimism about the chances for compulsory voting is that it becomes a self-fulfilling prophecy. If even the supporters of compulsory voting believe that its chances are nil—and hence make no effort on behalf of it—it will indeed never be adopted!

Probably the most serious objection to compulsory voting is normative in nature: compulsory voting may be an attractive partial solution to the conflict between the democratic ideals of participation and equality, but it is often said to violate a third democratic ideal, that of individual freedom. For this reason, Abraham (1955, 33) calls compulsory voting "undemocratic," and W. H. Morris Jones (1954, 25) argues that it belongs "to the totalitarian camp and [is] out of place in the vocabulary of liberal democracy."

That compulsion of any kind limits individual freedom cannot be denied, but the duty to vote entails only a very minor restriction. It is important to remember, first of all, that compulsory "voting" does not mean an actual duty to cast a valid ballot; all that needs to be required is for citizens to show up at the polls. At that point, citizens may choose to refuse to vote; the right *not* to vote remains intact.[23] Moreover, compulsory voting entails a very small decrease in freedom compared with many other problems of collective action that democracies solve by imposing obligations: jury duty, the obligation to pay taxes, military conscription, compulsory school attendance, and many others. These obligations are much more burdensome than the duty to appear at the polls on election days. It must also be remembered that nonvoting is a form of free riding—and that free riding of any kind may be rational but is also selfish and immoral. The normative objection to compulsory voting has an immediate intuitive appeal that is not persuasive when considered more carefully.[24]

Compulsory voting cannot solve the entire conflict between the ideals of participation and equality, but by making voting participation as equal as possible, it is a valuable partial solution. In the first sentence of *Why Europe Votes*, Gosnell (1930, vii) states that the "struggle for democracy has just begun with the broadening of the franchise." After universal suffrage, the next aim for democracy must be universal or near-universal *use* of the right to vote.

---

[22] However, Gosnell (1930, 192–212) was certainly not at all optimistic about the chances for mandatory voting in the United States. He begins the last chapter of *Why Europe Votes* with the question: "What use can be made of European political experience in America?" He discusses the advantages of compulsory voting at great length but quietly drops it from his final list of recommendations, which does include relatively radical proposals like proportional representation in elections to the U.S. House of Representatives, permanent voter registration that is the government's responsibility, and adoption of the short ballot.

[23] Malcolm M. Feeley (1974, 242) states that most of the objections to compulsory voting can be solved by including a "no preference" alternative—or, as others have suggested a "none of the above" choice—on the ballot. The right to refuse to accept a ballot, however, is an even more effective method to assure that the right not to vote is not infringed.

[24] A logical alternative to compulsory voting is to use rewards for voting instead of penalties for nonvoting: Citizens can be paid to vote. The only empirical example of this—obviously more expensive—arrangement appears to be ancient Athens (Hasen 1996, 2135, 2169; Staveley 1972, 78–82).

REFERENCES

Abraham, Henry J. 1955. *Compulsory Voting*. Washington, DC: Public Affairs Press.

Abramson, Paul R. 1995. "Political Participation." In *The Encyclopedia of Democracy*, ed. Seymour Martin Lipset. 4 vols. Washington, DC: Congressional Quarterly.

Ackaert, Johan, and Lieven De Winter. 1993. "De afwezigen hebben andermaal ongelijk: De stemverzaking in Vlaanderen op 24 november 1991." In *Kiezen is verliezen: Onderzoek naar de politieke opvattingen van Vlamingen*, ed. Marc Swyngedouw, Jaak Billiet, Ann Carton, and Roeland Beerten. Leuven: Acco.

Ackaert, Johan, and Lieven De Winter. 1996. "Electoral Absenteeism and Potential Absenteeism in Belgium." Presented at the annual meeting of the American Political Science Association, San Francisco.

Adviescommissie Opkomstplicht. 1967. *Rapport van de adviescommissie opkomstplicht*. The Hague: Staatsuitgeverij.

Aldrich, John H. 1993. "Rational Choice and Turnout." *American Journal of Political Science* 37 (February):246–78.

Almond, Gabriel A., and Sidney Verba. 1963. *The Civic Culture: Political Attitudes and Democracy in Five Nations*. Princeton: Princeton University Press.

Amy, Douglas J. 1993. *Real Choices/New Voices: The Case for Proportional Representation Elections in the United States*. New York: Columbia University Press.

Andersen, Jörgen Goul. 1984. "Decline of Class Voting or Change in Class Voting? Social Classes and Party Choice in Denmark in the 1970s." *European Journal of Political Research* 12 (September): 243–59.

Andeweg, Rudy B., and Galen A. Irwin. 1993. *Dutch Government and Politics*. New York: St. Martin's Press.

Ansolabehere, Stephen, and Shanto Iyengar. 1995. *Going Negative: How Attack Ads Shrink and Polarize the Electorate*. New York: Free Press.

Arendt, Hannah. 1958. *The Human Condition*. Chicago: University of Chicago Press.

Arneson, Ben A. 1925. "Non-Voting in a Typical Ohio Community." *American Political Science Review* 19 (November):816–25.

Baloyra, Enrique A, and John D. Martz. 1979. *Political Attitudes in Venezuela: Societal Cleavages and Political Opinion*. Austin: University of Texas Press.

Barber, Benjamin R. 1984. *Strong Democracy: Participatory Politics for a New Age*. Berkeley: University of California Press.

Barker, Ernest. 1958. *The Politics of Aristotle*. New York: Oxford University Press.

Bennett, Stephen Earl, and David Resnick. 1990. "The Implications of Nonvoting for Democracy in the United States." *American Journal of Political Science* 34 (August):771–802.

Berelson, Bernard, and Gary A. Steiner. 1964. *Human Behavior: An Inventory of Scientific Findings*. New York: Harcourt, Brace.

Blais, André, Donald Blake, and Stéphane Dion. 1996. "Do Parties Make a Difference? A Reappraisal." *American Journal of Political Science* 40 (May):514–20.

Blais, André, and R. K. Carty. 1990. "Does Proportional Representation Foster Voter Turnout?" *European Journal of Political Research* 18 (March):167–81.

Borg, Sami. 1995. "Electoral Participation." In *The Impact of Values*, ed. Jan W. van Deth and Elinor Scarbrough. Vol. 4 of *Beliefs in Government*. Oxford: Oxford University Press.

Botella, Joan. 1994. "Local Government in Catalonia: The Making of a Local Elite, 1979–1991." In *Local Elections in Europe*, ed. Lourdes López Nieto. Barcelona: ICPS.

Boyd, Richard W. 1981. "Decline of U.S. Voter Turnout: Structural Explanations." *American Politics Quarterly* 9 (April):133–59.

Boyd, Richard W. 1986. "Election Calendars and Voter Turnout." *American Politics Quarterly* 14 (January-April):89–104.

Boyd, Richard W. 1989. "The Effects of Primaries and Statewide Races on Voter Turnout." *Journal of Politics* 51 (August):730–39.

Burnham, Walter Dean. 1965. "The Changing Shape of the American Political Universe." *American Political Science Review* 59 (March):7–28.

Burnham, Walter Dean. 1980. "The Appearance and Disappearance of the American Voter." In *Electoral Participation: A Comparative Analysis*, ed. Richard Rose. Beverly Hills, CA: Sage.

Burnham, Walter Dean. 1987. "The Turnout Problem." In *Elections American Style*, ed. A. James Reichley. Washington, DC: Brookings Institution.

Calvert, Jerry W., and Jack Gilchrist. 1993. "Suppose They Held an Election and Almost Everybody Came!" *PS: Political Science & Politics* 26 (December):695–700.

Castles, Francis G., ed. 1982. *The Impact of Parties: Politics and Policies in Democratic Capitalist States*. London: Sage.

Castles, Frank, and Robert D. McKinlay. 1979. "Does Politics Matter: An Analysis of the Public Welfare Commitment in Advanced Democratic States." *European Journal of Political Research* 7 (June):169–86.

Corbetta, Piergiorgio, and Arturo M. L. Parisi. 1995. "Electoral Abstentionism in Italy." In *Electoral Abstention in Europe*, ed. Joan Font and Rosa Virós. Barcelona: ICPS.

Crewe, Ivor. 1981. "Electoral Participation." In *Democracy at the Polls: A Comparative Study of Competitive National Elections*, ed. David Butler, Howard R. Penniman, and Austin Ranney. Washington, DC: American Enterprise Institute.

Daalder, Hans. 1975. "Extreme Proportional Representation: The Dutch Experience." In *Adversary Politics and Electoral Reform*, ed. S. E. Finer. London: Anthony Wigram.

Dahl, Robert A. 1970. *After the Revolution? Authority in a Good Society*. New Haven, CT: Yale University Press.

Dahl, Robert A. 1989. *Democracy and Its Critics*. New Haven, CT: Yale University Press.

Dalton, Russell B. 1996. *Citizen Politics: Public Opinion and Political Parties in Advanced Industrial Democracies*. 2nd ed. Chatham, NJ: Chatham House.

Darcy, R., and Anne Schneider. 1989. "Confusing Ballots, Roll-Off, and the Black Vote." *Western Political Quarterly* 42 (September): 347–64.

Darcy, R., and James Vanderleeuw. 1993. *The Impact of Ballot Format on Voter Rolloff*. Stillwater: Department of Political Science, Oklahoma State University.

Denters, S. A. H. 1995. "Voter Turnout in Dutch Elections." In *Electoral Abstention in Europe*, ed. Joan Font and Rosa Virós. Barcelona: ICPS.

Denver, David. 1995. "Non-Voting in Britain." In *Electoral Abstention in Europe*, ed. Joan Font and Rosa Virós. Barcelona: ICPS.

De Winter, Lieven, and Johan Ackaert. 1994. "Abstentionnisme électoral et vote blanc ou nul: Le 'non-vote' en Wallonie." In *Élections la fêlure? Enquête sur le comportement électoral des Wallons et des Francophones*, ed. André-Paul Frognier and Anne-Marie Aish-Van Vaerenbergh. Brussels: De Boeck.

Dogan, Mattei. 1995. "Erosion of Class Voting and of the Religious Vote in Western Europe." *International Social Science Journal* 146 (December):525–38.

Erikson, Robert S. 1995. "State Turnout and Presidential Voting: A Closer Look." *American Politics Quarterly* 23 (October):387–96.

Farago, Peter. 1995. "Wahlforschung in der Schweiz: Der Neubeginn." *Swiss Political Science Review* 1 (Winter):121–30.

Farago, Peter. 1996. *Wahlen 95: Zusammensetzung und politische Orientierungen der Wählerschaft an den eidgenössischen Wahlen 1995*. Bern: Selects.

Feeley, Malcolm M. 1974. "A Solution to the 'Voting Dilemma' in Modern Democratic Theory." *Ethics* 84 (April):235–42.

Fernández Baeza, Mario. N.d. "El voto obligatorio en América Latina." In *Tratado de derecho electoral comparado de América Latina*, ed. Dieter Nohlen. San José, Costa Rica: Instituto Interamericano de Derechos Humanos. Forthcoming.

Fishkin, James S. 1991. *Democracy and Deliberation: New Directions for Democratic Reform*. New Haven, CT: Yale University Press.

Fishkin, James S. 1995. *The Voice of the People: Public Opinion and Democracy*. New Haven, CT: Yale University Press.

Flickinger, Richard S., and Donley T. Studlar. 1992. "The Disappearing Voters? Exploring Declining Turnout in Western European Elections." *West European Politics* 15 (April):1–16.

Fornos, Carolina A. 1996. "Explaining Voter Turnout in Latin America." Master's thesis, Louisiana State University.

Font, Joan, and Rosa Virós. 1995. "Catalan Electoral Abstention: A Critical Review." In *Electoral Abstention in Europe*, ed. Joan Font and Rosa Virós. Barcelona: ICPS.

Franklin, Mark N. 1992. "The Decline of Cleavage Politics." In *Electoral Change: Responses to Evolving Social and Attitudinal*

Structures in Western Countries, ed. Mark N. Franklin, Thomas T. Mackie, and Henry Valen. Cambridge: Cambridge University Press.

Franklin, Mark N. 1996. "Electoral Participation." In *Comparing Democracies: Elections and Voting in Global Perspective*, ed. Laurence LeDuc, Richard G. Niemi, and Pippa Norris. Thousand Oaks, CA: Sage.

Franklin, Mark N., Cees van der Eijk, and Erik Oppenhuis. 1996. "The Institutional Context: Turnout." In *Choosing Europe? The European Electorate and National Politics in the Face of Union*, ed. Cees van der Eijk and Mark N. Franklin. Ann Arbor: University of Michigan Press.

Gant, Michael M., and William Lyons. 1993. "Democratic Theory, Nonvoting, and Public Policy: The 1972–1988 Presidential Elections." *American Politics Quarterly* 21 (April):185–204.

Goldsmith, Michael, and Ken Newton. 1986. "Local Government Abroad." In *The Conduct of Local Authority Business*, ed. Committee of Inquiry into the Conduct of Local Authority Business. London: Her Majesty's Stationery Office.

Gosnell, Harold F. 1927. *Getting Out the Vote: An Experiment in the Stimulation of Voting*. Chicago: University of Chicago Press.

Gosnell, Harold F. 1930. *Why Europe Votes*. Chicago: University of Chicago Press.

Greenberg, Edward S. 1986. *Workplace Democracy: The Political Effects of Participation*. Ithaca, NY: Cornell University Press.

Greenberg, Edward S., Leon Grunberg, and Kelley Daniel. 1996. "Industrial Work and Political Participation: Beyond 'Simple Spillover.'" *Political Research Quarterly* 49 (June):305–30.

Hasen, Richard L. 1996. "Voting Without Law?" *University of Pennsylvania Law Review* 144 (May):2135–79.

Hibbs, Douglas A., Jr. 1977. "Political Parties and Macroeconomic Policy." *American Political Science Review* 71 (December):1467–87.

Hicks, Alexander M., and Duane H. Swank. 1992. "Politics, Institutions, and Welfare Spending in Industrialized Democracies, 1960–82." *American Political Science Review* 86 (September):658–74.

Hill, Kim Quaile, and Jan E. Leighley. 1992. "The Policy Consequences of Class Bias in State Electorates." *American Journal of Political Science* 36 (May):351–65.

Hirczy, Wolfgang. 1994. "The Impact of Mandatory Voting Laws on Turnout: A Quasi-Experimental Approach." *Electoral Studies* 13 (March):64–76.

Hirczy, Wolfgang. 1995. "Explaining Near-Universal Turnout: The Case of Malta." *European Journal of Political Research* 27 (February):255–72.

Hirschman, Albert O. 1970. *Exit, Voice, and Loyalty: Responses to Decline in Firms, Organizations, and States*. Cambridge: Harvard University Press.

Hoffmann-Martinot, Vincent. 1994. "Voter Turnout in French Municipal Elections." In *Local Elections in Europe*, ed. Lourdes López Nieto. Barcelona: ICPS.

Hout, Michael, Clem Brooks, and Jeff Manza. 1995. "The Democratic Class Struggle in the United States, 1948–1992." *American Sociological Review* 60 (December):805–28.

Inglehart, Ronald. 1990. *Culture Shift in Advanced Industrial Society*. Princeton: Princeton University Press.

Irwin, Galen A. 1974. "Compulsory Voting Legislation: Impact on Voter Turnout in the Netherlands." *Comparative Political Studies* 7 (October):292–315.

Jackman, Robert W. 1987. "Political Institutions and Voter Turnout in the Industrial Democracies." *American Political Science Review* 81 (June):405–23.

Jackman, Robert W., and Ross A. Miller. 1995. "Voter Turnout in the Industrial Democracies During the 1980s." *Comparative Political Studies* 27 (January):467–92.

Justel, Manuel. 1995. "Electoral Abstention in Spain: Characteristics and Factors." In *Electoral Abstention in Europe*, ed. Joan Font and Rosa Virós. Barcelona: ICPS.

Kaase, Max. 1996. "The Impact of Sociopolitical Change in Western Europe on Transatlantic Relations." In *Estranged Friends? The Transatlantic Consequences of Societal Change*, ed. Max Kaase and Andrew Kohut. New York: Council on Foreign Relations Press.

Kaase, Max, Kenneth Newton, and Elinor Scarbrough. 1996. "A Look at the *Beliefs in Government* Study." *PS: Political Science & Politics* 29 (June):226–28.

Kennedy, J. Ray. 1996. "Canadian Province Sees Four in October: Referendums in Quebec." *Elections Today* 5 (January):18–19.

Key, V. O., Jr. 1949. *Southern Politics in State and Nation*. New York: Vintage Books.

King, Anthony. 1981. "What Do Elections Decide?" In *Democracy at the Polls: A Comparative Study of Competitive National Elections*, ed. David Butler, Howard R. Penniman, and Austin Ranney. Washington, DC: American Enterprise Institute.

Klingemann, Hans-Dieter, Richard I. Hofferbert, and Ian Budge. 1994. *Parties, Policies, and Democracy*. Boulder, CO: Westview Press.

Lafferty, William M. 1989. "Work as a Source of Political Learning Among Wage-Laborers and Lower-Level Employees." In *Political Learning in Adulthood: A Sourcebook of Theory and Research*, ed. Roberta S. Sigel. Chicago: University of Chicago Press.

Leighley, Jan E. 1995. "Attitudes, Opportunities and Incentives: A Field Essay on Political Participation." *Political Research Quarterly* 48 (March):181–209.

Lijphart, Arend. 1994. "Democracies: Forms, Performance, and Constitutional Engineering." *European Journal of Political Research* 25 (January):1–17.

Linder, Wolf. 1994. *Swiss Democracy: Possible Solutions to Conflict in Multicultural Societies*. New York: St. Martin's Press.

Lipset, Seymour Martin. 1960. *Political Man: The Social Bases of Politics*. Garden City, NY: Doubleday.

Lipset, Seymour Martin. 1981. *Political Man: The Social Bases of Politics*. Expanded ed. Baltimore: Johns Hopkins University Press.

Lombardo, Salvatore. 1996. "Mandatory Voting and Voter Participation in Italy." Presented at the annual meeting of the American Political Science Association, San Francisco.

López Nieto, Lourdes. 1994. "Local Elections in the Spanish Political System: 1979–1991." In *Local Elections in Europe*, ed. Lourdes López Nieto. Barcelona: ICPS.

Mackerras, Malcolm, and Ian McAllister. 1996. "Compulsory Voting, Party Stability and Electoral Bias in Australia." Presented at the annual meeting of the American Political Science Association, San Francisco.

Manza, Jeff, Michael Hout, and Clem Brooks. 1995. "Class Voting in Capitalist Democracies Since World War II: Dealignment, Realignment, or Trendless Fluctuation?" In *Annual Review of Sociology*, vol. 21, ed. John Hagan and Karen S. Cook. Palo Alto, CA: Annual Reviews.

Marsh, Alan, and Max Kaase. 1979. "Background of Political Action." In *Political Action: Mass Participation in Five Western Democracies*, ed. Samuel H. Barnes and Max Kaase. Beverly Hills, CA: Sage.

McAllister, Ian. 1986. "Compulsory Voting, Turnout and Party Advantage in Australia." *Politics* 21 (May):89–93.

McAllister, Ian, and Anthony Mughan. 1986. "Differential Turnout and Party Advantage in British General Elections, 1964–83." *Electoral Studies* 5 (August):143–52.

Mebane, Walter R., Jr. 1994. "Fiscal Constraints and Electoral Manipulation in American Social Welfare." *American Political Science Review* 88 (March):77–94.

Mill, John Stuart. [1861] 1958. *Considerations on Representative Government*. New York: Liberal Arts Press.

Miller, William L. 1994. "Local Elections in Britain." In *Local Elections in Europe*, ed. Lourdes López Nieto. Barcelona: ICPS.

Molina Vega, José Enrique. 1991. *El sistema electoral venezolano y sus consecuencias políticas*. San José, Costa Rica: IIDH/CAPEL.

Molina Vega, José Enrique. 1995. "Los venezolanos abandonan el hábito de votar: La abstención en las elecciones de 1993." *Boletín Electoral Latinoamericano* 13 (January–June):159–79.

Morris Jones, W. H. 1954. "In Defense of Apathy: Some Doubts on the Duty to Vote." *Political Studies* 2 (1):25–37.

Mottier, Véronique. 1993. "La structuration sociale de la participation aux votations fédérales." In *Citoyenneté et démocratie directe: Compétence, participation et décision des citoyens et citoyennes suisses*, ed. Hanspeter Kriesi. Zurich: Seismo.

Nagel, Jack H. 1988. "Voter Turnout in New Zealand General Elections, 1928–1988." *Political Science* 40 (December):16–38.

Niedermayer, Oskar. 1990. "Turnout in the European Elections." *Electoral Studies* 9 (March):45–50.

Nieuwbeerta, Paul. 1995. *The Democratic Class Struggle in Twenty Countries, 1945–1990*. Amsterdam: Thesis Publishers.

Nohlen, Dieter, ed. 1993. *Enciclopedia electoral latinoamericana y del Caribe*. San José, Costa Rica: Instituto Interamericano de Derechos Humanos.

Ochoa, Enrique C. 1987. "The Rapid Expansion of Voter Participation in Latin America: Presidential Elections, 1845–1986. In vol. 25 of *Statistical Abstract of Latin America*, ed. James W. Wilkie and David Lorey. Los Angeles: UCLA Latin American Center Publications, University of California.

Oliver, J. Eric. 1996. "The Effects of Eligibility Restrictions and Party Activity on Absentee Voting and Overall Turnout." *American Journal of Political Science* 40 (May):498–513.

Oppenhuis, Erik Vincent. 1995. *Voting Behavior in Europe: A Comparative Analysis of Electoral Participation and Party Choice*. Amsterdam: Het Spinhuis.

Pacek, Alexander, and Benjamin Radcliff. 1995. "Turnout and the Vote for Left-of-Centre Parties: A Cross-National Analysis." *British Journal of Political Science* 25 (January):137–43.

Pateman, Carole. 1970. *Participation and Democratic Theory*. Cambridge: Cambridge University Press.

Peterson, Steven A. 1992. "Workplace Politicization and Its Political Spillovers: A Research Note." *Economic and Industrial Democracy* 13 (November):511–24.

Petrocik, John R. 1987. "Voter Turnout and Electoral Preference: The Anomalous Reagan Elections." In *Elections in America*, ed. Kay Lehman Schlozman. Boston: Allen and Unwin.

Piven, Frances Fox, and Richard A. Cloward. 1988a. *Why Americans Don't Vote*. New York: Pantheon Books.

Piven, Frances Fox, and Richard A. Cloward. 1988b. "National Voter Registration Reform: How It Might Be Won." *PS: Political Science & Politics* 21 (Fall):868–75.

Popkin, Samuel L. 1991. *The Reasoning Voter: Communication and Persuasion in Presidential Campaigns*. Chicago: University of Chicago Press.

Powell, G. Bingham, Jr. 1980. "Voting Turnout in Thirty Democracies: Partisan, Legal, and Socio-Economic Influences." In *Electoral Participation: A Comparative Analysis*, ed. Richard Rose. Beverly Hills, CA: Sage.

Powell, G. Bingham, Jr. 1982. *Contemporary Democracies: Participation, Stability, and Violence*. Cambridge: Harvard University Press.

Powell, G. Bingham, Jr. 1986. "American Voter Turnout in Comparative Perspective." *American Political Science Review* 80 (March):17–43.

Power, Timothy J., and J. Timmons Roberts. 1995. "Compulsory Voting, Invalid Ballots, and Abstention in Brazil." *Political Research Quarterly* 48 (December):795–826.

Puplick, Chris. 1995. "The Case for Compulsory Voting." *Elections Today* 5 (October):3–5.

Radcliff, Benjamin. 1994. "Turnout and the Democratic Vote." *American Politics Quarterly* 22 (July):259–76.

Radcliff, Benjamin. 1995. "Turnout and the Vote Revisited: A reply to Erikson." *American Politics Quarterly* 23 (October):397–403.

Rallings, Colin, and Michael Thrasher. 1990. "Turnout in English Local Elections: An Aggregate Analysis with Electoral and Contextual Data." *Electoral Studies* 9 (June):79–90.

Reif, Karlheinz, and Hermann Schmitt. 1980. "Nine Second-Order National Elections: A Conceptual Framework for the Analysis of European Election Results." *European Journal of Political Research* 8 (March):3–44.

Riggs, Fred W. 1988. "The Survival of Presidentialism in America: Para-Constitutional Practices." *International Political Science Review* 9 (October):247–78.

Rosenstone, Steven J., and John Mark Hansen. 1993. *Mobilization, Participation, and Democracy in America*. New York: Macmillan.

Särlvik, Bo, and Ivor Crewe. 1983. *Decade of Dealignment: The Conservative Victory of 1979 and Electoral Trends in the 1970s*. Cambridge: Cambridge University Press.

Schultze, Rainer-Olaf. 1995. "Voting and Non-Voting in German Elections." In *Electoral Abstention in Europe*, ed. Joan Font and Rosa Virós. Barcelona: ICPS.

Schweizer, Steven L. 1995. "Participation, Workplace Democracy, and the Problem of Representative Government." *Polity* 27 (Spring):359–77.

Seligson, Mitchell A., Annabelle Conroy, Ricardo Córdova Macías, Orlando J. Pérez, and Andrew J. Stein. 1995. "Who Votes in Central America? A Comparative Analysis." In *Elections and Democracy in Central America, Revisited*, ed. Mitchell A. Seligson and John A. Booth. Chapel Hill: University of North Carolina Press.

Shaffer, Stephen D. 1982. "Policy Differences Between Voters and Non-Voters in American Elections." *Western Political Quarterly* 35 (December):496–510.

Sidjanski, Dusan. 1983. "Turnout, Stability, and the Left-Right Dimension." In *Switzerland at the Polls: The National Elections of 1979*, ed. Howard R. Penniman. Washington, DC: American Enterprise Institute.

Smeets, Ingrid. 1995. "Facing Another Gap: An Exploration of the Discrepancies Between Voting Turnout in Survey Research and Official Statistics." *Acta Politica* 30 (July):307–34.

Smith, Julie. 1995. "The 1994 European Elections: Twelve Into One Won't Go." *West European Politics* 18 (July):199–217.

Sobel, Richard. 1993. "From Occupational Involvement to Political Participation: An Exploratory Analysis." *Political Behavior* 15 (December):339–53.

Staveley, E. S. 1972. *Greek and Roman Voting and Elections*. Ithaca, NY: Cornell University Press.

Steinberg, Jacques. 1996. "Vote Changes New York School Board Little." *New York Times* (National Edition, June 12):B8.

Studlar, Donley T., and Susan Welch. 1986. "The Policy Opinions of British Nonvoters: A Research Note." *European Journal of Political Research* 14 (1–2):139–48.

Teixeira, Ruy A. 1992. *The Disappearing American Voter*. Washington, DC: Brookings Institution.

Tingsten, Herbert. 1937. *Political Behavior: Studies in Election Statistics*. London: P. S. King & Son.

Topf, Richard. 1995. "Electoral Participation." In *Citizens and the State*, ed. Hans-Dieter Klingemann and Dieter Fuchs. Vol. 1 of *Beliefs in Government*. Oxford: Oxford University Press.

Tufte, Edward R. 1978. *Political Control of the Economy*. Princeton: Princeton University Press.

Urwin, Derek W. 1974. "Germany: Continuity and Change in Electoral Politics." In *Electoral Behavior: A Comparative Handbook*, ed. Richard Rose. New York: Free Press.

van der Eijk, Cees, Mark N. Franklin, and Michael Marsh. 1996. "What Voters Teach Us About Europe-Wide Elections: What Europe-Wide Elections Teach Us About Voters." *Electoral Studies* 15 (May):149–66.

Verba, Sidney. 1996. "The Citizen as Respondent: Sample Surveys and American Democracy." *American Political Science Review* 90 (March):1–7.

Verba, Sidney, Norman H. Nie, and Jae-On Kim. 1978. *Participation and Political Equality: A Seven-Nation Comparison*. Cambridge: Cambridge University Press.

Verba, Sidney, Kay Lehman Schlozman, and Henry E. Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*. Cambridge: Harvard University Press.

Verplanke, C. J. 1965. "De opkomstplicht." *Anti-Revolutionaire Staatkunde* 35 (March):75–98.

Wattenberg, Martin P. 1991. *The Rise of Candidate-Centered Politics: Presidential Elections of the 1980s*. Cambridge: Harvard University Press.

Wertheimer, Alan. 1975. "In Defense of Compulsory Voting." In *Participation in Politics*, ed. J. Roland Pennock and John W. Chapman. New York: Lieber-Atherton.

Widfeldt, Anders. 1996. "The Swedish European Election of 1995." *Electoral Studies* 15 (February):116–19.

Wolfinger, Raymond E. 1994. "The Rational Citizen Faces Election Day or What Rational Choice Theorists Don't Tell You About American Elections." In *Elections at Home and Abroad: Essays in Honor of Warren E. Miller*, ed. M. Kent Jennings and Thomas E. Mann. Ann Arbor: University of Michigan Press.

Wolfinger, Raymond E., David P. Glass, and Peverill Squire. 1990. "Predictors of Electoral Turnout: An International Comparison." *Policy Studies Review* 9 (Spring):551–74.

Wolfinger, Raymond E., and Steven J. Rosenstone. 1980. *Who Votes*? New Haven, CT: Yale University Press.

American Political Science Review Vol. 91, No. 1 March 1997

Capabilities, Perception, and Escalation

BRUCE BUENO DE MESQUITA and JAMES D. MORROW *Hoover Institution*
ETHAN R. ZORICK *University of Essex*

The evolution of crises depends upon interpreting intentions under uncertainty. We model crises as a game of two-sided incomplete information. Players are uncertain about their own payoffs from war because of differences between observable and actual capabilities. We derive four hypotheses, testing them against crises in Europe between 1815 and 1970. We show a nonmonotonic relationship between ex ante observable capabilities and the likelihood of violence in a crisis, as well as the ex ante likelihood of a negotiated settlement. We answer five questions: (1) How do differences in observable capabilities between rivals influence the likelihood of a crisis and the escalation to violence? (2) How do intangible capabilities alter the effects of observable capabilities on the likelihood of conflict and violence? (3) What do national leaders learn from the responses of their adversaries in crises? (4) Under what conditions can deterrence succeed? (5) Under what conditions are the strong likely to give in to the weak or vice versa in a crisis?

In 1856 Frederick William IV, the king of Prussia, demanded the surrender of Neuchâtel by the Swiss and asserted that a failure to comply within the month meant certain war. Yet, the Swiss held firm, the Prussian ultimatum lapsed, and Prussian forces retreated, which assured Swiss sovereignty over the canton of Neuchâtel. Why did a great power like Prussia accept defeat at the hands of a militarily insignificant nation so soon after threatening it with an ultimatum? Why would one nation demand something of another, under threat of war, only to back down later? This paper suggests a general answer to these and related questions.

Our concern is why some foreign policy relationships become conflictual and why some disputes escalate while others do not. We develop a model of international crises that is related to a recent class of formal models (Banks 1990; Bueno de Mesquita and Lalman 1992; Fearon 1990, 1994a; Kilgour and Zagare 1991, 1993, 1994; Kim and Bueno de Mesquita 1995; Langlois 1991; Morrow 1989a, b; Powell 1989, 1996). These models, though differing in their specific structural details, all focus on the role of uncertainty and perceptions in crisis decision making. They share a common concern with how the prior beliefs of national leaders influence disputes and with how those beliefs are revised as disputes progress. Our model highlights these specific areas and illuminates how uncertainty about war outcomes affects decision making.

We address the influence of uncertainty about differences in national capabilities, which in turn influence perceptions of the chance for success and, thereby, decisions to initiate or escalate crises. This is an important difference between the model proposed here and previous analyses of signaling in international crises. By focusing on uncertainty about capabilities we draw attention to conditions under which participants in a crisis cannot be sure of their own payoffs in the event of a violent dispute. Their uncertainty about payoffs remains even after they acquire private information about their own capabilities. This fundamental source of uncertainty has been overlooked in previous research on crisis escalation.[1] The payoffs to players depend on all players' private information. As they update their beliefs about their opponent's capabilities, they also update their own expected payoffs in the event of armed conflict.

The model and our results address several fundamental questions about international conflict. We highlight new conclusions and help clarify issues in existing debates. The questions we examine are as follows.

1. How do differences in observable capabilities between rivals influence the likelihood of a crisis and the escalation to violence?
2. How do intangible capabilities alter the effects of observable capabilities on the likelihood of conflict and violence?
3. What do national leaders learn from the responses of their adversaries in crises?
4. Under what conditions can deterrence succeed?
5. Under what crisis conditions are the strong more likely to cede to the weak than vice versa?

Our primary result shows that the probability of a crisis escalating to armed conflict as a function of observable relative strength is *nonmonotonic*. This finding is strongly supported by the evidence presented here. In addition, we show that this nonmonotonic relationship is not expected by other theories of general or immediate deterrence in crises. The nonmonotonicity indicated by our theory will not disappear as the measurement of capabilities improves. Intangibles, by definition, are unmeasurable ex ante, and they always influence decisions. Intangible capabilities produce effects on decisions that influence whether disputes are initiated, whether they escalate, and whether

---

Bruce Bueno de Mesquita is Senior Fellow and James D. Morrow is Senior Research Fellow, Hoover Institution, Stanford University, Stanford, CA 94305-6010. Ethan R. Zorick is Lecturer in International Relations, Department of Government, University of Essex, Colchester Essex C04 3SQ, United Kingdom. The authors benefitted from comments and suggestions raised in the Politics and Organization Seminar at Stanford University and from discussions with Jeffrey Banks, Randall Calvert, James Fearon, and Robert Powell.

[1] Nalebuff 1987 as well as Morrow 1989a, b also use models with some of the same features as the game we use here.

one or both parties are prepared to use force to press their demands.

As a crisis progresses, nations gain information about the relative strength of their opponent and make their decisions based on this new information. When a nation chooses to initiate a dispute it has better information about its own capabilities than about those of its opponent. Since the probability of victory depends on the capabilities of both sides, this uncertainty means that each side cannot know its own payoffs for some outcomes. After all, the payoffs in the event of hostilities depend on the unknown probability of victory for each side. At each succeeding stage of the crisis the actors learn about each other, thereby gaining a more accurate estimate of the strength of their foe.

We begin with a discussion of the limited-information approach to crises in general. Next, we present and discuss our model and the testable implications that arise from it. These hypotheses are then evaluated against a large data set of European dyadic international interactions between 1816 and 1970.

THE LIMITED INFORMATION APPROACH

Disputes arise when one nation demands something of value from another nation. The decision to make a demand necessitates judgments regarding what is to be demanded and what the expected reply will be. The target of a demand must also make judgments. Accepting or rejecting a demand requires assessments of both the losses associated with ceding to the demand and the credibility of the threat accompanying the demand. The initiator of the crisis has information about the resources it can use against the target but is unaware of exactly what resources the target has available. The target is in a parallel situation; it knows its own capabilities but not those of its rival. Each judgment involves the use of some information which one party knows and which the other party can only surmise from the observed actions of its opponent. As a player chooses to initiate, escalate, or terminate the crisis, the opponent may be able to surmise additional information about its foe's capabilities.

Of course, not all important factors relevant to a crisis are private information. Some factors in any situation are known by all participants, with each knowing that everyone else knows that information. These factors include, for example, the status quo and outstanding grievances between the parties. Basic features of national military capabilities are common knowledge, although the detailed particulars of morale, skill of leadership, willingness to suffer costs, effectiveness of military doctrine, reliability of technology, and so forth, typically are private information.

International relations scholars have been acutely aware of the incompleteness of knowledge about national capabilities. Indeed, it is a sensitivity to just such incomplete information that has spawned the many efforts to improve measurements of national capabilities (Fucks 1965; Mearsheimer 1989; Moul 1988; Organski 1958; Organski and Kugler 1980; Singer, Bremer, and Stuckey 1972). These efforts are motivated by the notion that improved estimates of capabilities are essential to determine the outcome of a crisis. Certainly, improved estimates can reduce measurement error, which undoubtedly is valuable. But we are concerned here with ex ante unmeasurable factors that influence *actions* in a crisis. A combination of observable resources on each side and ex ante unobservable features shape actions in crises. The outcome of any armed conflict is a function of the capabilities of both sides. As more information about the other state's resources becomes available, expectations about armed conflict will necessarily change.

Our goal is to think systematically about how inherently unobservable elements influence behavior during a crisis. Our model links the behavioral influences of ex ante unobservable aspects of national capabilities to the process of learning about adversaries from their actions during a crisis. In the framework we adopt, beliefs matter because foreign policy choices are based on perceptions. All the germane information cannot be known in advance; actors must rely on their judgments.

If only observable capabilities matter, then each state's chance in war would be fairly predictable. In such circumstances, states would be likely to avoid costly confrontations because they could anticipate the outcome. The strong would demand, and the weak generally would comply, saving both sides the costs of the conflict (Fearon 1995). Crises involving the threat of violence would be unlikely to occur. Intangible differences allow the weak to believe they can overcome the seemingly strong. Likewise, those with tangible advantages may recognize their own unobservable weaknesses and, therefore, demur rather than press forward. This uncertainty, then, opens the possibility that the seemingly weak can overcome their foe by force of arms or by convincing the foe to back down. As long as two states are sufficiently close together in *observable* capabilities that the intangible differences could shift the balance, the role of intangible military qualities is essential. Improved indicators of observable capabilities can *never* eliminate this fact.

Indeed, many believe that war arises because uncertainty leads to misperceptions (e.g., Jervis 1976, Lebow 1981). We agree that uncertainty and the subjective beliefs of actors are essential features of the choice process, and we agree that uncertainty makes the question of differences in perceptions central.[2] As events unfold and new information is acquired, actors revise their beliefs. Of course, in reality, as in our model, war is sometimes caused by beliefs that prove to be mistaken or the result of a rational bluff. At other times it is an anticipated consequence of foreign policy decisions.

---

[2] We distinguish between misperception and differences in perception. It is often difficult or impossible to say, even after the fact, what true reality was in an uncertain world. It is not surprising that ex ante perceptions appear inaccurate. We can say how things turned out and who ended up doing well or poorly. Of course, the ultimate outcome itself need not be an indicator about whose ex ante perceptions were closer to the unknown reality. Good discussions of related points can be found in Fearon 1995, Huth and Russett 1990, and Kim and Bueno de Mesquita 1995.

Choices in crises are interdependent. Each side must be aware that its own actions inform the judgments of its rivals and help shape their responses. Therefore, each side must pick its actions with this realization in mind. Games of limited information are particularly well suited for situations involving strategic interdependence under uncertainty. Consequently, game theory is the appropriate analytic tool for modeling crises.

Moves in a crisis serve as signals about players' types. It is commonly believed that an aggressive action like a military mobilization reflects confidence in one's war capabilities (Huth and Russett 1990, Lebow and Stein 1990). If such a signal is so interpreted, then it may serve to deter one's opponent from aggressive action. Knowing this, a nation with weak capabilities may mobilize to trick its opponent, thereby avoiding a war. Of course, both sides in a dispute recognize that there are incentives to bluff, and it is these that states must evaluate in a crisis. Each signal must be seen as a potential bluff. The costs associated with each action provide the information each side must interpret. If the costs are sufficiently high, then the signal may alter beliefs (Fearon 1990, Morrow 1989a). In this model, the cost is the risk that the opponent will choose to initiate armed conflict.

A GAME THEORETIC MODEL OF THE ORIGIN AND ESCALATION OF DISPUTES

Our model begins with the choice to initiate a dispute. Initiation requires that a demand be made; otherwise, the status quo remains in effect. If a demand is made, then it carries with it a threat to use force. The exact contents of the demand are exogenous to this model. Whatever the demand, it is the trigger for the onset of a crisis. Making the demand and its associated threat does not commit the demander to use force. The threatener may be sincere about its claimed intention to use force, or it may be bluffing in the hope that the rival will give in.

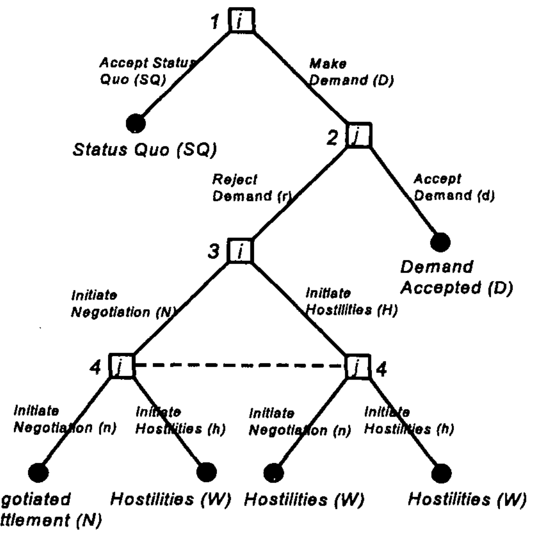
How can the target respond to this demand? The concession could be granted, or the target could resist. Conceding ends the crisis, with the demand replacing the status quo ante. Rejecting the demand continues the crisis. A state may reject the demand, hoping that the initiator of the crisis will not choose violence. Alternatively, the target may be strong enough not to fear violence and stand ready to initiate such an action to defend its interests.

If the target resists, then the antagonists face a difficult decision. Each must worry that the other will seize the moment and attack to enforce its point of view. Knowing that it is at risk, even a state inclined to negotiate may prefer to attack. As Thucydides (1959, 3:12) aptly described the dilemma facing states in a crisis: "Seeing it is in their hands to invade at pleasure, it ought to be in ours to anticipate." We capture this fundamental dilemma in which a state decides to turn to violence or seeks a negotiated settlement in the face of uncertainty about the intentions of its rival by modeling the final choice to use force as being taken by each side without the benefit of knowing the choice the rival will make. That is, we treat this decision as an information set in which it is as if each state makes its choice at the same time. Thus, either can escalate the crisis, bringing each nation's armed forces into the conflict, or can offer a negotiated settlement. At this juncture, either player will escalate to violence if it believes that is in its best interest. If either uses force, the game ends in hostilities. If neither uses force, the game ends with negotiation.

The exact structure of the game we analyze is depicted in Figure 1. The players are labeled i, the initiator, and j, the target. Each side's value for nonviolent outcomes is exogenously given and is common knowledge among the players. In particular, the values of the status quo (SQ), the demand state i can make (D) and the value of the negotiated settlement (N) are assumed to be known in advance. These values have several restrictions. First, the value of having its demand accepted is larger for state i than the value of negotiation ($U_i(D) > U_i(N)$). State j values negotiation over accepting the demand ($U_j(N) > U_j(D)$). Furthermore, state i values its demand more highly than the status quo ($U_i(D) > U_i(SQ)$).

The game in Figure 1 is not intended as a literal depiction of crisis decision making. Rather, the model reflects key strategic elements present in all crises. Each side has the opportunity to avoid violence by making concessions. The first two moves represent this possibility. When a decision to enforce one's position must be made, neither party to a dispute can be certain of the intentions of its rival. This is shown by the information set in Figure 1. In reality, of course, the opportunities to negotiate back and forth before resorting to violence are unlimited; no state can irrevocably make a take-it-or-leave-it demand. Ultimata can always be renegotiated by the issuer; no state is ever obligated to carry out its threats. The structure of the game we propose captures these features while truncating the prospectively infinite time horizon over

FIGURE 1. Game Form



which states may jockey back and forth. Other models of crisis decision making (Bueno de Mesquita and Lalman 1992; Carlson 1995; Kilgour and Zagare 1991; Morrow 1989a, b) stylize the situation by mandating a final move that creates a take-it-or-leave-it offer. Models that appear to have open-ended bargaining (Fearon 1994a; Powell 1987, 1989) create terminal moves by imposing bargaining costs that make war preferable to any negotiated settlement at a later date. These games make the bargaining process finite.

The information set in our model reflects the strategic uncertainty that arises in crises. Nations, when deciding to use violent means themselves, cannot know whether the other side is committed to using violence. Each may prefer to negotiate, but fear that the other will attack may be sufficient to make them choose violence. Our game structure reflects this very dilemma.

Military capabilities come into play if either side chooses to resolve the dispute violently. The outcome of a violent dispute is not common knowledge; it depends on both observable and unobservable factors of military capability. K represents the *observable* military advantage of one or the other side in the event that the antagonists use force to resolve their dispute. K relates only to differences in tangible, observable factors. Among the factors that enter into the value of K are such elements as existing military forces and technology, commonly anticipated support from allies, population, and economic base. K includes strictly ex ante observable differences between i and j that could influence the outcome of violence. K is what is commonly measured by indicators of power or national capabilities, such as those constructed by the Correlates of War Project and others. In our model, K is bounded between -1 and 1. Higher values of K indicate greater advantage for state i in terms of its tangible resources compared to j's. Positive values suggest, on balance, that i has the advantage, and negative values indicate that the advantage lies with j.

Each player has intangible, unobservable military abilities, p_i and p_j. These resources are drawn from a uniform probability distribution on the interval [0, 1], with one draw revealed to each participant in the prospective crisis.[3] Each state knows its own intangibles but must draw inferences about those of its rival. In that sense, these intangible factors represent private information, and the random draws reflect uncertainty about them. Nations are aware that these factors affect their prospects. Without comparable information about an opponent's morale or relations with an ally, the state can only hold beliefs, represented as a probability distribution, about these intangibles. Consequently, no state knows ex ante the true probability of its winning a violent dispute. In our model, i's draw (i.e., i's private information about intangible factors that may help or hinder it), j's draw (j's intangibles), and K together determine the outcome of a violent dispute. Nation i wins if $p_i + K \geq p_j$. Otherwise, j wins.

A numerical example will help clarify the important effects that K, p_i, and p_j can have. Suppose K equals .45, and i's draw on p_i equals .4. State i might reasonably conclude that it has a very good chance of winning in the event there is a violent dispute with j. Indeed, based on the information known to it, i does have an excellent chance of winning. The only way j can defeat i is if its draw on p_j is greater than .85, an event that has only a .15 chance of occurring. Still, j's intangibles—known to it—may be exceptionally valuable. Weak countries, such as Switzerland in 1856 or North Vietnam in 1964–73, do sometimes prevail against seemingly stronger opponents.

A few additional assumptions are required to complete the description of the game in the event of violence. The value for winning equals one, and the value for losing is zero. Each nation also pays a cost, $c > 0$, for participating in armed hostilities. Finally, we assume two other conditions: $1 - c > U_i(N)$ and $1 - c > U_j(N)$. In essence, these conditions require that the *favorable* outcome of an armed conflict be preferred to negotiation. We make these assumptions to keep c, the cost of violence, relatively small. If c is allowed to be large, of course, then violence will never occur in the game, and the problem will have been trivialized.

Before characterizing the equilibria, we define some additional terms. Let pih, pjh, pil, and pjl be critical threshold values, with the precise values found in the Appendix. Pil defines the minimum value for state i to reject the status quo and make a demand. Pjl is the minimum value of p_j at which j resists i's demand. Pih and pjh equal the minimum values of p_i and p_j, respectively, that are required to initiate hostilities.

At node 1, if $p_i > pil$, state i initiates a dispute with j. That is, the expected gains from the dispute outweigh the value of living with the status quo. State j rejects i's demand at node 2 if $p_j > pjl$. The initiator's demands are rejected so long as the rival expects to do better in the ensuing crisis than it will do by acquiescing. If $pil < p_i < pih$, then i will make a demand over which it prefers not to fight. That is, i prefers to negotiate rather than fight if the demand is rejected. In this case, i is taking a risk at node 1. If $pjl < p_j < pjh$, then j will reject i's demand at node 2 but still hopes to avoid armed conflict. Nation j prefers negotiation. If nation i is sufficiently strong that $p_i > pih$, then, having made a demand at node 1, it prefers armed conflict at node 3 following j's rejection of its demand. Armed conflict is the understood end-result of i's foreign policy decision making in this case. Similarly, at node 4 for state j, if $p_j > pjh$, j prefers hostilities to negotiation.

The threshold values pih, pjh, pil, and pjl are determined by evaluating the choice each nation faces at each node of the game. Naturally, pih, pjh, pil, and pjl are functions of the many parameters of the model.

---

[3] With a uniform distribution, the average draw on p for each side is .5. This means that any value for p below .5 indicates that the side is weaker than it otherwise appears to be. A draw above .5 indicates that the side is advantaged. We make sure that the greatest possible value of p is at least as large as the greatest possible absolute value of K. If the absolute value of K were greater than the largest possible value of p, then both sides would know who will win the war, eliminating the uncertainty in the model and in reality. So, p need not be larger than K, but it must be possible for p to be larger than K.

In particular, they are all functions of K, the expected difference between i's and j's capabilities. It is this difference that can be critical in determining whether i or j is *expected* to be victorious in the event of hostilities.

Resolving which actions to take can be quite difficult in a crisis. At nodes 3 and 4, for instance, i and j must each determine its own value for attempting negotiation as compared to choosing violence. They face the problem that if either chooses violence, both bear the costs of that path. To help them make the choice to pursue negotiation or to engage in armed conflict, they take advantage of whatever information has been conveyed to them in the previous moves in the game. So, to understand the choices made at nodes 3 and 4, we must examine what information is conveyed in the first two moves of the game.

What does i know when it reaches node 3? State j has resisted its demand, meaning that $p_j > pjl$. How does i know that p_j really is larger than pjl? State i knows that if $p_j < pjl$, the risk of violence for j outweighs the cost of ceding to i's demand. That is what determines the value of pjl. If $p_j < pjl$, then it is not in j's interest to bluff. State i can be certain that when it makes its decision at node 3, $pjl < p_j < 1$.

State j is in a similar situation when making its choice at nodes 2 and 4. It knows that $p_i > pil$. How can it be sure that this is the case? State j knows that if $p_i < pil$, then state i is not willing to bluff; i prefers to accept the status quo rather than risk violence. The moves at node 3 and 4 are simultaneous, so that j has no opportunity to learn from i's choice at node 3, and i, of course, does not learn from j's move at node 4.

THE GAME'S FIVE EQUILIBRIA

Depending on differing values for the underlying parameters, particularly K, different patterns of behavior are in equilibrium. When K is very small, i does not make a demand, and so the game ends with the continuation of the status quo. This case, equilibrium 1 in the Appendix, corresponds to the threshold value $pil \geq 1$. When K is very large, i always makes a demand, and j always accepts, ending the game with i receiving its desired revision of the status quo. Equilibrium 5 in the Appendix corresponds to the case in which $pjl \geq 1$. In equilibria 1 and 5 the observable power distribution is so skewed that the weaker side has no incentive to risk violence.

When K is not quite so skewed as in equilibria 1 and 5, conflict can occur. In equilibrium 2, where K favors j, it is still the case that some i will make a demand $(0 < pil < 1)$. Some j's accept i's demand, while others reject it $(0 < pjl < 1)$. If the demand is rejected, both sides are committed to using violence to try to enforce their position $(pil = pih$, and $pjl = pjh)$. Equilibrium 4 occurs when all i's make a demand $(pil \leq 0)$. In this instance, K favors i. Some but not all j's resist $(0 < pjl < 1)$. When j resists, violence ensues as in equilibrium 2 $(pil = pih$, and $pjl = pjh)$.

The four equilibria described thus far span all possible values of K. In each of them, any demand that is resisted always leads to armed conflict. There is another equilibrium, however, equilibrium 3, for moderate values of K. The range of K that supports this equilibrium overlaps, but does not cover, the range of K that supports equilibria 2 and 4. In equilibrium 3, making a threat does not entail a commitment to fight. Neither does resisting a demand. Instead, some i's that make a demand offer to negotiate at node 3 $(0 < pil < pih < 1)$. Similarly, some j's that resist a demand seek negotiation rather than pursue armed hostilities $(0 < pjl < pjh < 1)$. It is in this equilibrium that nations, having made a demand, may learn enough about the intangible capabilities of their rival to conclude, by the time the threat of violence is imminent, that negotiation is the best choice. It may appear, ex post, that they bluffed at the outset of the game, but their retreat from the brink of violence arises only because of the resolve demonstrated by their opponent, *a resolve they could not know in advance.*

For the range of K that supports equilibrium 3, equilibria 2 and 4 also exist, and the model cannot guide us as to which of these will be chosen in any particular case. Specifically, neither equilibrium 2 nor 4 Pareto-dominates equilibrium 3 for all types of i and j, and neither does 3 Pareto-dominate 2 or 4. The model provides no reason, then, to believe that the selection of equilibria between 2 and 4, on the one hand, and 3, on the other hand, is deterministically related to K. This means that within the range of K in which equilibrium 3 can be supported, some cases will be equilibria 2 or 4, and some will be equilibrium 3.

What are the empirical implications of the five equilibria? We begin with whether a demand is made. In equilibrium 1, in which K is very small, i never makes a demand. In equilibria 2 and 3, in which K is larger than in equilibrium 1, a demand is more likely as K increases. In equilibria 4 and 5, a demand by i is certain. Therefore,

HYPOTHESIS 1. *The probability of a demand increases with increases in the value of* K.

In equilibrium 2, which represents the first opportunity for j to choose to accept or reject a demand, the probability that j accepts a demand does not vary with the magnitude of K. For low values of K in equilibrium 2, only i's with a very large intangible advantage (p_i close to 1) make demands. As K increases, i's with smaller intangible advantages make demands. So, when K is most advantageous for j but i still makes a demand, j infers that its apparent advantage is offset by intangibles. Conversely, as K becomes less advantageous for j, a demand by i can occur with smaller intangibles for i. This produces a selection effect. Given that j observes a demand by i in equilibrium 2, j's response is essentially invariate with K because increments in K are offset by decrements in intangibles.

The chance that j will reject i's demand also does not vary with K in equilibrium 3 and for the same reasons. But the magnitude of that chance, on average, is slightly higher in equilibrium 3 than in 2. This is true because equilibrium 3 holds out a chance of a negotiated settlement of the dispute, making resistance more attractive; equilibrium 2 does not.[4]

Equilibria 4 and 5 are the inverses of equilibria 2 and 1, respectively. In equilibrium 4, the chance of a demand being accepted rises with K, and in equilibrium 5 demands are always accepted. Looking across the range of possible K's, we expect the probability that j accepts i's demand rises with K, recalling that we cannot distinguish the small difference implied by the selection of equilibrium 2 or 3. We state this as our second hypothesis.

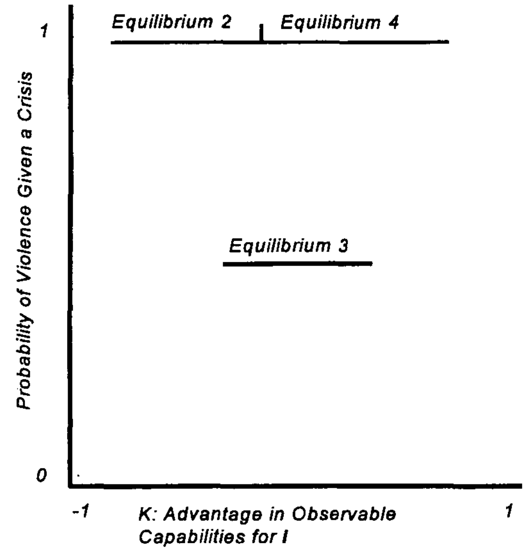
HYPOTHESIS 2. *As* K *increases, the probability that* j *accepts* i*'s demand increases.*

If i's demand is accepted, then the dispute terminates in an acquiescence by the target. If the demand is rejected, then the dispute continues, becoming a crisis. In a crisis, leaders must choose between accepting a negotiated settlement and the use of force (Lalman 1988). The first two hypotheses do not strike us as surprising. The fact that the model produces these intuitively appealing predictions provides confidence in the game's structure when we examine the main counter-intuitive implication.

The ability to reach a negotiated settlement can be confounded or enhanced by the beliefs each state has about its chances of prevailing in armed conflict. Reaching a negotiated settlement requires the assent of both sides. What is the influence of a disparity in observable resources? When a demand is made, we cannot be in equilibrium 1. When a demand is resisted, we cannot be in equilibrium 5. Therefore, at the stage in the game where a choice must be made between violence and negotiation, equilibria 2, 3, and 4 are the only ones of interest.

In equilibria 2 and 4, if a demand is resisted, then violence is certain. In equilibrium 3, if a demand is resisted, then both violence and negotiation are possible. Then, the probability of violence in equilibrium 3 is lower than in 2 or 4. Figure 2 depicts the relationship between the likelihood of violence and K in these three equilibria. Since the range of K's that support equilibrium 3 overlaps but does not cover those for 2 and 4, we expect a nonmonotonic relationship between K and the probability of violence, given a crisis in which the chance of violence decreases and then increases. Because there are multiple equilibria at this juncture in the game, we cannot predict which specific cases will fall into equilibrium 3 and which will fall into 2 and 4. We can predict, however, that those falling into 3 are likely to exhibit more moderate values of K than those falling into 2 or 4. We do know that the cases falling into equilibria 2 or 4 must have larger absolute values of K than in equilibrium 3 and so produce high probabilities of violence given a crisis.

FIGURE 2. Probability of Violence in Equilibria 2, 3, and 4



HYPOTHESIS 3. *The probability of violence given a crisis declines at first with increases in* K *and then rises with further increases in* K.

The intuition behind the nonmonotonicity follows from our earlier discussion of selection effects in equilibria 2 and 4. Recall that in equilibrium 2, i is disadvantaged in terms of its observable capabilities, K. In equilibrium 2, i only makes a demand when it is prepared to fight. Similarly, in equilibrium 2, j knows that i will fight if i made a demand. So, j has learned that i's intangible capabilities must be substantial. Otherwise, i could not afford to risk making a demand. Therefore, if j resists the demand, j must be prepared to fight. Equilibrium 4 mirrors this logic; j will fight if it resists a demand. In equilibrium 3 this selection effect does not operate. When i makes a demand in equilibrium 3, it may be prepared to fight, or it may be open to negotiations. Consequently, j cannot be sure of i's intentions. In the face of this uncertainty, some j's who are unwilling to fight will, nevertheless, resist demands. Likewise, i cannot be sure whether j will negotiate or fight. Some i's will make demands even though they have no intention of fighting for those demands. When capabilities are fairly even, neither making nor resisting a demand is a clear-cut signal of an intangible advantage.

Relatively extreme values of K (within the bounds of equilibria 2–4) select out states that are unwilling to fight. Moderate values of K do not; some states that do not want to fight make and resist demands in the hope of gaining a negotiated settlement. This means, on average, that as the sides become equally matched on paper, violence becomes less likely. Those unwilling to fight are willing to make and resist demands only when their observable capabilities are about equal.

---

[4] In equilibrium 2 the chance of resistance equals $\frac{U_i(SQ) + c}{U_i(D) + c}$. In equilibrium 3 the chance of resistance is $\frac{(U_i(SQ) + c)(1 - U_i(N) - c)}{U_i(D)(1 - U_i(N) - c) + c}$. Notice that the latter contains $1 - U_i(N) - c$ in the numerator and denominator, which would cancel and reduce to the former except for the addition $+c$ in the denominator for the term from equilibrium 3. Recall that c is assumed to be a small quantity. Thus, the difference between the two terms is small.

A final question we address concerns the ex ante probability that a dispute ends in negotiations. Obviously, the probability of negotiation *given a crisis* must be nonmonotonic in the opposite manner from the probability of violence given a crisis. But, at the outset of a putative dispute, the relationship to expect between K and negotiation is not self-evident. When K is very small, as in equilibrium 1, no demand is made, so there is no chance for negotiation later in the game; the game ends with the status quo. In equilibrium 5, in which K is very large, there is also no ex ante chance for negotiation because j cedes to i's demand to avert a crisis. In the model, as we have seen, negotiation can only occur in equilibrium 3. Therefore, ex ante, as K increases from equilibria 1 and 2 to 3, the probability of a negotiated settlement rises. As K increases further, from equilibrium 3 to equilibria 4 and 5, the chance of a negotiated resolution decreases.

HYPOTHESIS 4. *The ex ante probability of a negotiated settlement rises as* K *increases and then falls.*

Hypotheses 1 and 2 are as one might expect, while 3 and 4 may seem puzzling. Hypothesis 3 diverges from a prominent hypothesis in the literature (e.g., Huth and Russett 1984). As Huth (1988, 41) notes: "The probability of [immediate] deterrence success increases as the balance of military forces between attacker and defender shifts to the advantage of the defender." In our terms, this claim is that violence (i.e., failure of immediate deterrence) is more likely as K increases. We claim this relationship reverses direction for large enough values of K.

Fearon (1990, 1994b) reinforces the monotonic hypothesis of Huth and Russett, although the argument leading to his conclusion is quite different. His models under limited information show that "the more (ex ante observable) balance of forces favors the defender, the more likely is the defender's threat to work" (Fearon 1994b, 247). Fearon's argument depends on a selection effect: Weak challengers only threaten peripheral interests of stronger opponents, reducing the chance that the defender will protect such an interest. If the defender resists the challenge in this case, then the challenger is likely to back down because the defender's unexpected resistance is a strong signal of the defender's unobservable willingness to fight. In the Huth and Russett argument, the challenger backs down because of its observable military weakness.

Our hypothesis 3 combines these two arguments. Surprisingly, the combination leads to a departure from the view that when the ex ante observable military balance favors the defender (K is small), immediate deterrence is more likely to succeed. Our model concerns signaling over unobservable military capabilities, a factor not considered in the alternative arguments. Consequently, they predict a monotonic relationship between ex ante observable capabilities and the likelihood of violence. We predict a nonmonotonic relationship. In particular, the prior arguments contend that the probability of violence always increases with K. We contend that at the immediate deterrence stage the probability of violence decreases as K increases up to a point, after which the probability of violence increases with K as in the prior arguments. So, we agree with Huth and with Fearon that the prospective challenger is least likely to threaten when it is weakest relative to the defender, but we disagree that it is also unlikely to fight given that it made a demand. In our model, relatively weak challengers who make demands are very likely to fight if their demand is resisted. This, then, represents an important novel claim about crisis escalation that allows us to test our model against the models and arguments in the literature.

TESTING THE HYPOTHESES

To test our hypotheses we rely on the militarized interstate disputes data (Gochman and Maoz 1984) for Europe from 1816 to 1970 and on the data set from Bueno de Mesquita and Lalman's *War and Reason* (1992). They provide a basis for estimating K, the critical independent variable of interest to us.

The operational definition of K is based on two separate sets of indicators of observable capabilities. The Correlates of War Project has constructed a widely used indicator of national composite capabilities, which we denote as CAP, with a subscript identifying the country in question. This indicator is the average of each nation's share of the international system's military expenditures, military personnel, total population, urban population, commercial fuel consumption, and iron or steel production.[5] Each of these six components is certainly readily observable. We convert the CAP indicators for each dyad into a variable normalized between -1 and 1 by the following means: Normalized $CAP = (CAP_i - CAP_j)/(CAP_i + CAP_j)$.

We also compute a modified version of the composite capabilities score that takes into account the shared security commitments of states as a reflection of their relative intensity of preference for supporting one side or the other in the event of an international dispute between any specified dyad. The formula for the calculation of this adjusted composite capability score is as follows:

$$AdjCap_i = \frac{\sum\limits_{k|U_{ki}>U_{kj}} (CAP_k)(U_{ki} - U_{kj})}{\sum\limits_{k=1}^{n} (CAP_k)|(U_{ki} - U_{kj})|}$$

with k being a state, and U_{ki} and U_{kj} referring to state k's utility for the policies of states i and j, respectively. The "utility" terms are based on Bueno de Mesquita's tau b alliance portfolio similarity indicator and his measure of risk-taking propensities (1975, 1981a, 1985; see also Morrow 1987). As with the CAP indicator, all the elements in the adjusted capabilities measure are based on ex ante readily observable alliances, including defense pacts, ententes, and nonaggression and neutrality pacts. The *AdjCap* measure and the *CAP* measure are strongly correlated (.78), but nearly 40% of their variance is not shared.

---

[5] The indicator counts pig iron production before 1900 and ingot steel production after 1900. Commercial fuel consumption in metric ton equivalents is included as part of the composite capabilities only after 1885.

With these components in mind, we construct *K*. The operational definition is:

$$K = \frac{Normalized\ CAP + (AdjCap_i - AdjCap_j)}{2}$$

K varies between +1 and −1.

Of the 707 dyads in our data, 469 eventuated in a dispute.[6] To test our hypotheses we use a series of logit analyses. The independent variables in all these tests measure the difference in mean expected capabilities of the states in the dyads, that is, *K*. The left-hand side variables of each of these is indicated in the appropriate table and corresponds with a state having taken the action indicated in the table. The first dependent variable asks whether state *i* made a demand accompanied by an explicit or implicit threat to use force. This is equivalent to any event in which the initiator at least threatened to use force.[7] In 238 of our observations we use randomly paired European dyads in which such a demand was not made in the year in question. For those cases, the dependent variable is coded as 0. The remaining 469 cases are coded as 1. The theory instructs us to look only at cases in which a threat was made when we examine the expected response of state *j*. The second dependent variable is coded as 0 if state *j* rejects *i*'s demand and as 1 if the demand is accepted. Rejecting the demand is equivalent to the defender making a counter-threat or taking stronger action, as coded in the militarized interstate disputes data set as described in Bueno de Mesquita and Lalman (1992, 283–6). The 238 cases in which *i* did not make a demand are not included because they are not relevant to this part of the analysis.

If *j* accepts *i*'s demand, then the dispute ends according to our model. Therefore, in testing hypotheses about the subsequent response by the disputants to the continuation of the conflict, we focus only on the 316 remaining cases in which *i* made a demand and *j* rejected it. The hypothesis concerned with the escalation of the dispute to violence is tested by creating a dependent variable coded as 1 if either (or both) belligerents reached a hostility level of at least 4 in the militarized interstate disputes data. That is, each side used force.

TABLE 1. *K* and the Probability a Demand Is Made

	Constant	*K*	
Initiate dispute	0.62	0.430	$\chi^2(1) = 8.73$
Standard error	0.08	0.150	$p = 0.003$
Probability (one-tailed)	0.00	0.002	$N = 707$

The theory developed here is concerned with violent or nonviolent dispute resolution. Yet, most studies of international crises address whether a war takes place, defining war as satisfying the Correlates of War criteria, which require mutual use of force sufficient to inflict at least 1,000 battle-related fatalities and involving at least 100 regular troops on each side. These events are coded as 5 in the Gochman and Maoz hostility-level data. Most and Starr (1983) argue that the precise definition of war can influence results. So that our results can more easily be compared to other analyses regarding crises, we distinguish among (1) violent outcomes, whether reciprocal or not; (2) events involving the reciprocal use of force, including those that fall below the Correlates of War threshold (and so are coded as 4 in the Gochman and Maoz hostility-level indicator); and (3) wars that satisfy the Correlates of War criteria. This analysis highlights the importance of selection effects in studies of international conflict. There are critical differences between reciprocal violence and war; they are not two peas in the same pod in our argument or in reality.

The first hypothesis indicates that the larger the expected advantage, *K*, of the potential initiator, the greater is the chance that a demand occurs. Table 1 shows the results of the logit analysis. The first hypothesis is strongly supported by the evidence. For every 10% increase in *i*'s capabilities relative to *j*'s (*K*), there is a 5.4% increase in the likelihood that *i* will make a demand.[8] So, if *i* were to double its capabilities advantage, there would be a 54% increase in its chances of making a demand. Clearly, the substantive effect of *K* is substantial.

The second hypothesis states that *j*'s likelihood of accepting the demand increases as the mean expected strength of *i*, embodied in *K*, increases. Therefore, we expect the coefficient associated with *K* to be positive. This test is performed on the subset of cases in which a demand was actually made. Table 2 reports the results. As can be seen, our hypothesis is once again supported by the empirical evidence. The substantive effect of increases in *K* on *j*'s decision to resist or accept *i*'s demand is even larger than its effect on *i*'s decision to make the demand. For every 10% increase in *K*, there is an 8.5% increase in the odds that *j* will accept the demand.

Recall that our third hypothesis anticipates a nonmonotonic relationship between *K* and the probability

---

[6] Of the 469 disputes in our data set, we have complete data on 466, and so lose three: the Netherlands versus Britain, France, and Belgium in 1832. The Netherlands is coded in the Gochman-Maoz data set as having taken no action while also being coded as the initiator of the interaction.

[7] The Militarized Interstate Disputes data, first developed by Gochman and Maoz, categorize disputes so that a score of 1 indicates that a nation took no action in response to a threat. Threats without the use of force and without mobilization of armed forces are coded as 2. A code of 3 indicates military mobilization or the display, but not use, of force. A code of 4 indicates the use of force, but at a level not high enough to qualify as a war according to standard Correlates of War coding procedures. Violence that qualifies as a war is coded 5. This includes at least 1,000 battle-related fatalities and 100 regular troops committed to combat.

[8] The marginal effect of *K* is estimated using the odds ratio, evaluated at the mean of all independent variables.

TABLE 2. *K* and the Probability a Demand Is Accepted

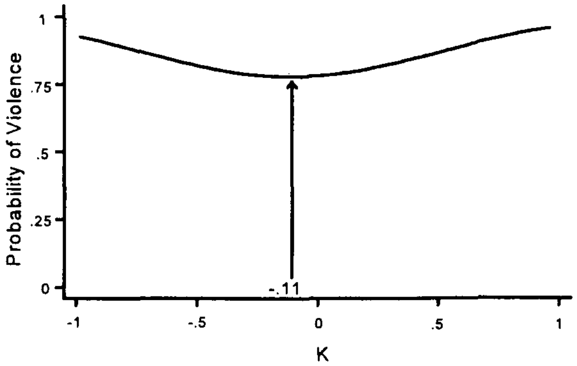
	Constant	*K*	
Accept initial demand	−0.85	0.620	$\chi^2(1) = 10.87$
Standard error	0.10	0.190	$p = 0.001$
Probability (one-tailed)	0.00	0.001	$N = 466$

of violence given a crisis. Specifically, as K increases up to some limit, the likelihood of violence *decreases* (contra Fearon 1994b, Huth 1988, Huth and Russett 1984, and others), but after some point, further increases in K are theoretically expected to produce an *increase* in the probability of the use of force (as in Fearon, Huth and Russett, and others). Therefore, we include a K^2 term in the logit analysis to capture this hypothesized change in direction if it exists in reality. The sample used in this test only includes cases in which an initial demand was made and rejected—immediate deterrence—as this is the contingency under which the hypothesis is operative. The statistical effect of expected strength (K) on the chance of hostilities is indeed nonmonotonic, with the signs anticipated by the theory, and the effect is quite strong and significant.[9] Figure 3 graphs the estimated relationship between K and the probability of violence given a crisis. The curve in Figure 3 has a minimum at $K = -0.11$. The likelihood of violence rises symmetrically for larger and smaller values of K around that minimum.

Table 3 supports hypothesis 3. Still, we must be concerned about whether the nonmonotonicity adds meaningfully to the explanation we offer. To ascertain this, we report the marginal effect of changes in K and examine the logit analysis without the K^2 term. K has two effects on the probability of violence. The linear effect is 4.8% for every 10% change in K, while the nonlinear effect is 45.4% for each 10% change in K^2. Of course, because K lies between -1 and $+1$, K^2 changes more slowly than K. Nevertheless, the nonlinear effect of K^2 is much greater than the linear effect of K for changes in K above 10%.

The results of the logit analysis without K^2 are found in Table 4. The analysis as a whole is not significant when K^2 is omitted, thereby strongly reinforcing the importance of the theoretically derived relationship and suggesting a sharp departure from prior perspectives on this issue. The logit analysis without K^2 yields a χ^2 with two degrees of freedom of only 1.88. The significance level for K in this table is based on a *two-tailed* test because, absent K^2, our theory does not suggest a direction of effect for K by itself. It is interesting to note, however, that the arguments predicting a monotonic relationship anticipate a positive sign for K. A one-tailed test on K in Table 4 is close to conventional levels of statistical significance and helps understand why the monotonic hypothesis is so commonly stated. It is not so much wrong as incomplete.

FIGURE 3. Estimated Probability of Violence as a Function of *K*



Hypothesis 4 indicates that the chance of a negotiated settlement before a dispute even begins has the inverse shape as the probability of violence given that a demand has been made and rejected. Hypothesis 4, like hypothesis 3, makes a novel claim. Table 5 shows strong support for hypothesis 4. We should also note that negotiated settlements and violence are not perfect complements of one another in the data set. Only one-third of the disputes that did not end in violence ended with negotiated settlements (47 negotiated settlements out of 143 nonviolent outcomes). The correlation between negotiation and violence is $-.25$ ($N = 707$). The linear effect of K on the ex ante probability of a negotiated settlement is -3.6% for every 10% change in K. The nonlinear effect is -8.0% for every 10% change in K^2. The probability of a negotiated settlement is maximized when $K = -0.14$.

Now that we have tested the hypotheses deduced from our model, it is appropriate to evaluate how the results differ when the same tests are performed on ad hoc hypotheses about war and reciprocal violence. Recall that our hypotheses are about the probability that a crisis will become violent, whereas the literature generally is concerned with the likelihood of escalation to war. Tables 6 and 7 present replications of the analysis in Table 3, but with the dependent variable changed to reciprocal violence in Table 6 and war in Table 7.

A weakly nonmonotonic relationship is found with respect to reciprocated violence and is completely

TABLE 3. *K* and Its Nonmonotonic Effect on the Probability of Violence

	Constant	*K*	K^2	
Initiate hostilities	1.30	0.39	1.700	$\chi^2(2) = 8.47$
Standard error	0.22	0.34	0.710	$p = 0.015$
Probability (one-tailed)	0.00	0.13	0.008	$N = 316$

---

[9] Recall that the range of K is between -1 and $+1$. Therefore, a positive coefficient indicates negative effects for $K < 0$ and positive effects for $K > 0$, as can be seen in Figure 3.

TABLE 4. Testing a Linear Relationship Between K and the Probability of Violence

	Constant	K	
Initiate hostilities	1.70	0.40	$\chi^2(2) = 1.88$
Standard error	0.16	0.29	$p = 0.17$
Probability (two-tailed)	0.00	0.17	$N = 316$

absent when it comes to Singer-Small wars.[10] Figure 4 depicts the estimated relationships between K and violence, reciprocal violence, and war, respectively. The figure makes clear what we believe is a strong selection effect in international disputes. Reciprocal violence represents further escalation beyond a unilateral decision to use force. Likewise, escalation to war represents a more costly choice than the willingness to use a lower level of violence, as in reciprocal violence. Of course, each level of escalation is nested within the previous level in the data as well as logically.

The likelihood of reciprocal violence is much lower than the likelihood of violence, as is evident from Figure 4. Furthermore, war is much less likely than reciprocal violence. Recall that equilibria 2 and 4 predict that when the absolute value of K is fairly large, and a demand has been made and resisted, then violence is a virtual certainty. Recall also that equilibrium 3 overlaps, but does not cover, equilibria 2 and 4. Therefore, the tails in Figure 4 for violence depict cases of equilibria 2 and 4. The figure clearly reflects the theoretical expectation that violence is a virtual certainty under these equilibria and is less likely under equilibrium 3.

Not all violence is alike. As an extension of the selection logic in our model, we note that as K approaches -1, it is very unlikely that a crisis will arise, but if it does, j should be especially likely to capitulate following the use of force by i. Thus, i's observable weakness coupled with its willingness to use force, signaling its strength in intangibles, make the reciprocal use of violence less likely. The more intense is i's use of force when i otherwise appears weak, the less likely it is that j fights back. A similar picture is also expected with regard to i's preparedness to capitulate when a weak j uses force compared to a strong j. That is, we anticipate a selection effect. The strong are more likely to cede to the weak in a crisis than are the weak to cede to the strong. This seemingly surprising implication is tested in Table 8. As is evident from the table, the expectation is borne out. When the weaker state uses violence, 20% of the stronger adversaries give in. When the stronger side uses force, only 12% of their weaker foes give in. The difference is significant at less than the .05 level.

One threat to the validity of our inferences is that the results are overly dependent on a few cases drawn from a small range in the value of K. That is not the case. In fact, K is almost uniformly distributed across values in the data set of 707 disputes and control cases. The mean in that sample is .10, with a standard deviation of .55. When we examine only the disputes ($N = 466$), K's mean is higher, at .14, with the same standard deviation. When the sample is reduced to reflect the choice node in our game in which j accepts i's demand, the mean for K is .26 with a standard deviation of .53. At this juncture, those cases in which there is at least a moderately high value of K (.2 and above) are more likely to end with j accepting i's demand. Finally, for the subset of cases in which i makes a demand and j does not give in, the mean for K is .13, and the standard deviation is .56. Clearly, K is highly variable under each contingent condition. Furthermore, K's effects are as predicted by the model and show that knowledge of K alone is not a strong predictor of whether the crisis eventuates in violence. As our argument emphasizes, knowing observable capabilities is insufficient to predict the outcome of crises.

TABLE 5. K and the Probability of Negotiated Settlement *Before* a Demand Is Made

	Constant	K	K^2	
Negotiation, ex-ante	−2.21	−0.450	−1.610	$\chi^2(2) = 8.857$
Standard error	0.21	0.330	0.660	$p = 0.012$
Probability (one-tailed)	0.00	0.084	0.008	$N = 707$

DISCUSSION

Especially encouraging for our model is the fact that the nonmonotonic effects of K, which seem counter to other arguments, is strongly supported. Balance-of-power theorists seem generally to believe that an increasing power advantage for one side monotonically raises the risk of war (e.g., Kissinger 1979). Power transition theorists, by contrast, seem to argue that an increasing power advantage for one side monotonically decreases the risk of war (e.g., Gilpin 1981, Organski 1958). Our theory indicates, and the data show, that there is not a simple linear or even monotonic relationship between power distributions and the likelihood of war (Bueno de Mesquita 1981b) or negotiated dispute resolutions. Our theory and the data go on to identify the form that the relationships apparently take. In addition, we are able to show the conditions under which the apparently strong are particularly likely to cede to the seemingly weak.

Our other hypotheses conform with standard views,

TABLE 6. K and Its Nonmonotonic Effect on the Probability of Reciprocal Violence

	Constant	K	K^2	
Reciprocal violence	0.17	0.630	0.64	$\chi^2(2) = 11.33$
Standard error	0.17	0.220	0.46	$p = 0.004$
Probability (one-tailed)	0.33	0.003	0.08	$N = 316$

---

[10] Such wars include at least 1,000 battle-related fatalities and at least 100 regular troops committed by each state involved in the war.

TABLE 7. *K* and Its Nonmonotonic Effect on the Probability of Singer-Small War

	Constant	K	K^2	
Singer-Small war	−1.05	0.650	0.11	$\chi^2(2) = 7.98$
Standard error	0.19	0.250	0.50	$p = 0.02$
Probability (one-tailed)	0.00	0.005	0.42	$N = 316$

TABLE 8. Relative Strength and the Likelihood of Giving in to Violence

	Foe Gives In	Foe Uses Violence
Weaker side uses violence	34 (20%)	137 (80%)
Stronger side uses violence	18 (12%)	137 (88%)

$\chi^2 = 4.15$, $p < .05$

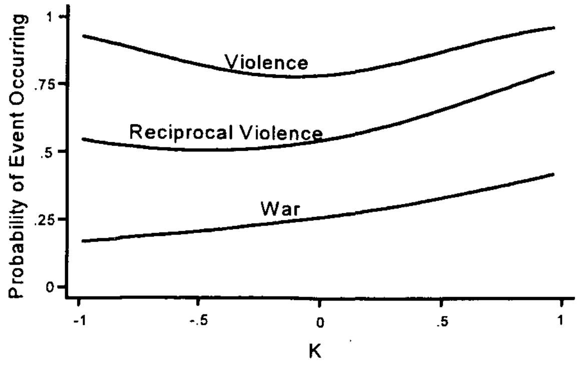
lending added credence to the plausibility of the model we have proposed. At each node in our game, the predicted effect of *K* is supported by the evidence. Furthermore, the evidence is consistent with the contention that decision makers improve their perception of their prospects of success from their opponent's actions in a crisis and adjust their behavior accordingly. Thus, initially observable, tangible capabilities by themselves do not seem to preordain the course of events. Unobservable factors appear to be important to the unfolding of a crisis. The nonmonotonic relationships we identified are the logical consequence of the generally unobservable private information about capabilities that shapes choices in our model.

CONCLUSION

We have shown how ex ante indicators of capabilities help predict the initiation and escalation of crises. We have also shown that these are not sufficient to predict all outcomes. Our limited information approach highlights the nonmonotonic nature of the influence that capabilities are expected to have on crises. Since our evidence is consistent with expectations, we believe that our analysis provides support for such an approach to the study of crises.

Our model also highlights the role that bluffs can play in crises. We observe, as happened at Neuchâtel, that states sometimes make demands they prove unwilling to back with force. At other times states make demands they fight for. The challenge for the target is to separate these types of opponents. Moreover, a rejected demand is a powerful signal of strength on the part of a target. Indeed, it can be so powerful that the state making the initial threat quite sincerely alters its own expectations, such that ex post the threat appears to have been empty. Failure to recognize the distinction between cases of real bluff and cases in which a sincere threat was abandoned because of what was learned about the foe has produced some confusion and misplaced debate in studies of the credibility of deterrence (e.g., Huth and Russett 1990, Lebow and Stein 1990).

FIGURE 4. Estimated Probabilities of Events as a Function of *K*



One cannot assume that the initiator of a demand *knows* from the outset that it will be willing to go to war to enforce its demand. When the demand is made, the state may fully intend to enforce its will with violence if that proves necessary, or it may be bluffing. The target of the threat cannot know which is true. What is more, we have shown that the initiator of the threat cannot know whether it is truly superior in capabilities to its opponent at the outset of the dispute. In the case of Neuchâtel, for instance, when the Prussians issued their ultimatum, it is likely that they thought they were superior to the Swiss. Only later, as they learned more about their own and Swiss capabilities, did their estimates and intentions change. It is not possible to analyze the escalation of crises accurately without taking into account the influence of beliefs about the combined effects of observable and unobservable capabilities on the expected payoffs from war.

APPENDIX

This appendix contains complete statements and proofs of the five equilibria of the game. We abbreviate the moves as given in Figure 1 and specify each equilibrium as follows: (*i*'s first move; *j*'s first move; *i*'s second move; *j*'s second move: *j*'s belief at its first move; *i*'s belief at its second move; *j*'s belief at its second move). All beliefs on the equilibrium path follow directly from Bayes's Rule in all equilibria. Beliefs off the equilibrium path are determined by universal divinity (Banks and Sobel 1987); the most resolved type is the most likely type to defect (i.e., $p_i = 1$).

EQUILIBRIUM 1. For $K \in \left[-1, \frac{U_i(SQ) - U_i(D)}{U_i(D) + c}\right]$, $(SQ$; $(r$ if $p_j > \frac{U_i(SQ) + c}{U_i(D) + c}$, d otherwise); H; h: 1; $\left[\frac{U_i(SQ) + c}{U_i(D) + c}, 1\right]$; 1).

Proof. In response to *j*'s strategy, *i* prefers *SQ* to *D* when

$$U_i(SQ) \geq \left(\frac{U_i(SQ) + c}{U_i(D) + c}\right) U_i(D) + \left(\frac{U_i(D) - U_i(SQ)}{U_i(D) + c}\right)(-c).$$

Note that *i* can never win a war in this case, so its utility from going to war is $-c$. Since the two sides of the statement are equal, it is true. If $p_i = 1$, then *j* with $p_j > K + 1$ prefers *r* to d. H and h are always mutual best replies; if one side uses force, then hostilities result, and the other's move is irrelevant.

Equilibrium 2. Let $\alpha = \dfrac{\dfrac{U_i(SQ) + c}{U_i(D) + c} - K - U_j(D) - c}{1 - U_j(D) - c}$.

For $K \in \left(\dfrac{U_i(SQ) - U_i(D)}{U_i(D) + c}, \dfrac{U_i(SQ) + c}{U_i(D) + c} - U_j(D) - c,\right.$ (D if $p_i > \alpha$, SQ otherwise); (r if $p_j > \dfrac{U_i(SQ) + c}{U_i(D) + c}$, d otherwise); H; h: $[\alpha, 1]$; $\left[\dfrac{U_i(SQ) + c}{U_i(D) + c}, 1\right]$; $[\alpha, 1]$).

Proof. D is a best reply to j's strategy when

$$\left(\frac{U_i(SQ) + c}{U_i(D) + c}\right)U_i(D) + \left(\frac{U_i(D) - U_i(SQ)}{U_i(D) + c}\right)U_i(W) \geq U_i(SQ)$$

where $U_i(W)$ is i's expected utility for hostilities. The lowest type of i to make a demand will lose a conflict against the types of j that resist the demand, so $U_i(W) = -c$ in this case. The inequality above is always true because the left side is always at least as large as the right. j's best reply to a demand is r if

$$U_j(W) \geq U_j(D)$$

$$\left(\frac{p_j - K - \alpha}{1 - \alpha}\right)(1) + \left(\frac{1 - p_j + K}{1 - \alpha}\right)(0) - c \geq U_j(D)$$

$$p_j \geq (\alpha)(1 - U_j(D) - c) + K + U_j(D) + c = \frac{U_i(SQ) + c}{U_i(D) + c}$$

H and h are always mutual best replies. The boundaries of K are found by setting $\alpha = 1$ for the lower bound and $\alpha = 0$ for the upper bound.

Equilibrium 3. Let β, γ, λ, and θ be as follows:

$$\beta = \frac{(U_i(SQ) + c)(1 - U_i(N) - c)}{U_i(D)(1 - U_i(N) - c) + c}$$

$$\gamma = \frac{(U_i(SQ) + c)(1 - U_i(N))}{U_i(D)(1 - U_i(N) - c) + c}$$

$$\lambda = \frac{\gamma - K - U_j(D) - c}{1 - U_j(D) - c}$$

$$\theta = \frac{(\gamma - K)(U_j(N) - U_j(D)) + (U_j(D) + c)(1 - U_j(N) - c)}{(U_j(N) + c)(1 - U_j(D) - c)}$$

For $K \in [\gamma - 1, \gamma + U_j(D) + c]$, ($D$ if $p_i \geq \lambda$, SQ otherwise; r if $p_j \geq \beta$, d otherwise; H if $p_i \geq \theta$, N otherwise; h if $p_j \geq \gamma$, n otherwise: $[\gamma, 1]$; $[\beta, 1]$; $[\gamma, 1]$) is a perfect Bayesian equilibrium.

Proof. D is a best reply for i if

$$\beta(U_i(D)) + (\gamma - \beta)(U_i(N)) + (1 - \gamma)(U_i(W)) \geq U_i(SQ)$$

$$U_i(W) - U_i(SQ) + \beta(U_i(D) - U_i(N)) + \gamma(U_i(N) - U_i(W)) \geq 0$$

$U_i(W) = \dfrac{p_i + K - \beta}{1 - \beta} - c$, so substituting and solving for p_i, $p_i \geq \lambda$. Note that $U_i(W)$ is increasing in p_i, and for $p_i = \lambda$, $U_i(N) > U_i(W)$. Then, the above inequality is true even for p_i, such that $U_i(W) > U_i(N)$.

In response to D, r is a best reply for j if

$$\left(\frac{\theta - \lambda}{1 - \lambda}\right)(U_j(N)) + \left(\frac{1 - \theta}{1 - \lambda}\right)(U_j(W)) \geq U_j(D)$$

$$U_j(W) - U_j(D) + \theta(U_j(N) - U_j(W)) + \lambda(U_j(D) - U_j(N)) \geq 0$$

$U_j(W) = \dfrac{p_j - K - \theta}{1 - \theta} - c$ in this case, so substituting and solving for p_j, $p_j \geq \beta$. Note that $U_j(W)$ is increasing in p_j, and for $p_j = \beta$, $U_j(N) > U_j(W)$. Then, the above inequality is true even for p_j such that $U_j(W) > U_j(N)$.

H is a best reply for i if

$$\frac{p_i + K - \beta}{1 - \beta} - c \geq \left(\frac{1 - \gamma}{1 - \beta}\right)\left(\frac{p_i + K - \gamma}{1 - \gamma} - c\right) + \left(\frac{\gamma - \beta}{1 - \beta}\right)U_i(N).$$

$$p_i \geq \theta$$

j's best reply is h if

$$\frac{p_j - K - \lambda}{1 - \lambda} - c \geq \left(\frac{1 - \theta}{1 - \lambda}\right)\left(\frac{p_j - K - \theta}{1 - \theta} - c\right) + \left(\frac{\theta - \lambda}{1 - \lambda}\right)U_j(N).$$

$$p_j \geq \gamma$$

Equilibrium 4. For $K \in \left(\dfrac{U_i(SQ) + c}{U_i(D) + c} - U_j(D) - c, 1 - U_j(D) - c\right]$, ($D$; ($r$ if $p_j > K + U_j(D) + c$, d otherwise); H; h: $[0, 1]$; $[K + U_j(D) + c, 1]$; $[0, 1]$).

Proof. D is a best reply for i when

$$(1 - K - U_j(D) - c)U_i(D) + (K + U_j(D) + c)U_i(W) \geq U_i(SQ),$$

where $U_i(W)$ is i's expected utility for hostilities. The minimum of $U_i(W)$ is $-c$. When $K \geq \dfrac{U_i(SQ) + c}{U_i(D) + c} - U_j(D) - c$, the above inequality is true. j's best reply if i makes a demand is r if

$$U_j(W) \geq U_j(D)$$

$$(p_j - K)(1) + (1 - p_j + K)(0) - c \geq U_j(D)$$

$$p_j \geq K + U_j(D) + c$$

As before, H and h are mutual best replies. The upper bound for K is found by setting $p_j = 1$.

Equilibrium 5. For $K \in (1 - U_j(D) - c, 1]$, ($D$; d; H; h: $[0, 1]$; 1; $[0, 1]$).

Proof. i prefers D to SQ whenever j plays d and j's best reply to D is d in this case:

$$U_j(D) \geq U_j(W),$$

$$U_j(D) \geq (p_j - K)(1) + (1 - p_j + K)(0) - c$$

$$U_j(D) \geq p_j - K - c \geq p_j - 1 + U_j(D)$$

Because $p_j \leq 1$, this inequality is true. As before, H and h are mutual best replies.

REFERENCES

Banks, Jeffrey S. 1990. "Equilibrium Behavior in Crisis Bargaining Games." *American Journal of Political Science* 34 (August):599–614.

Banks, Jeffrey S., and Joel Sobel. 1987. "Equilibrium Selection in Signaling Games." *Econometrica* 55 (May):647–62.

Bueno de Mesquita, Bruce. 1975. "Measuring Systemic Polarity." *Journal of Conflict Resolution* 19 (June):187–215.

Bueno de Mesquita, Bruce. 1981a. *The War Trap*. New Haven: Yale University Press.

Bueno de Mesquita, Bruce. 1981b. "Risk, Power Distributions, and the Likelihood of War." *International Studies Quarterly* 25 (December):541–68.

Bueno de Mesquita, Bruce. 1985. "The War Trap Revisited." *American Political Science Review* 79 (March):157–76.

Bueno de Mesquita, Bruce, and David Lalman. 1992. *War and Reason*. New Haven: Yale University Press.

Carlson, Lisa. 1995. "A Theory of Escalation and International Conflict." *Journal of Conflict Resolution* 39 (September):511–34.

Fearon, James D. 1990. "Deterrence and the Spiral Model: The Role of Costly Signals in Crisis Bargaining." Presented at the annual meeting of the American Political Science Association, San Francisco, August 30–September 2.

Fearon, James D. 1994a. "Domestic Political Audiences and the Escalation of International Disputes." *American Political Science Review* 88 (September):577–92.

Fearon, James D. 1994b. "Signaling versus the Balance of Power and Interests." *Journal of Conflict Resolution* 38 (June):236–69.

Fearon, James D. 1995. "Rationalist Explanations for War." *International Organization* 49 (Summer):379–414.

Fucks, W. 1965. *Formeln zur Macht*. Stuttgart: Deutsche Verlaganstalt.

Gilpin, Robert. 1981. *War and Change in World Politics*. Cambridge: Cambridge University Press.

Gochman, Charles S., and Zeev Maoz. 1984. "Militarized Interstate Disputes, 1816–1976: Procedures, Patterns and Insights." *Journal of Conflict Resolution* 28 (December):585–615.

Huth, Paul. 1988. *Extended Deterrence and the Prevention of War*. New Haven: Yale University Press.

Huth, Paul, and Bruce Russett. 1984. "What Makes Deterrence Work?" *World Politics* 36 (July):496–526.

Huth, Paul, and Bruce Russett. 1990. "Testing Deterrence Theory: Rigor Makes a Difference." *World Politics* 42 (July):466–501.

Jervis, Robert. 1976. *Perception and Misperception in International Politics*. Princeton: Princeton University Press.

Kilgour, D. Marc, and Frank Zagare. 1991. "Credibility, Uncertainty, and Deterrence." *American Journal of Political Science* 35 (May):303–34.

Kilgour, D. Marc, and Frank Zagare. 1993. "Asymmetric Deterrence." *International Studies Quarterly* 37 (March):1–27.

Kilgour, D. Marc, and Frank Zagare. 1994. "Uncertainty and the Role of the Pawn in Extended Deterrence." *Synthese* 100 (September):379–412.

Kim, Woosang, and Bruce Bueno de Mesquita. 1995. "How Perceptions Influence the Risk of War." *International Studies Quarterly* 39 (March):51–65.

Kissinger, Henry. 1979. *The White House Years*. Boston: Little Brown.

Lalman, David. 1988. "Conflict Resolution and Peace." *American Journal of Political Science* 32 (August):590–615.

Langlois, Jean-Pierre. 1991. "A Theory of Rational Expectations in Recurrent International Disputes." Department of Mathematics, San Francisco State University. Mimeo.

Lebow, Richard Ned. 1981. *Between Peace and War: The Nature of International Crisis*. Baltimore: Johns Hopkins University Press.

Lebow, Richard Ned, and Janice Gross Stein. 1990. "Deterrence: The Elusive Dependent Variable." *World Politics* 42 (April):336–69.

Mearsheimer, John. 1989. "Assessing the Conventional Balance." *International Security* 13 (Spring):54–89.

Morrow, James D. 1987. "On the Theoretical Basis of a Measure of National Risk Attitudes." *International Studies Quarterly* 31 (December):423–38.

Morrow, James D. 1989a. "Capabilities, Uncertainty and Resolve: A Limited Information Model of Crisis Bargaining." *American Journal of Political Science* 33 (November):941–72.

Morrow, James D. 1989b. "Bargaining in Repeated Crises: A Limited Information Model." In *Models of Strategic Choice in Politics*, ed. Peter C. Ordeshook. Ann Arbor: University of Michigan Press.

Most, Benjamin A., and Harvey Starr. 1983. "Conceptualizing 'War': Consequences for Theory and Research." *Journal of Conflict Resolution* 27 (March):137–59.

Moul, William Brian. 1988. "Balances of Power and the Escalation to War of Serious Disputes among the European Great Powers, 1815–1939: Some Evidence." *American Journal of Political Science* 32 (May):241–75.

Nalebuff, Barry. 1987. "Credible Pretrial Negotiation." *Rand Journal of Economics* 18 (Summer):198–209.

Organski, A. F. K. 1958. *World Politics*. New York: Alfred A. Knopf.

Organski, A. F. K., and Jacek Kugler. 1980. *The War Ledger*. Chicago: University of Chicago Press.

Powell, Robert. 1987. "Crisis Bargaining, Escalation, and MAD." *American Political Science Review* 81 (September):717–35.

Powell, Robert. 1989. *Nuclear Deterrence Theory*. New York: Cambridge University Press.

Powell, Robert. 1996. "Stability and the Distribution of Power." *World Politics* 48 (January):239–67.

Singer, J. David, Stuart Bremer, and John Stuckey. 1972. "Capability Distribution, Uncertainty and Major Power War, 1820–1965." In *Peace, War and Numbers*, ed. Bruce Russett. Beverly Hills, CA: Sage.

Thucydides. 1959. *History of the Peloponnesian War*, trans. T. Hobbes. Ann Arbor: University of Michigan Press.

Separation-of-Powers Games in the Positive Theory of Congress and Courts

JEFFREY A. SEGAL *State University of New York at Stony Brook*

The hallmark of the new positive theories of the judiciary is that Supreme Court justices will frequently defer to the preferences of Congress when making decisions, particularly in statutory cases in which it is purportedly easy for Congress to reverse the Court. Alternatively, judicial attitudinalists argue that the institutional structures facing the Court allow the justices to vote their sincere policy preferences. This paper compares these sincere and sophisticated models of voting behavior by Supreme Court justices. Using a variety of tests on the votes of Supreme Court justices in statutory cases decided between 1947 and 1992, I find some evidence of sophisticated behavior, but most tests suggest otherwise. Moreover, direct comparisons between the two models unambiguously favor the attitudinal model. I conclude that the justices overwhelmingly engage in rationally sincere behavior.

Morris Fiorina's 1975 *AJPS* article extolling the virtues of positive political theory listed the judiciary as ripe for mathematical modeling. Though judicial specialists have long examined strategic behavior (e.g., Brace and Hall 1990; Brenner 1979; Caldeira and Wright 1988; Hall 1992; Murphy 1964; Schubert 1959; Songer, Segal, and Cameron 1994; see also Smith 1988), we are only now beginning to see extensive formal models of judicial behavior. For example, Schwartz (1992) has begun modeling various aspects of Supreme Court rules; McNollgast (1990) has used positive political theory to examine judicial oversight of administrative agencies; and Schwartz, Spiller, and Urbiztondo (1994), among others, have examined legislative intent. Perhaps most important, positive political theorists, following Marks's (1988, 1989) lead, have begun to include courts into their separation-of-powers games (Epstein and Walker 1995; Eskridge 1991b; Ferejohn and Shipan 1990; Gely and Spiller 1990; Spiller and Gely 1992).

The separation-of-powers games typically start with an assumption shared by judicial attitudinalists: Judges (frequently Supreme Court justices) are interested in imposing their policy preferences on society. The models diverge because the positive political theorists argue that in order to come as close as possible to achieving that goal the Court must frequently defer to the preferences of Congress, especially in statutory interpretation cases. Attitudinalists, alternatively, argue that the rules and structures of the U.S. political system allow the justices to vote sincerely, that is, without constraints from Congress and/or the president (Rohde and Spaeth 1976, 70–4; Segal and Spaeth 1993, 69–72).

In this paper I evaluate the competing arguments. In the first section I lay out the attitudinal model and then present the separation-of-powers game from the viewpoint of positive political theory. These games demonstrate that the Court must be concerned with the preferences of Congress (and occasionally the president) if it wishes to be an effective policymaker. The following section evaluates the theoretical model, demonstrating that if some seemingly plausible changes are made to the model, different results obtain. Next, I review existing empirical tests of both the attitudinal model and the separation-of-powers games. Finally, I take a more systematic approach to the evidence, considering all Supreme Court statutory decisions in civil liberties cases since the start of the Vinson Court, looking for evidence that the justices change their behavior as the political environment changes.

THE THEORETICAL MODELS

The Attitudinal Model

The attitudinal model holds that judges decide disputes in light of the facts of the case vis-à-vis their sincere ideological attitudes and values (Schubert 1965, Segal and Spaeth 1993). To illustrate, imagine an affirmative action plan, the legality of which must be determined by the Supreme Court. Assume that a company in a large urban area, in response to previous discriminatory practices, sets a goal of 50% minority hires and implements that goal by establishing a hard quota on future hiring. Now imagine a second case, similar to the first in that guilt of discriminatory hiring had been proven, but different in that the company simply agrees to taking race as one factor among many into account in hiring. Both plans are taken to Court on the ground that the hiring plans violate the Civil Rights Act.

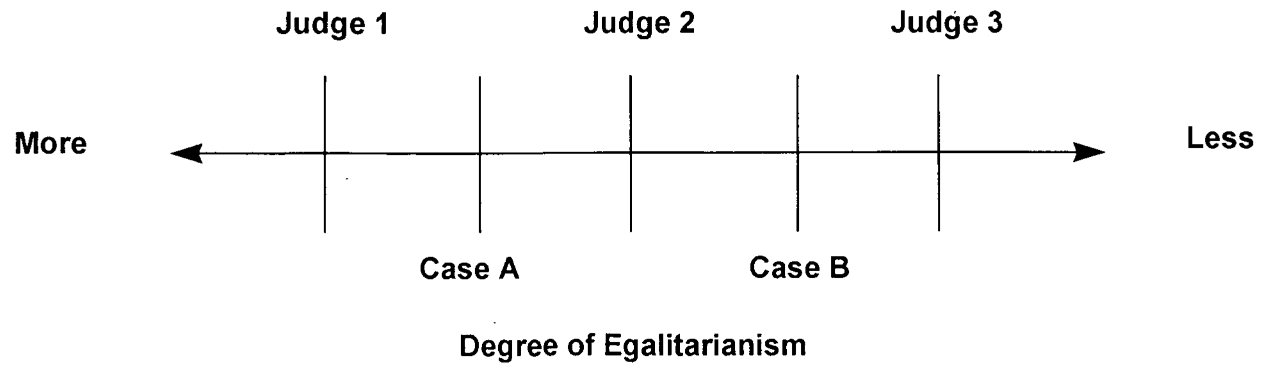
According to the attitudinal model, we can place these cases in ideological space. Since the hard quota is more egalitarian than the soft goal, we place the first case to the left of the second case. This is diagramed in Figure 1, where *A* represents the first case and *B* the

---

Jeffrey A. Segal is Professor of Political Science, State University of New York at Stony Brook, Stony Brook, NY 11794.

An earlier version of this paper, "Marksist (and NeoMarksist) Models of Supreme Court Decision Making: Separation-of-Powers Games in the Positive Theory of Law and Courts," won the Franklin L. Burdette Pi Sigma Alpha Award for best paper presented at the 1995 annual meeting of the American Political Science Association, and the American Judicature Society award for the best paper on law and courts delivered at the same. I thank Harold Spaeth for his Supreme Court data and Robert Boucher for his research assistance and ADA scores. I also thank Larry Baum, Christina Wolbrecht, Joel Kaji, Edward Schwartz, Pablo Spiller, Barry Weingast, and various participants at Columbia University and Washington University brown bags for incisive comments. Special thanks to Charles Cameron and Lee Epstein for advice and criticism throughout the project.

FIGURE 1. Justices and Cases in Ideological Space



second. Presumably, any affirmative action case will locate on the line; depending on its characteristics, the case will be to the left of *A*, between *A* and *B* (inclusive), or to the right of *B*. The more prior justification for the remedy (e.g., a finding of purposeful past discrimination) and the less severe the remedy (e.g., goals versus quotas), the farther to the right the case will fall. The less prior justification and the more severe the remedy, the farther to the left it will be. The points on the line where the cases lie are traditionally termed *j*-points.

Next, we place the justices in ideological space. Consider three judges, 1, 2, and 3, who are, respectively, liberal, moderate, and conservative. They can easily be rank ordered on an ideological scale, with 1 on the left, 2 in the middle, and 3 on the right. With some additional information we may be able to go a bit farther and say that Judge 1 is sufficiently liberal to uphold virtually any affirmative action plan as long as it does not stigmatize particular members of the majority group that is harmed. Thus, we could place Judge 1 to the left of *A*. Judge 2 may not be quite so egalitarian; she would uphold the goals but not the strict quotas. Thus, we could place Judge 2 to the right of *A* but to the left of *B*. Finally, Judge 3 may believe that virtually all affirmative action plans are illegal, and only individuals who have proven that they are the victims of a company's practices are entitled to legal remedy. Thus, we could place Judge 3 to the right of *B*. The judges are placed in ideological space with the cases in Figure 1.

The placements of the judges on the scale constitute their *i*-points, or indifference points. A judge will vote to uphold all affirmative action plans to the right of his or her indifference point and will strike all affirmative action plans to his or her left. Thus, Judge 1 upholds all plans to the right of 1, rejects all plans to the left of 1, and is indifferent whether plans at 1 are upheld or overturned.[1]

For a variety of reasons, including the institutional protections granted the Court (e.g., life tenure, no diminution of pay), and because docket control weeds out legally unambiguous cases, attitudinalists argue that the justices of the Supreme Court are free to vote their sincere preferences. Positive political theorists find at least one gaping hole in the institutional protections offered the Court: Congress, they argue, can readily overturn the Court's statutory decisions.[2]

The Separation-of-powers Model

The basic separation-of-powers model derives from the influential (though unpublished) work of Brian Marks,[3] the first scholar known to have included courts in the American politics separation-of-powers games. Under Marks's game, we begin with a status quo, x°. The Court pronounces its policy choice, x_{ct}, at its own ideal point, C. This becomes the new status quo. Finally, the legislature (L) reacts. The committee with gatekeeping power (G) chooses whether to propose remedial legislation. Congress, without amendment, can adopt or not adopt the committee's proposal. X^* represents the policy outcome in equilibrium. In the unicameral case, if the Court's preferences are within the set of Pareto-optimal (i.e., irreversible) decisions between the committee's ideal point (the committee is assumed to be homogeneous) and the median member of the legislature, the Court's policy cannot be overturned. See Figure 2a. If the Court's decision rests outside the set of Pareto optimals, the results are as follows: (1) If the committee's ideal point is closer to the preference of the median legislator than is the Court's policy, the committee will propose new legislation at its ideal point (Figure 2b); (2) if the committee's ideal point is farther from the median legislator than is the Court's policy, the committee will propose new legislation at the point where the legislature is indifferent between the Court's policy and the committee's proposal, $L(C)$

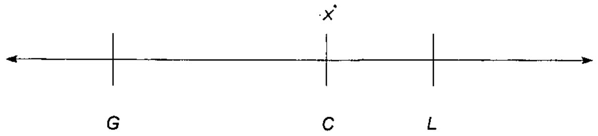
---

[1] Though attitudinalists view justices' decisions in terms of indifference points, these points can map directly into a more traditional Downsian ideological space. The farther to the left a justice's indifference point, the farther to the left is her ideal point. (Or, if one starts from a Downsian perspective, the farther to the left the justice's ideal point, the farther to the left is her indifference point.) Thus, if Justice 2 is the median member of the Court, then the Court will have an ideal point that corresponds to Justice 2's preferences.

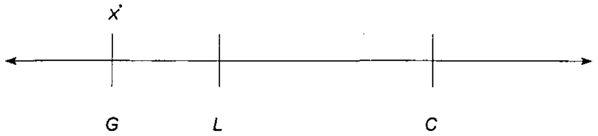
[2] The separation-of-powers scholars typically assume, as do the attitudinalists, that the Supreme Court is not bound by legal factors in cases decided on the merits.

[3] To Eskridge (1991b, 643), it is the "most notable paper"; to Cameron (1993, 1) it is the "locus classicus"; to Rodriguez (1994, 91, n. 395), it is "the seminal contribution."

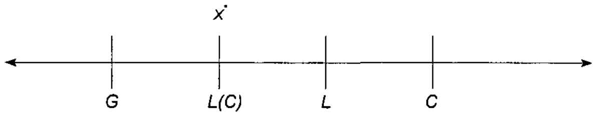
FIGURE 2. Marks's Model



2a. Court policy holds.



2b. Committee introduces at G. Legislature passes.



2c. Committee introduces at *L(C)*. Legislature passes.

Legend: x^* = equilibrium; G = gatekeeping committee; $L(C)$ = legislature indifference point; L = legislature; C = Court.

in Figure 2c.[4] In the bicameral case, if $\min(G^H, G^S, H, S) < C < \max(G^H, G^S, H, S)$, where G^H and G^S are the preferences of the relevant House and Senate gatekeeping committees, respectively, and H and S are the ideal points of the median of each chamber, the Court's policy is a structurally induced equilibrium that cannot be overturned by Congress.

Marks's substantive interest was in explaining why Congress did not overturn the Supreme Court's decision in *Grove City College* v. *Bell* (1984) prior to 1986.[5] The traditional judicial response to such a question is that Congress acquiesced in the Court's decision (see Marks 1989, 6, n. 15). Yet, Marks makes a convincing case that a majority of both chambers preferred the dissenting position (and the Civil Rights Restoration Act) to the majority opinion. The decision stood because the chairman of the Senate Judiciary Committee, Orrin Hatch, preferred the Court's decision to the position favored by a majority of his colleagues. The Court's decision constituted a structure-induced equilibrium that could not be overridden, given the preferences of key players and congressional rules of procedure.

Marks's interest lay solely in the Court's influence on Congress, not vice versa.[6] He considers the judiciary exogenous to the legislative structure: "The Court is unconcerned with the impact of its decision on the legislature" (Marks 1989, 21). It is only subsequent theorists, such as Ferejohn and Shipan (1990) and Gely and Spiller (1990; Spiller and Gely 1992) who consider court decisions endogenous.

Ferejohn and Shipan's primary interest is in Congress's influence on bureaucracy, but they extend their model to include its influence on courts as well. First, under their extended model,[7] we start with a status quo (*SQ*). Second, the agency (*A*) chooses a new policy (*x*). Third, the court chooses between the agency policy and the status quo. Fourth, a legislative committee can propose a new law under an open rule. Fifth, if the committee proposes a new law, the legislature votes on the law under open rule. Figure 3 presents potential preferences of the agency, the committee, and the legislature. In addition, the point labeled $G(L)$ represents the position to the left of the committee at which the committee is indifferent to the position favored by the legislature.[8]

Working backward, we note the following results. If the legislature receives a bill from committee, it will amend the bill to L. The committee will then only propose legislation if G is closer to L than to the policy chosen by the Court (x or *SQ*). We now arrive at what, for purposes of this paper, is the crucial stage of the separation-of-powers model. If the Court voted its sincere preferences, it will simply choose between x and *SQ* based on which is closer to its ideal point. Such a result, though, may not be subgame perfect.[9] For example, consider again the situation depicted in Figure 3. The Court obviously prefers *SQ* to x, but the choice of *SQ* would lead to a proposal by the committee and an amended bill passing at L. Alternatively, if the Court accepts x, the committee will not propose legislation (preferring x to L), the resulting policy thus will be x. Under the assumptions set forth in the model, a strategic Court clearly will choose x over *SQ* even though x is farther from the preferred outcome, since choosing x avoids a final outcome at L.[10] Finally, note that if L moves to the right and G stays where it is, then $G(L)$ moves to the left, thus potentially allowing a liberal Court to move to the left when L moves to the right. This obtains because the committee will tolerate increasingly liberal Court decisions in order to avoid increasingly conservative legislative decisions.

Gely and Spiller (1990) and Spiller and Gely (1992) provide extended models, but regardless of the specific assumptions made, these models (with the important exception of Marks) assume that the Court will push legislation as close to its ideal point as possible without getting overturned by Congress.

Overall, the separation-of-powers model has much in common with the attitudinal model: Justices are policy-motivated actors who use legal rules to achieve

---

[4] Assuming policy space is the real line, $L(C) = 2L - C$.

[5] The Court ruled that racially discriminatory practices by a college would subject only the discriminating offices or programs, not the entire school, to loss of federal funds.

[6] The dissertation is entitled "A Model of Judicial Influence on Congessional Policymaking. . . ."

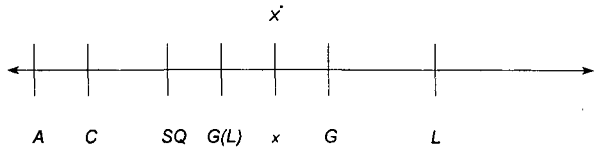
[7] Without presidential veto. The results for the presidential-veto model are essentially the same as far as the Court is concerned.

[8] That is, the distance between $G(L)$ and L is the same as the distance between G and L.

[9] That is, it would not lead to optimal results at the end of the game, given subsequent choices by other players.

[10] Note, moreover, that with the Court limited to a binary choice, it is not even necessarily the case that the Court will choose its more preferred outcome, even when $\min(L, G) < C < \max(L, G)$. If, for example, we move C in Figure 3 from its current position to a position between G and L, and closer to L than to x, then a strategic Court chooses *SQ* over x in an attempt to wind up with legislation at L (Spiller and Tiller 1993).

FIGURE 3. The Court as a Strategic Actor



Legend: x^* = equilibrium; A = agency; C = Court; SQ = status quo; $G(L)$ = gatekeeper indifference point; G = gatekeeping committee; L = legislature; x = agency policy.

policy results. Indeed, one might imagine that legalists would consider the two models minor variations on the same (wrongheaded) scheme. Nevertheless, there exists at least one crucial difference: In the attitudinal model, the justices vote their sincere preferences; in the separation-of-powers model, strategic action leads justices to vote sincerely only when they are within the set of Pareto optimals.

THE SEPARATION-OF-POWERS MODEL REVISITED

The separation-of-powers model, like all models, makes a series of assumptions about the behavior in question. What is noteworthy about the simplifications is that they typically make it more likely that courts will defer to congressional preferences. These assumptions involve the Supreme Court, Congress, and interactions between the two.

Assumptions about the Court

Perfect and Complete Information. Virtually every separation-of-powers work models the justices as having perfect and complete information about the preferences of Congress (Rodriguez 1994, 95; cf. Knight and Epstein 1996). Implicitly, these models also assume complete information about Congress's transaction and opportunity costs (which are, in fact, assumed to be zero). (Alternatively, we can view this as perfect information about whether the Court is facing a Congress that will override if the Court rules outside the set of Pareto optimals, which Congress always would do.) No doubt, it is hard to imagine that if the Court knew a particular decision would lead to a reaction by Congress that would make the result worse, then it would not make the necessary compromises to obtain a better final outcome. But the Court, at best, will have some form of probability distribution as to the preferences of Congress, with some vague notions about the costs to Congress of override and the benefits to Congress of override (based on its preferences and loss function). If the justices typically believe they are facing a Congress unlikely to override its decisions (Eskridge 1991a, 365), then they will be significantly more likely to vote their sincere preferences.[11]

Enforced Statutory Interpretation. The separation-of-powers modelers are nearly unanimous in their decisions to treat the level of interpretation as exogenously determined. Nevertheless, in many cases the Court can opt out of statutory mode and find constitutional bases for its decisions (Spiller and Spitzer 1992). Consider, for example, a Court intent on striking private affirmative action plans. Eskridge (1991b) argues that the *United Steelworkers* v. *Weber* (1979) Court had such preferences but was deterred by a Congress that would have overridden such action. If we forget that a divided Court had upheld the constitutionality of affirmative action just one year earlier (*Regents* v. *Bakke* 1978) and thus could not have been that opposed to affirmative action, we still have the possibility that a conservative Court could have opted into constitutional mode. Following *Shelley* v. *Kraemer* (1948), the Court could have ruled that such private contracts, though legal, could not be enforced by the judicial power of the United States. Forcing the Court into statutory mode underestimates the Court's freedom to act.

Unidimensional Issues. The standard separation-of-powers model forces the Court to reach decisions not only in statutory mode but also in unidimensional policy space. Unlike the congressional committee system, there is no corresponding method that keeps the Court from bundling issues. Indeed, Court cases often contain multiple dimensions (Epstein and Knight 1995). One likely result is that a strategic Court, through what Riker (1984) has labeled "heresthetics," can reframe and/or bundle issues so as to protect them from reversal. For example, the Supreme Court recently required a state institution to provide funding for a Christian newspaper (*Rosenberger* v. *University of Virginia* 1995). Surprisingly, the howls of outrage we might expect from such a decision did not occur. The reason, no doubt, is that the Court packaged the case as a free speech issue. If the University of Virginia generally provides funding to other student newspapers, it cannot deny funding to *Wide Awake* because of the religious slant of its articles. The Court's ability to manipulate issues strategically enables it to protect itself from override by Congress.

Assumptions about Congress

Costless Legislation/salience. The separation-of-powers model treats passing legislation as costless. Needless to say, there are both transaction costs and opportunity costs to passing legislation (Eskridge 1991a; Rodriguez 1994, 96). At the very least this expands the discretionary zone for the Court and thus makes it less likely that the Court will need to defer to Congress out of fear of being overturned.

Limited Veto Points. How legislation gets passed is vitally important to the separation-of-powers model. Theoretically, it is easy to get the gist of the model by a simple placement of a gatekeeping committee and a chamber median. Of course, greater complexity and greater realism can be added by including bicameralism, presidential veto, and so forth, but the fundamental understanding of the model does not change. Nevertheless, greater realism is crucially important to the model if one is interested in assessing the extent to which the Court must defer to Congress. Specifically, as the number of places at which legislation can be kept off the floor increases, the discretionary zone of the Court increases. Thus, under a *potentially* more realistic view of the legislative process, the Court's ability to act sincerely may be guaranteed almost all the time.

---

[11] See Enelow (1981) and Krehbiel and Rivers (1990) on the role of uncertainty within Congress.

In the House of Representatives, virtually all public bills that conceivably may deal with attempted overrides of Supreme Court decisions have to pass from the Judiciary or some other substantive committee through the Rules Committee in order to be placed on the legislative calendar. According to Oleszek (1989, 122), "action, or lack of it, by the Rules Committee generally determines whether a bill is considered at all." That committee, however, is substantially under the control of the Speaker, who appoints majority party members. Moreover, the Speaker, selected by the majority caucus, retains the power of recognition, potentially enabling him or her to block legislation.

In the Senate, major legislation reaches the floor directly from committee either through unanimous consent or by motion. While unanimous consent seems to suggest that any senator can block legislation, reality demonstrates otherwise. Either due to consensual norms (Matthews 1960), rational self-interest (Krehbiel 1986), or legislative bargaining (Oleszek 1989), senators rarely object to unanimous consent agreements. The scheduling of floor action in the Senate resides in the majority leader, often in consultation with the minority leader (Oleszek 1989, 205). While it may be too much to model legislation to require support of both the majority and minority leaders to reach the floor, one plausible conclusion is that it is extraordinarily difficult for legislation to reach the floor without the support of the majority leader.

According to some views of congressional procedure, relevant committee chairs may also be capable of bottling up legislation. Indeed, Marks's (1988) study of *Grove City* (1984) gives full credit/blame for keeping override legislation off the floor to Senate Judiciary Chairman Orrin Hatch. So, too, Eskridge (1991b, 625, 635) finds committee chairs to have special influence over their committee.

To the extent this view of the legislative process is correct (cf. Cox and McCubbins 1993, Kieweit and McCubbins 1991), the set of Pareto optimals is likely to be much greater than is currently posited by the standard separation-of-powers model. For example, between 1956 and 1986 the Senate Judiciary Committee was headed by either James Eastland (D-MS) or Strom Thurmond (R-SC), except for the reign of Ted Kennedy (D-MA) in 1979–80. Eastland's ADA scores while chair averaged 12.1, while Thurmond's averaged 2.5 (Sharp 1988). Meanwhile, the House Judiciary Committee during this period was headed by either Emanuel Celler (D-NY) (1955–72) or Peter Rodino (D-NJ) (1973–88). Both were liberals whose ADA score averaged in the 80s (Sharp 1988). If the support of the Judiciary chairs in both chambers is needed for override legislation, then the Court will be almost completely immune from congressional interference. Thus, the Supreme Court may have significantly more discretion than these models suggest.

Assumptions about Congress/Court Interactions

Last Licks. The separation-of-powers models discussed above and below vary as to whether presidents, agencies, and second chambers of Congress are modeled. One point on which they do not vary is that Congress is always given the final move. Ferejohn and Weingast (1992, 263) note, however, that

> if we can say nothing else with certainty, we can say that there is no "last word" in politics. No person or individual ever gets to say what the law is finally; Congress can and often does react to Court decisions, as can agencies and the president. Each actor in the political and legal setting—president, agency, Congress, litigants, court—can take new courses of action, devise new interpretations or enact new statutes. This capacity to react is a fundamental feature of the political process.

As others have noted, giving Congress the final move is completely arbitrary and, moreover, biases the outcome toward finding congressional influence (Cameron 1993; Rutten 1993). As Ferejohn and Weingast suggest, if Congress overturns a statutory decision of the Court, it is up to the Court to interpret the meaning and the validity of the remedial legislation. For example, the Supreme Court responded to Congress's decision to override *Lampf* v. *Gilbertson* (1991) by declaring pertinent parts of the override unconstitutional (*Plaut* v. *Spendthrift Farm* 1995). Of course, Congress can take the battle into the second round, too. In 1977, the Court ruled existing mandatory retirement plans consistent with the Age Discrimination in Employment Act's exception for any "bona fide employee benefit plan" that was not a subterfuge to avoid the act (*United Air Lines* v. *McMann* 1977). Congress quickly reversed, largely eliminating mandatory retirement plans, but in 1989 the Court narrowly interpreted the override so as not to prevent age discrimination in employee disability plans. Congress again passed override legislation (Eskridge 1991b, 636–8). In at least one such series, "Congress had to pass the same statute three times to achieve its original goal" (Eskridge 1991a, 410). While Congress may win these battles more often than not, the standard separation-of-powers model underestimates the Court's freedom to act by completely eliminating the Court's ability to react to congressional action.

Exogenous Judicial Preferences. The separation-of-powers literature treats judicial preferences as if they were exogenously determined, which allows them often to be far to the right or left of congressional and/or presidential preferences. While it may not be unusual to find particular Supreme Court justices whose preferences lie outside the set of Pareto optimals, it is rare indeed to find that the president and the Senate consistently nominate and approve people who are well to the left or right of their preferences (Cameron, Cover, and Segal 1990; Segal, Cameron, and Cover 1992). This is particularly true when seats that may affect the median position of the Court are at stake (Lemieux and Stewart 1988). Thus, under what is known as the Dahl (1957) model, the Supreme Court follows the preferences of the dominant electoral coalition not because of deference (whether strategic or otherwise) to its preferences, but because it is chosen by that coalition to have the same preferences. By modeling judicial preferences as exogenous, the separation-of-powers model significantly overestimates the need for sophisticated voting.

In sum, I believe I have made a strong enough case to at least cast theoretical doubt on the standard separation-of-powers model. These doubts could be erased, indeed should be erased, if the empirical evidence supports the model. What I will examine next is whether the model empirically improves upon the attitudinal model in its ability to explain Court decisions.

EMPIRICAL EVIDENCE

The Attitudinal Model

The attitudinal model holds that justices decide cases in light of the facts of the case vis-à-vis their sincere attitudes and values. Whatever its limitations, the model has found strong support in the empirical literature. Various scholars have demonstrated the importance of facts/case stimuli in judicial decision making (George and Epstein 1992, Hagle 1992, Ignagni 1994, McGuire 1990, Segal 1984, Segal and Reedy 1988, Wolpert 1991). Of course, case stimuli are also a component of the legal model, so this should be seen as a necessary but clearly not sufficient condition for attitudinal decision making. More recent research has demonstrated that the justices' attitudes are strong predictors of their decisions. Segal and Cover (1989) found a correlation of .80 between their measure of justices' attitudes and decisions in civil liberties cases. More recently, Segal, Epstein, Cameron, and Spaeth (1995) backdated the Segal and Cover work to the Vinson Court and extended it to include economic cases. Though their results are not as strong as the original Segal and Cover findings, attitudes still correlate fairly well with the votes of the justices. Thus, while these works do not demonstrate that attitudes are the sole influence on Supreme Court decisions,[12] they do demonstrate that attitudes are strong predictors of judicial decisions in the two areas constituting the bulk of the Court's docket over the past 50 years: civil liberties and economics cases. Moreover, the spatial model depicted in Figure 1, which includes both case stimuli and attitudes, well explains the justices' search-and-seizure decisions (Segal and Spaeth 1993, chapter 6). There are problems with the empirical tests, to be sure: Epstein and Mershon (1996) find limits to the measures; systematic tests using independent measures of attitudes do not predate the Vinson Court; and areas other than civil liberties and economics have not been well tested (Baum 1994). Nevertheless, even critics of the attitudinal model have conceded its exceptional explanatory ability (Epstein and Mershon 1996; Rosenberg 1994).

The Separation-of-Powers Model

Empirical support for the separation-of-powers model is more problematic. Many articles provide a model, leaving real-world concerns to others. That is fine, but such articles cannot answer the question of whether the Supreme Court actually behaves as posited. More typically, scholars will find a case that seems to satisfy the expectations of the model. This, too, will not do, for as Epstein has so cogently noted, "the modus operandi of the theorem provers who have studied these questions will not suffice. The standards of social science simply require more than reading some cases (e.g., *Grove City* seems to be a favorite), developing a model, and then testing the model against the same cases used to develop it (again, *Grove City* comes to mind)" (quoted in Segal and Spaeth 1994, 12).

The standards of social science to which Epstein refers also demand a degree of reliability and validity to the measurement of preferences that is not found in many separation-of-powers studies. For example, Eskridge (1991b, 650) argues that the early Burger Court kept its civil rights decisions to the left of center because of a Congress substantially to the left of the Court and a series of presidents who were often to the left of Congress. The fact that Richard Nixon—the president who devised the "Southern strategy" for capturing the Wallace vote, who activated that policy by nominating G. Harrold Carswell to the Supreme Court, and who campaigned to end school busing—must be categorized as a civil-rights liberal to make the model work suggests the possibility of alternative explanations. The same can be said of President Ford, who in his last 15 years in Congress never had an ADA score higher than 17.[13] In addition, Eskridge places the gatekeeping committees to the left of Congress, yet the Senate Judiciary Committee median was to the right of the full Senate median every year between 1970 and 1977. He also places the committees to the left of Congress in the 1981 through 1990 period, but again, the Senate Judiciary Committee median was to the left of the Senate median three times during this era, was to the right four times, and was tied three times.[14] I also must question Gely and Spiller's (1990) categorization of Congressman John Dingell (D-MI) as a strong supporter of government safety regulation of the automotive industry.

---

[12] One line of scholarship, in fact, has demonstrated systematic support by the Court for the solicitor general/executive branch in decisions on the merits (e.g., Epstein, Walker, and Dixon, 1989; O'Connor 1983; Puro 1981; Scigliano 1971; Segal 1988).

[13] Nixon's average ADA score was 14 in the House and 26.5 in the Senate (Sharp 1988).

[14] I thank Robert Boucher for sharing his data with me. He gathered ADA scores between 1947 and 1985 from Sharp (1988); from 1986 through 1991 from the *Congressional Quarterly Almanac*; and for 1992 directly from the Americans for Democratic Action. I obtained data on committee assignments from Nelson (1993).

Finally, we must consider Baum's (1995, 4) contention that an extraordinarily wide array of behavior can be interpreted consistently with strategic action. Consider, for example, Eskridge's (1991b) analysis of the Court's decision in civil rights cases. He saves what might otherwise be considered sincere, nonstrategic decisions, that is, those at the Court's ideal point but outside the set of Pareto optimals, because the Court was "mistaken about the congressional median" (p. 658) or was trying to shift congressional preferences (p. 659). With these added assumptions, the strategic model of behavior becomes completely unfalsifiable.

Crucial to Eskridge's more general argument that the Court was constrained in the 1970s is the Court's purported shift to the right on civil rights following the election of Ronald Reagan. Eskridge (1991a, 395–7) provides summary data of voting shifts for the Court as a whole that seem to back his claim.[15] A fuller examination of the data, however, using the U.S. Supreme Court Judicial Database (Spaeth 1994), suggests otherwise.[16] If we compare the moderates and conservatives on the Court from the appointment of Stevens in 1975 until the retirement of Burger in 1986, we ought to find that these justices, who purportedly were constrained in the 1970s, moved to the right beginning in 1981 as Reagan protected them from override.[17] The results, presented in Table 1, show nothing of the sort. First, O'Connor's replacement of Stewart, as Eskridge suggests, had little effect. Next, the Court as a whole, and two of the three justices with liberalism scores below 50% in this period (Burger and Rehnquist), moved slightly (but insignificantly) to the left during this period. Finally, two of the three justices who had scores in the 50–60% range moved to the left (Blackmun, whose move was statistically significant, and Stevens), while one moderate (White) moved ever so slightly to the right.[18] Interestingly, only the liberals, Brennan and Marshall, show a directional move to the right, but by all accounts they should not have been constrained in either period, and indeed their changes are insignificant.

TABLE 1. Proportion of Liberal Supreme Court Justices in Civil Rights Cases, 1975–86

Justice	1975–80	1981–86	*p*	*n*
Court	.52	.51	.85	200
Brennan	.85	.77	.18	196
Stewart/O'Connor	.44	.52	—	84/95
White	.53	.51	.73	200
Marshall	.84	.79	.43	197
Burger	.37	.39	.72	199
Blackmun	.54	.68	.03	195
Powell	.42	.39	.69	185
Rehnquist	.22	.28	.31	197
Stevens	.51	.56	.54	187

Source: Derived from U.S. Supreme Court Judicial Database.

Beyond a doubt the clearest support for the separation-of-powers model comes from Spiller and Gely (1992), who find in their sophisticated econometric analysis that changes in the ideal points of relevant congressmen influence Court decisions in National Labor Relations Act cases to the same extent that changes in the ideal points of the Supreme Court do. Before we accept that finding, a few points need to be examined more fully. First, Spiller and Gely tested their model only under their closed-rule framework. Though the closed-rule model fits the data better than their unconstrained model, it is nevertheless worth noting that, under open rule, "for most observations the set of nonreversible decisions included almost the whole range of feasible outcomes" (1992, 470, n. 24). Thus, under the Spiller/Gely model, if Congress operates under open rule (the Senate almost always does, and the House frequently does [Oleszek 1989, 123, 186]), then the Court is free to vote sincerely in virtually all circumstances.

As for empirical results under a closed-rule model, if the Supreme Court changes its decisions as relevant congressional preferences change, two plausible explanations need to be examined. The change may be due to changing membership on the Court and/or to some subset of sitting justices changing their position in order to avoid being overturned. If decisional change is due to the former, then the results are completely consistent with the attitudinal model: Justices are voting their preferences, and change occurs because new justices arrive. Alternatively, if some of the aggregate change can be allocated to switches in the voting behavior of sitting justices, then the results are consistent with the separation-of-powers model: Change occurs because some justices strategically alter their position in light of changes in the political environment.

Spiller and Gely recognize the problem and attempt to solve it by controlling for the attitudinal predispositions of the justices. And while they probably used the best measure then available (partly identification), there are obvious problems in assuming that both Democrats (e.g., Brennan and Jackson) and Republicans (e.g., Warren and Scalia) on the Court are homogeneous. If Court preferences are not adequately controlled for, and Spiller and Gely's econometric tests suggest the possibility of specification problems, then we may wonder whether the same political environment allegedly by influencing the Court directly (through the Court's vote) is in reality only influencing the Court indirectly (through the appointment process). The results may be the same—the Court follows the election returns—but the explanation differs entirely.

---

[15] Though the separation-of-powers model deals with policy, several of the leading proponents, including Eskridge (1991a) and Gely and Spiller (1990, Spiller and Gely 1992) have used summary voting measures as a valid measure of policy positions. It is hard to imagine that the correlation between the two would not be extraordinarily high, and recent research has systematically demonstrated that it is high (Wenzel 1995). I follow their lead in the work that follows.

[16] I chose all formally decided cases, whether per curiam or with signed opinions. For unit of analysis I originally included, in addition to the standard citation plus split votes, records in the database corresponding to multiple legal provisions (or multiple issues/legal provisions), as numerous cases with statutory content nevertheless could show a constitutional issue in the first record. I then selected all records in which the Court's stated authority for its decision was interpretation of a federal statute, treaty, court rule, executive order, regulation, or rule. Finally, I deleted duplicate records from the same case dealing with the same value area, unless the direction of the decision differed from one record to the next. I then selected all cases Spaeth (1994) coded as dealing with civil rights.

[17] I would be more willing to argue that it was Republican control of the Senate following the 1980 election that freed the conservatives, but Eskridge (1991a, 395, n. 202) disputes this.

[18] Stewart's half term while Reagan was president showed him moving to the right as well, but it is hard to make much over half a term.

The question can best be answered by examining the votes of individual justices. If the separation-of-powers model is correct, then some justices should switch their votes as the political environment changes. Thus, the entire question of allocating changing aggregate behavior to Congress, on the one hand, or to changing justices, on the other, can readily be solved by examining the behavior of individual justices.

In sum, while ardent adherents argue that the separation-of-powers model has been theoretically and empirically verified "beyond any reasonable doubt" (Knight and Epstein 1996, 92), the evidence is far from convincing. More systematic proof clearly is needed.

REEXAMINING THE SEPARATION-OF-POWERS MODEL

Because of the theoretical and empirical concerns noted above, I undertake an examination of whether the voting patterns of the Supreme Court justices change as the political environment changes. Just what shifts in that environment lead to changes in the justices' behavior is not entirely clear, for the constraints they face depend crucially on who controls the legislative process in Congress. Yet, this is not the place to answer longstanding arguments over committee and/or leadership power. Rather, I hope to show that consistent results are achieved independent of the legislative-process model employed.

I examine three models of the legislative process: committee gatekeeping, multiple veto, and party caucus. Briefly, the committee-gatekeeping model resembles the Ferejohn/Shipan (1990) model and explicitly allows the respective Judiciary Committee's median preference to differ from that of its parent chamber. The multiple-veto model grants extensive veto power over the consideration of legislation to committee chairs and the majority party leaders. Finally, the party-caucus model assumes that majority party leaders, committee chairs, and even majority party committee members act as relatively faithful agents of their party caucus.

The tests for each model follow a similar procedure. Specifically, I attempt to place Supreme Court justices and members of Congress on a consistent ideological dimension and measure the preferences of the Court vis-à-vis the set of Pareto optimals established by the relevant model. For each justice/year, I measure whether, under each model, the justice is constrained and, if so, by how much and in which direction. I then use those data in a time-series analysis to determine whether the constraints influence the justices in their statutory decisions.

I measure (revealed) ideological preferences of members of Congress using the support scores provided by the Americans for Democratic Action. While ADA scores have noted deficiencies, most especially the fact that nonvoting congressmen are counted as voting against the ADA position, this should have very little influence on chamber and committee medians. Moreover, recent research demonstrates the reliability, validity, and stability of ADA scores as a measure of congressional ideology (Herrera, Epperlein, and Smith 1995). Finally, unlike NOMINATE scores, ADA scores are available throughout the entire period of study.

Assuming that ADA scores measure preferences on a liberal-conservative dimension, I need a measure of Supreme Court preferences that does the same and is independent of the preferences of Congress. I use two separate indicators: an inferential measure, the Segal-Cover scores (Segal and Cover 1989),[19] and a more direct "constitutional" measure—predicted, annual, liberalism support scores in nonunanimous civil liberties constitutional cases, as derived from the U.S. Supreme Court Judicial Database.[20] The Segal-Cover scores have the advantage of being clearly independent of congressional preferences, but they do contain some amount of measurement error (Epstein and Mershon 1996). Thus, to ensure the robustness of my results, I include my more direct measure, one derived from the justices' votes, but only after purging those scores of any plausible influence from the political environment. I accomplished this as follows.

First, and for obvious reasons, I excluded statutory decisions. While others have argued that the justices' votes in *all* past cases are the best measure of their sincere preferences (Epstein and Mershon 1996), this can only be true if the separation-of-powers argument is false. Thus, the Epstein/Mershon measure is not appropriate for testing separation-of-powers hypotheses. Instead, I chose the votes of the justices in constitutional cases. Overwhelmingly, the averages of these votes should be independent of congressional preferences, even if on rare occasions the justices do defer to congressional preferences (see, e.g., Epstein and Walker [1995] on the *Ex Parte McCardle* [1869] decision and Murphy [1962] on the *Barenblatt* [1959] case). Even Eskridge (1991b, 652) is willing to use votes in constitutional cases, "where there was little chance of being overridden," as a valid baseline against which to measure votes in statutory cases. Second, I chose civil liberties cases because both the House and the Senate Judiciary Committee has jurisdiction over almost all the Court's civil liberties decisions (Segal 1995). While this choice may limit generalizability, it does so over an area that encompasses a majority of the Court's docket.

---

[19] The Segal-Cover scores use newspaper editorials prior to each Court nominee's confirmation to measure the justices' ideological values.

[20] Using citation plus split votes as the unit of analysis, I chose all cases in which the authority the Court gave for its decision was judicial review at the national or state level.

Third, I chose nonunanimous decisions only. I did so to enhance the ability to scale these decisions with the ADA measure of congressional preferences. I discuss the scaling of these measures below. Fourth, I used annual support scores, not aggregates across an entire career. As recently demonstrated, a fair number of justices demonstrate long-term changes in their sincere preferences (Epstein, Hoekstra, Segal, and Spaeth 1995). Fifth, I used predicted not actual annual support scores.[21] This further helps ensure that these votes are independent of short-term contemporary congressional preferences. It also has an added advantage in that it eliminates short-term fluctuations due to changes in case stimuli (Baum 1988).

With these steps, I have what I hope to be reliable and valid measures of the justices' ideological preferences. The overall scores range from a high of 93.3 (Douglas) to a low of 5 (Rehnquist). The next step is to scale them comparably to ADA scores.[22] There is no clear way of doing so, so I sought expert opinion from four highly regarded public law colleagues. The question I asked them was how these scores related, in their judgment, to ADA scores. That is, is 93.3 about where Douglas would be if he had real and comparable ADA scores, or is it too high or low? Is 5 about where Rehnquist would be if he had real and comparable ADA scores, or is it too high or low? The three scholars who answered my query unanimously stated that the scores were better as is than rescaling them higher, lower, more toward the middle, more toward the extremes, or any combination thereof.[23] As this is my view as well, I use the scores as is. While this is obviously not a textbook example of scaling, the results have, I believe, a fair amount of face validity and are certainly far less arbitrary than the placement of players that one sometimes finds in the separation-of-powers literature (see above).

I next determined for each model the set of Pareto optimals that the Court faced for each year, such that decision mapping within that set could not be reversed. I will discuss this procedure individually for each model below. From these I calculated the constraints facing each justice each year. If a justice's predicted preference (or, in the second set of tests, the Segal-Cover inferential preferences) falls within the set of Pareto optimals, the constraint is zero. If a justice's predicted preference falls above the maximum, then his or her constraint is the maximum minus the preference (yielding a negative score). If a justice's predicted preference falls below the minimum, then his or her constraint is the minimum minus the preference (yielding a positive score). These constraint scores form the key independent variable in a set of maximum likelihood time series analyses of the justices' votes.[24] The dependent variables consist of the justices' annual votes in all statutory decisions in civil liberties cases rendered by the Supreme Court between the beginning of the 1946 term and the end of the 1992 term, yielding the equation Stat Decisions$_t$ = $\alpha + \beta_1$ Constraint$_t$ + ε_t.[25] To create a fair test of the model, justices need at least ten terms of service with at least four terms falling outside the set of Pareto optimals for inclusion. I derived the data from the U.S. Supreme Court Judicial Database, as backdated by Harold Spaeth (1994) to cover the Vinson Court era.[26] This selection procedure yielded 1,052 cases, with the Court voting liberally in 48.2% of them.[27]

The Committee-Gatekeeping Model

In this section I present tests of the separation-of-powers model that are most consistent with the specific derivations of the model created by Ferejohn and Shipan (1990) and Gely and Spiller (1990). First, I derive the set of Pareto optimals. For eras with a Republican president, the minimum of the set is the minimum of the 33d percentile House member, the 33d percentile Senate member, the median of the House Judiciary Committee, the median of the Senate Judiciary Committee, the House Judiciary indifference point, and the Senate Judiciary indifference point.[28] The maximum of the set is the maximum of the House median, the Senate median, the median of the House Judiciary Committee, the median of the Senate Judiciary Committee, the House Judiciary indifference point, and the Senate Judiciary indifference point. For eras with a Democratic president, the minimum of the set is the minimum of the median House member, the median Senate member, the median of the House Judiciary Committee, the median of the Senate Judiciary Committee, the House Judiciary indifference point, and the Senate Judiciary indifference point. The

---

[21] I did this with a time-series regression of each justice's liberalism in constitutional cases with a counter (the term of the Court), and then used the predicted values for each justice in each year. I chose the unbiased OLS estimator rather than unbiased and efficient ML estimators because congressional preferences could be among the factors that shape *p*.

[22] Needless to say, my ideological support scores for the justices are not created in a manner similar to ADA scores for congressmen. My goal, however, is not to replicate ADA scores but to come up with scores that measure liberalism for both justices and congressmen. While interest-group support scores for justices would be nice if they existed, following ADA ranking procedures is not essential and even could be harmful to my purposes. For example, counting absentee votes of the justices as conservative would produce an obviously inferior measure of the justices' ideology. Moreover, even if ADA procedures could be followed precisely for the justices, that would not relieve the need to ensure that the votes were similarly scaled.

[23] Thanks are extended to Larry Baum, Greg Caldeira, and Tom Walker.

[24] Because of the threat of autocorrelation, I estimated a first-order autoregressive process throughout the models.

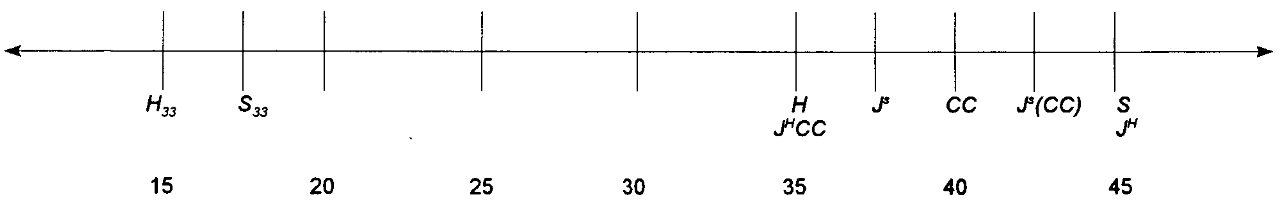
[25] I added controls in subsequent analyses.

[26] See note 16 for derivation of the data. Here, I use all civil liberties cases, not just civil rights cases.

[27] Civil liberties cases are those involving the First Amendment, criminal procedure, equal protection, due process, the right to privacy, and attorneys. Liberal decisions are pro-individual and against the government in First Amendment, criminal procedure, due process, and privacy cases; pro-women or -minorities in equal protection cases; and pro-attorney in attorney cases.

[28] In a bicameral model it is innapropriate to calculate the gatekeeping committee's indifference point by the traditional $2H - G$ formula, as the committee must look forward to the ultimate two-chamber outcome, not the parent chamber outcome. Though the final outcome could obtain anyplace between H and S, I use the midpoint between the two.

FIGURE 4. The Set of Pareto Optimals [15, 45] in Congress, 1982



Legend: H_{33} = House 33d percentile; S_{33} = Senate 33d percentile; H = House median; J^s = Senate Judiciary Committee median; CC = Conference Committee median; $J^s(CC)$ = Senate Judiciary Committee indifference point; S = Senate median; J^HCC = House Judiciary Committee indifference point; J^H = House Judiciary Committee median.

maximum of the set is the maximum of the 67th percentile House member, the 67th percentile Senate member, the median of the House Judiciary Committee, the median of the Senate Judiciary Committee, the House Judiciary indifference point, and the Senate Judiciary indifference point.

An example may illuminate, as seen in Figure 4. In 1982, the median ADA score was 35 for the House and 45 for the Senate. Thus, the gatekeeping committees should look forward to an eventual outcome at 40 (see note 28). The House Judiciary Committee median was 45, making its indifference point 35, because at any point below 35 the committee would prefer the expected conference outcome (40) to the indifference point. Therefore, if the Court's rulings were between 35 and 45, the committee would not introduce legislation. Without presidential veto, Supreme Court decisions below 35 would be reversible in the House. But assuming a Reagan veto of liberal overrides, legislation above 15 would be blocked by the most conservative one-third of the House. The Senate was irrelevant in 1982, as a 33d percentile of 17, a chamber median of 45, a Judiciary median of 37.5, and a Judiciary indifference of 42.5 left the Senate's set of Pareto optimals within the House's set. Thus, the set of Pareto optimals for the House and Senate was all points between 15 and 45.

In 1982, the predicted preferences of White (35.1), Burger (21), Powell (34.7), and O'Connor (15.4) fell within the set of Pareto optimals. These justices should not be affected by congressional preferences. Of the five remaining justices, the predicted preferences of four fell above the maximum (Marshall 93, Brennan 91.1, Stevens 64.3, and Blackmun 50.8), while Rehnquist (5.7) fell below the minimum. Of the 18 justices who served ten or more terms during this period, Powell and White never located outside the set of Pareto optimals, while Burton (1), Frankfurter (3), Stewart (3), and O'Connor (2) rarely did. This leaves 12 justices on whom we can test the committee-gatekeeping model.

The tests involve a series of maximum likelihood time-series regression models of each justice's votes in statutory cases, with the constraint variable as the independent variable. If the justice's predicted preferences exceed the maximum of the set of Pareto optimals, then the justice should be constrained in a conservative direction. If the justice's predicted preferences fall below the minimum of the set of Pareto optimals, then the justice should be constrained in a liberal direction. In either case, the value of the constraint variable should be positive. For example, in 1982, the constraint variable takes on the value zero for White, Burger, Powell, and O'Connor, 9.3 (5.7 − 15.0) for Rehnquist, −48.0 (45.0 − 93.0) for Marshall, −46.1 (45.0 − 91.1) for Brennan, −19.3 (45.0 − 64.3) for Stevens, and −5.8 (45.0 − 50.8) for Blackmun. As these constraints rise and fall for the justices, so, too, should their liberalism scores in statutory cases. Alternatively, if the coefficient in the various justices' models is indistinguishable from zero, then we cannot reject the null hypothesis of no influence.

The first results show little support for the committee-gatekeeping model (see Table 2). Using one-tailed significance tests, no justice shows a significant coefficient at the conventional $p < .05$ level, while Warren could be added if we move to a more lenient .10 level. At the same probability level, however, Blackmun's coefficient shows significance in the negative direction. Moreover, of the twelve justices, only five had coefficients in the expected direction.[29]

In a first attempt to test the robustness of the results, I examine the above model with two basic controls. First, though the separation-of-powers model, like much political research, relies on the stability assumption (i.e., that the preferences of individual justices do not change over time), recent studies question whether that is so (Epstein et al. 1995). If the justices are changing over time, then perhaps the above model's lack of control for that could be attenuating the results. Thus, I add the variable *Term*, a counter for each term of the Court, to control for long-range changes in the justices' preferences. Second, to control for the possibility that changing case types may affect the justices' liberalism score, I control for the direction of the lower court decision.[30] Under these controls, nine of the

---

[29] Table 2 also presents the first-order autoregressive coefficient, ρ. The coefficient is a control for any autocorrelation that may exist in the models.

[30] This reflects the fact that the Supreme Court by and large follows a reversal strategy, collectively taking cases that it wishes to reverse (Segal and Spaeth 1993, chapter 4). Thus, some of the fluctuation in the justices' annual liberalism score could result from the differing mixes of lower court decisions the Court hears each year. Though this is not a perfect measure of case stimuli, the only alternative, Baum's (1988) adjustment scores, would undoubtedly overcorrect for

TABLE 2. Maximum Likelihood Estimates of Committee-Gatekeeping Model

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Clark	.56	(.97)	.28	.26	.35	(1.11)	.39	.33	18
Douglas	−.09	(.21)	.33	.51	.07	(.17)	.36	−.07	29
Reed	−.24	(1.02)	.42	−.37	.09	(1.34)	.48	−.57	11
Black	.10	(.28)	.36	.48	.08	(.35)	.42	.38	25
Harlan	−.37	(1.30)	.39	.08	−.59	(1.42)	.35	−.22	17
Marshall	.16	(.14)	.14	−.21	.15	(.15)	.17	−.21	24
Brennan	−.01	(.13)	.49	−.05	.01	(.14)	.47	−.03	34
Warren	.53	(.36)	.09	.29	.40	(.23)	.06	−.39	16
Burger	−.58	(.58)	.18	.01	−1.16	(.39)	.01	−.54	17
Blackmun	−1.05	(.78)	.10	.12	.27	(1.01)	.40	−.14	22
Rehnquist	−.15	(.31)	.33	.23	−.08	(.32)	.42	.28	21
Stevens	.29	(.44)	.26	.62	.23	(.54)	.34	.64	17

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of independent variable constraint; significance tests are one-tailed; ρ = first-order autoregressive coefficient; cotnrols are for the term of the Court and the aggregate direction of the lower court decisions being considered.

twelve justices have coefficients in the proper direction, but only Warren's coefficient falls below a .10 probability level.[31] Moreover, Burger's coefficient is significant ($p < .01$) in the opposite direction.

While these results do not bode well for the leading conceptualization of the separation-of-powers model, they do not necessarily refute the concept. On the one hand, the committee-gatekeeping model may seriously underestimate the number of places in Congress where legislation can be blocked. On the other hand, the model may place authority in committees, whereas authority actually rests in the majority party caucus.

The Multiple-Veto Model

Based on the earlier discussion, it may be necessary to include the preferences of the House Rules Committee median (R), the House Rules Committee Chair (R_C), the House and Senate Judiciary Committee chairs ($J_C{}^H$, $J_C{}^S$), the Speaker of the House (Sp), and the senate majority leader (SML) in the model. R, R_C, J^H, J^S, $J_C{}^H$, $S_C{}^H$, Sp, and SML should only support remedial legislation if they prefer the expected outcome of the Conference Committee ($CC = [H + S]/2$]) over the Court's position (X_{ct}).[32] Thus, under a Republican president, the set of Pareto optimals is given by [min(H_{33}, S_{33}, J^H, J^S, $J^H(CC)$, $J^S(CC)$, $R(CC)$, $R_C(CC)$, $J_C{}^H(CC)$, $J_C{}^S(CC)$, $Sp(CC)$, $SML(CC)$), max (H, S, J^H, J^S, $J^H(CC)$, $J^S(CC)$, $R_C(CC)$, $J_C{}^H(CC)$, $J_C{}^S(CC)$, $Sp(CC)$, $SML(CC)$))]. Under a Democratic president, the set of Pareto optimals is given by [min(H, S, J^H, J^S, $J^H(CC)$, $J^S(CC)$, $R(CC)$, $R_C(CC)$, $J_C{}^H(CC)$, $J_C{}^S(CC)$, $Sp(CC)$, $SML(CC)$), max(H_{67}, S_{67}, J^H, J^S, $J^H(CC)$, $J^S(CC)$, $R(CC)$, $R_C(CC)$, $J_C{}^H(CC)$, $J_C{}^S(CC)$, $Sp(CC)$, $SML(CC)$))].

One problem in deriving the set of Pareto optimals for this model is that, in any given year, a large number of missed votes can skew the ADA scores for any individual who was ill, running for president, or other such reasons. For example, in 1980, Judiciary Committee Chair Ted Kennedy registered an ADA score of only 33 as he sought delegates to the Democratic convention. A larger measurement problem surrounds the Speaker, who rarely votes, except in the case of a tie. As such, ADA does not provide scores for him. For relevant members other than the Speaker, large numbers of missed votes affect the scores of Rules Chairman Sabath (D-IL) in 1952, House Judiciary Chair Rodino (D-NJ) in 1978, Senate Judiciary Chair Kennedy (D-MA) in 1980, Senate Judiciary Chair Biden (D-DE) in 1988, and Senate Majority Leader Mansfield (D-MT) in 1968. For each of those years I use the person's ADA score from the previous year. For the Speakers, I use their average for the years prior to becoming Speaker.

The multiple-veto model provides a much broader set of irreversible decisions for the Court. In the gatekeeping model, the average range between the minimum and the maximum of the set of Pareto

---

the problem and pick up any influence from the political environment. I used Spaeth's *Lower Court Direction* variable for the set of cases in my sample but reversed the direction for the few cases in which Spaeth (1994) coded directionality based on dissent and not the Court majority. This follows what Spaeth does in his most recent version of the database. Thus, from his older version that I used, I changed the codings of *Lower Court Direction* in the following statutory civil liberties cases: 337 U.S. 86, 341 U.S. 214, 410 U.S. 578, and 483 U.S. 107.

[31] There conceivably could be other variables for which to control, but it is not clear what they would be. Certainly, the separation-of-powers model excludes virtually all other variables on theoretical grounds. The well-known influence of the solicitor general/executive branch is already accounted for, at least indirectly, through the influence of the president in determining the set of Pareto optimals. Adding this influence again through a direct measure would only lessen the likelihood of finding influence in the constraint variable. There is little evidence that any other such variables would be empirically important. No other aggregate-level variables (e.g., public opinion) have been convincingly demonstrated to influence the votes of the justices. Finally, since most of the potentially excluded variables would correlate positively with the political environment, the inclusion of such variables would, if anything, attenuate estimates of the influence of the constraint variable.

[32] While the Rules Committee can enforce a closed rule on the House floor, it cannot enforce one on the Conference Committee. Thus, the Rules Committee cannot guarantee that it can obtain the Judiciary Committee result if it prefers that to the House median.

TABLE 3. Maximum Likelihood Estimates of the Multiple-Veto Model

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Douglas	.26	(.19)	.10	.39	.35	(.13)	.01	−.29	29
Black	.67	(.34)	.03	.61	.97	(.39)	.02	.34	25
Marshall	.01	(.13)	.46	−.15	−.02	(.16)	.45	−.06	21
Brennan	−.06	(.17)	.37	−.06	.06	(.21)	.39	−.03	34
Warren	.59	(.49)	.13	.29	.83	(.33)	.02	.17	16
Rehnquist	.19	(.34)	.30	.18	.24	(.34)	.25	.23	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of the independent variable constraint; significance tests are one-tailed; ρ = first-order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

optimals is 40.7 points. In the multiple-veto model, the range is an extraordinary 70.7 points. Much of this is due to the fact, discussed earlier, that for substantial periods the House and Senate Judiciary Committee chair located at complete opposite ends of the political spectrum.[33] Under this model and the preferences of the relevant players, override of Supreme Court decisions appears extraordinarily difficult.

One obvious result is that most of the justices always locate within the set of Pareto optimals and thus cannot be constrained by Congress. We now add Frankfurter, Burton, Clark, Reed, Harlan, Stewart, Burger, and O'Connor to the list of justices always so located. In addition, Blackmun and Stevens fall outside only three times and thus are not tested. The six who, according to the multiple-veto model, should be constrained are listed in Table 3, which presents the same analysis as Table 2 but for the multiple-veto model. Of the six justices who served ten or more terms and could be constrained, five show coefficients in the correct direction. Black's coefficient is even significant at the .05 level, while Douglas's could be added if we move to a more lenient .10. If we add the control variables to the equation, the coefficient for Warren and Douglas joins that of Black as positively significant. Thus, if the multiple-veto model is correct, justices overwhelmingly fall within the set of Pareto optimals, but two or three justices appear to show constraint when they fall outside the set.

The Party-Caucus Model

Recent models of congressional lawmaking provide theoretical and empirical evidence that policymaking typically represents neither independent committee nor independent leadership preferences but, rather, the preferences of the majority party caucus (Kiewiet and McCubbins 1991, Cox and McCubbins 1993). Under such models, the type of legislation that can come to a vote and be approved by a chamber moves to the left when the chamber switches from Republican control to Democratic control and moves to the right when control passes from Democrats to Republicans.[34] Thus, for example, the takeover of the Senate by Republicans, as in the 1980 election, moves the balance of power in that chamber *and* in its Judiciary Committee to the right.[35]

To test the party-caucus model, I operationalize potential gatekeepers (the judiciary committees and chairs, as well as majority party leaders) as representing the preferences of the median member of the House and Senate majority party caucus (MPC^H and MPC^S). Thus, under a Republican president, the set of Pareto optimals is given by [min(H_{33}, S_{33}, MPC^H, MPC^S, $MPC^H(CC)$, $MPC^S(CC)$), max(H, S, MPC^H, MPC^S, $MPC^H(CC)$, $MPC^S(CC)$)]. Under a Democratic president, the set of Pareto optimals is given by [min(H, S, MPC^H, MPC^S, $MPC^H(CC)$, $MPC^S(CC)$), max(H_{67}, S_{67}, MPC^H, MPC^S, $MPC^H(CC)$, $MPC^S(CC)$)].

Under the party-caucus model, Clark, Burton, Harlan, Stewart, White, Blackmun, and Powell join the list of never-constrained justices, while Reed, Frankfurter, and Stevens fall outside the set of Pareto optimals three times, two times, and one time, respectively. For the remaining seven justices, four have slopes in the negative direction, and only one, Warren, has a significant positive coefficient (see Table 4). Adding the control variables to the equation provides an extra positive (and insignificant) coefficient but also results in a negative significant coefficient for Burger.

Further Tests

Inferential Measures of Attitudes. As a further test of the robustness of the results presented, I replace the direct measure of judicial ideology used above with an inferential measure, the Segal/Cover scores (Segal and Cover 1989) as updated by Segal, Epstein, Cameron, and Spaeth (1995). As these scores range from −1 to +1, I adjust them to the same range as the earlier measure.[36] Tables 5, 6, and 7 present the results for

---

[33] These results are consistent with the fact that when override legislation does pass Congress, it usually does so with an overwhelming margin (Eskridge 1991b). The results are also consistent with Spiller and Gely's (1992) open-rule model.

[34] For a defense of this model as applied to the separation-of-powers games, see Segal (1995).

[35] This assumption looms large when one recalls the Ferejohn-Shipan demonstration that moving the chamber to the right while holding committee preferences constant can, in some circumstances, enable a strategic and liberal court to move to the *left*.

[36] Multiplying the ideology score in Segal et al. (1995) by 44.15 and

TABLE 4. Maximum Likelihood Estimates of the Party-Caucus Model

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Douglas	−.08	(.21)	.36	.52	.07	(.22)	.38	−.07	29
Black	.11	(.25)	.34	.47	.15	(.34)	.33	.34	25
Marshall	.01	(.17)	.48	−.15	.02	(.19)	.47	−.09	21
Brennan	−.09	(.16)	.29	−.04	−.13	(.20)	.26	−.02	34
Warren	.84	(.39)	.03	.20	.85	(.28)	.02	−.39	16
Burger	−.88	(.92)	.18	−.02	−2.04	(.63)	.01	−.66	17
Rehnquist	−.02	(.29)	.48	.25	−.03	(.28)	.46	.30	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of the independent variable constraint; significance tests are one-tailed; ρ = first-order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

TABLE 5. Maximum Likelihood Estimates of the Committee-Gatekeeping Model with Segal-Cover Scores

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Douglas	−.13	(.27)	.32	.53	.00	(.22)	.50	−.05	29
Frankfurter	−.53	(.60)	.19	.52	−.63	(.59)	.31	.42	15
Reed	.18	(.34)	.31	−.38	−.04	(.39)	.46	−.58	11
Black	.24	(.29)	.22	.46	.15	(.30)	.32	.37	25
Harlan	−.15	(.44)	.38	.08	−.24	(.53)	.66	.26	17
Stewart	.24	(.41)	.29	−.11	.46	(.34)	.10	−.44	23
Marshall	.11	(.11)	.16	−.19	.15	(.15)	.17	−.17	24
Brennan	.05	(.14)	.37	−.06	.00	(.14)	.49	−.03	34
White	−.49	(1.46)	.37	.14	−.65	(.79)	.21	−.49	30
Warren	.81	(.35)	.02	.31	.56	(.27)	.03	−.40	22
Burger	−.65	(.44)	.09	−.12	−.73	(.37)	.04	−.45	21
Blackmun	.35	(.56)	.28	.29	−.55	(.44)	.12	−.33	22
Powell	.32	(.67)	.33	.11	.25	(.69)	.36	.08	16
Rehnquist	−.18	(.33)	.31	.22	−.12	(.33)	.37	.27	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of the independent variable constraint; significance tests are one-tailed; ρ = first-order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

those justices who served ten or more terms and whose inferential preferences fall outside the set of Pareto optimals four or more times. The separation-of-powers model fares no better. In the committee-gatekeeping model, Warren's coefficient alone among the fourteen justices tested is significant, while Burger's is significant in the negative direction when I control for lower court direction and term. Similar to the original tests, Douglas, Black, and Warren have significant coefficients in the multiple-veto model, but Harlan's coefficient is significant in the opposite direction. Finally, in the party-caucus model, the coefficient for Warren and Powell is significantly positive, while Stewart's is significantly negative. With the control variables, the coefficient for Powell is no longer significant, while Burger's joins Stewart's as significantly negative. Under the Segal-Cover scores, the results are almost precisely the same as before. It appears as if Earl Warren, alone among the justices, is playing the separation-of-powers game.

Pooled Results. In order to test directly the relative efficacy of the attitudinal model and separation-of-powers model, I created a pooled cross-sectional time-series data set of each justice from Black through Souter and each year from 1947 through 1992. Because the runs here include all the justices, not just one at a time, I can directly add both the constraint predictions from the separation-of-powers model and the attitudinal predictions using the Segal-Cover scores. This also allows me to include justices who previously had too few terms for inclusion and to increase the n to 412. As above, I use both constitutional cases and the Segal-Cover scores to calculate constraints. I test these models using least-squares dummy variable (LSDV) regression. This corrects for autocorrelation by adding a dummy variable for each justice but one and for each year but one.[37] I present the results in Table 8.[38]

---

adding a constant of 49.15 produces scores with a minimum of 5 and a maximum of 93.3, the same as the earlier scores, but with different values for each justice.

[37] Because the constant plus all but one of the justice dummies perfectly predicts the ideology scores, it is necessary to drop two justice dummies. I drop the first two alphabetically, Black and Blackmun.

[38] Annual controls such as *Lower Court Direction*, or any other variable, are unnecessary, as the annual dummies pick up all

TABLE 6. Maximum Likelihood Estimates of the Multiple-Veto Model with Segal-Cover Scores

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Douglas	1.18	(.43)	.01	.25	.89	(.40)	.02	−.12	29
Frankfurter	−1.25	(1.07)	.14	.49	−1.55	(1.19)	.12	.30	15
Reed	.91	(.57)	.08	−.37	.73	(.57)	.13	−.51	11
Black	.83	(.31)	.01	.51	.87	(.31)	.01	.39	25
Harlan	−1.06	(.47)	.03	−.17	−1.00	(.52)	.04	−.20	17
Stewart	−.37	(.97)	.36	−.03	−.32	(.97)	.38	−.24	23
Marshall	.03	(.13)	.42	−.16	−.03	(.15)	.43	−.05	24
Brennan	−.04	(.15)	.40	−.05	.02	(.15)	.44	−.04	34
Warren	1.80	(.55)	.01	−.16	1.54	(.40)	.01	−.52	16
Blackmun	.88	(1.15)	.23	.14	.21	(1.15)	.44	−.16	22
Rehnquist	.26	(.38)	.26	.19	.35	(.39)	.40	.25	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of the independent variable constraint; significance tests are one-tailed; ρ = first-order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

TABLE 7. Maximum Likelihood Estimates of the Party-Caucus Model with Segal-Cover Scores

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Douglas	−.17	(.26)	.26	.57	−.14	(.30)	.33	−.00	29
Frankfurter	.35	(.56)	.28	.65	.26	(.64)	.35	.58	15
Reed	−.17	(.44)	.36	−.34	−.63	(.47)	.12	−.69	11
Black	.06	(.27)	.42	.46	.03	(.30)	.47	.38	25
Harlan	−.35	(.67)	.61	.09	−.41	(.70)	.29	.26	17
Stewart	−1.40	(.59)	.02	.26	−1.60	(.61)	.01	.11	23
Marshall	.03	(.14)	.42	−.17	.02	(.19)	.47	−.09	24
Brennan	−.03	(.19)	.45	−.05	−.14	(.19)	.25	−.02	34
Warren	1.09	(.36)	.01	.10	1.00	(.36)	.01	−.43	16
Burger	−.62	(.67)	.19	−.05	−1.31	(.49)	.02	−.59	17
Blackmun	.80	(.62)	.11	.31	−.08	(.62)	.45	−.17	22
Powell	1.84	(1.00)	.05	.36	1.52	(1.12)	.11	.28	16
Rehnquist	−.07	(.31)	.42	.25	−.09	(.30)	.39	.29	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of the independent variable constraint; significance tests are one-tailed; ρ = first-order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

As can readily be seen, the ideology variable massively outperforms the constraint variable. The ideology coefficient ranges from .27 to .35. In all six models the ideology coefficient is in the right direction, substantively strong, and statistically significant at any plausible level. Alternatively, the constraint coefficient is negative in three of the six models. It ranges from −.20 to .20, with an average value of .008. The coefficient attains significance in one model, but it is almost significant in the negative direction in another. In the *strongest* results for the separation-of-powers model, the standardized beta for the ideology variable is more than five times larger. In the remaining models in which the constraint coefficient is in the proper direction, the standardized beta for the attitudinal variable has nine to eleven times the explanatory power of the contraint variables, respectively.[39]

Median Voters. As a final test of the separation-of-powers model, I examine a crucial subset of justices: those who occupy the median position on the Court in any given term. They alone may have the capability to keep the Court's preferences consistent with those of Congress. The attitudinal position, however, is that these justices, more than any others, may frequently fall inside the set of Pareto optimals and thus can only infrequently be constrained. And even if they fall outside the set, factors such as the ability to manipulate issues, the ability to review congessional overrides, and the high cost of passing legislation can still lead to rationally sincere voting. The results are as follows.

First, the median justices frequently fall within the set of Pareto optimals. I begin by using the constitutional scores to identify the median justice for each

---

between-year variation. Adding any annual variable, and then dropping one of the annual dummies to prevent complete multicollinearity, has no effect on the results.

[39] While the statistically significant result in the first multiple-veto model must be acknowledged, those wishing to push these results over all others must recall that if the multiple-veto model is correct, the justices are almost never constrained. In the pooled model, 73% of the justices (by year) face no constraints at all. Only three times in 46 years is the median justice constrained (see below). It is rather ironic that the only model that shows any potential effect at all is the one in which the justices are almost never constrained.

TABLE 8. LSDV Estimates of Pooled Models

	Constitutional Preference Measure			Segal-Cover Preference Measure		
Estimates	Committee-Gatekeeping Model	Multiple-Veto Model	Party-Caucus Model	Committee-Gatekeeping Model	Multiple-Veto Model	Party-Caucus Model
Constraint						
β	−.06	.20	.07	−.07	.11	−.20
S.E. β	(.09)	(.10)	(.11)	(.13)	(.14)	(.13)
p	.27	.03	.27	.29	.20	.07
Std. β	−.04	.09	.04	−.05	.05	−.11
Ideology						
β	.31	.35	.33	.29	.35	.27
S.E. β	(.06)	(.06)	(.06)	(.08)	(.06)	(.07)
p	.0001	.0001	.0001	.0002	.0001	.0001
Std. β	.40	.46	.43	.38	.45	.35

Note: $n = 412$ for all models. Dummy variable coefficients will be provided by author on request.

term of the Court.[40] Then, using the constitutional measure of the justices' values, the median justice falls ouside the set of Pareto optimals 6 of 46 times in the committee-gatekeeping model, and only 3 of 46 times in the multiple-veto and party-caucus models. Alternatively, using the Segal-Cover measure of the justices' values, the median justice falls outside the set of Pareto optimals 28, 9, and 20 times, respectively.

Second, when the median justices fall outside the set of Pareto optimals, they evidence little constraint. I demonstrate this by rerunning the LSDV models with an interaction term that takes on the value of the constraint score for the median justice and the value of zero for all other justices. I present the results in Table 9, where the variable *Median* represents the median interaction term. Needless to say, the coefficients for the multiple-veto and party-caucus models using the constitutional scores are largely irrelevant, as the median justices in those models are almost never constrained. Overall, however, in none of the six runs is the interaction coefficient for the median justice significant. The average standardized beta is .018, which is once again dwarfed by an average standardized beta of .413 for the ideology variable.[41]

DISCUSSION AND CONCLUSION

While these results should at least give pause to those certain about the separation-of-powers model as applied to the U.S. Supreme Court, the generic model is not necessarily invalid.[42] The federal judiciary was designed to be independent, so we should not be surprised that in fact it is. Courts in diverse institutional settings, however, may evidence precisely the type of behavior predicted by the models. One can readily imagine a spectrum along which the separation-of-powers model may be relatively true (a parliamentary system with strong party control over members and a single majority party; a judiciary without constitutional powers; judges without life tenure) or relatively false (a congressional system with decentralized power; a constitutional court; judges with life tenure). In sum, we should hardly be surprised if institutional structures matter. Comparative studies of other nations or U.S. state courts, whose institutional features vary dramatically, could prove consequential (Brace and Hall 1995).

As for the U.S. Supreme Court, the theoretical evidence combined with the empirical results cast serious doubt on whether the justices vote other than sincerely with regard to congressional preferences, except on the rarest of occasions. This does not mean, however, that the justices are irrationally myopic. If the multiple-veto model is correct, then there is nothing irrational about the Court virtually always voting sincerely, as there will almost always be at least one person in the lawmaking coalition who would and could block legislative overrides. But even if the committee-gatekeeping or party-caucus model is a more accurate representation of the lawmaking process, and justices are often outside the set of Pareto optimals, sincere voting by the justices still may be rational if there are high opportunity and/or transaction costs to passing legislation, if the Court can bundle issues, if the Court can move into constitutional mode, if the Court can respond to congressional decisions, or if the Court is uncertain about congressional preferences. The facts that the justices do not appear to change as the political environment changes and that, nevertheless, Congress overturns relatively few Court decisions (Eskridge 1991a, 365) suggest that some combination of these factors must be at work. So, too, evidence of strategic behavior at other stages of the Court's decisions (e.g., Boucher and Segal 1995, Brenner 1979, Epstein and Knight 1995, Murphy 1964) suggests that the justices

---

[40] Epstein and Mershon (1996) find that the Segal-Cover scores are inappropriate for locating median justices.

[41] Tests on another subset, the chief justices, show that while Warren appears constrained, none of the other chiefs do. I will provide full results on request.

[42] Though this paper does not test areas other than civil liberties, evidence for economics cases based on the party-caucus model is no better (Segal 1995). Moreover, it is possible that sophisticated actions vis-à-vis Congress may show up in majority opinions even if they do not show up in voting patterns. Although this could happen, evidence demonstrating that votes and policy positions are closely linked (Wenzel 1995, Epstein and Knight 1995) makes this unlikely to occur in any sort of systematic fashion.

TABLE 9. LSDV Estimates of Pooled Models with Median Interaction

	Constitutional Preference Measure			Segal-Cover Preference Measure		
Estimates	Committee-Gatekeeping Model	Multiple-Veto Model	Party-Caucus Model	Committee-Gatekeeping Model	Multiple-Veto Model	Party-Caucus Model
Constraint						
β	−.05	.21	.07	−.08	.13	−.20
S.E. β	(.09)	(.10)	(.11)	(.13)	(.14)	(.13)
p	.30	.02	.26	.26	.17	.07
Std. β	−.04	.09	.04	−.06	.06	−.11
Median						
β	.35	1.22	.47	.14	−.21	.10
S.E. β	(.34)	(.91)	(.50)	(.18)	(.23)	(.20)
p	.16	.09	.18	.14	.18	.32
Std. β	.03	.04	.03	.03	−.03	.01
Ideology						
β	.31	.35	.33	.30	.35	.27
S.E. β	(.06)	(.06)	(.06)	(.08)	(.06)	(.07)
p	.0001	.0001	.0001	.0002	.0001	.0001
Std. β	.41	.45	.43	.39	.45	.35

Notes: n = 412 for all models. Dummy variable coefficients will be provided by the author on request.

can act in a sophisticated fashion *when they need to* do so. But the institutional protections granted the Court mean that with respect to Congress and the presidency, they almost never need to do so.

REFERENCES

Barenblatt v. *United States*, 360 U.S. 109 (1959).

Baum, Lawrence. 1988. "Measuring Policy Change in the U.S. Supreme Court." *American Political Science Review*, 82 (September):905–12.

Baum, Lawrence. 1994. *The Supreme Court*. Washington, DC: CQ Press.

Baum, Lawrence. 1995. "What Motivates Supreme Court Justices? Assessing the Evidence on Justices' Goals." Paper presented at the annual meeting of the Midwest Political Science Association, Chicago.

Boucher, Robert, and Jeffrey A. Segal. 1995. "Supreme Court Justices as Strategic Decision Makers: Offensive Grants and Defensive Denials on the Vinson Court." *Journal of Politics* 57 (August):824–37.

Brace, Paul, and Melinda Gann Hall. 1990. "Neo-institutionalism and Dissent in State Supreme Courts." *Journal of Politics* 52 (February):54–70.

Brace, Paul, and Melinda Gann Hall. 1995. "Studying Courts Comparatively: The View from the American States." *Political Research Quarterly* 48 (March):5–29.

Brenner, Saul. 1979. "The New Certiorari Game." *Journal of Politics* 41 (May):649–55.

Caldeira, Gregory, and John Wright. 1988. "Organized Interests and Agenda Setting in the U.S. Supreme Court." *American Political Science Review* 82 (December):1109–28.

Cameron, Charles M. 1993. "New Avenues for Modeling Judicial Politics." Paper presented at the 2d Annual Conference of the W. Allen Wallis Institute of Political Economy, University of Rochester, Rochester.

Cameron, Charles M., Albert D. Cover, and Jeffrey A. Segal. 1990. "Supreme Court Nominations and the Rational Presidency." Paper presented at the annual meeting of the American Political Science Association, San Francisco.

Congressional Quarterly Almanac. Various years. Washington, DC: Congressional Quarterly News Features.

Cox, Gary W., and Matthew D. McCubbins. 1993. *Legislative Leviathan: Party Government in the House*. Berkeley: University of California Press.

Dahl, Robert. 1957. "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker." *Journal of Public Law* 6 (Fall):179–295.

Enelow, James M. 1981. "Saving Amendments, Killer Amendments, and an Expected Utility Theory of Sophisticated Voting." *Journal of Politics* 43 (November):1062–89.

Epstein, Lee, Valerie Hoekstra, Jeffrey A. Segal, and Harold J. Spaeth. 1995. "Do Sincere Political Preferences Change? A Longitudinal Study of U.S. Supreme Court Justices." Paper presented at the annual meeting of the Midwest Political Science Association, Chicago.

Epstein, Lee, and Jack Knight. 1995. "Positive Approaches to Supreme Court Decision Making." Paper presented at the annual meeting of the American Political Science Association, Chicago.

Epstein, Lee, and Carol Mershon. 1996. "Measuring Political Preferences." *American Journal of Political Science* 40 (February):261–94.

Epstein, Lee, and Thomas G. Walker. 1995. "The Role of the Supreme Court in American Society: Playing the Reconstruction Game." In *Contemplating Courts*, ed. Lee Epstein. Washington, DC: CQ Press.

Epstein, Lee, Thomas G. Walker, and William J. Dixon. 1989. "The Supreme Court and Criminal Justice Disputes: A Neoinstitutional Perspective." *American Journal of Political Science* 33 (November): 825–41.

Eskridge, William N., Jr. 1991a. "Overriding Supreme Court Statutory Interpretation Decisions." *Yale Law Journal* 101 (November): 331–455.

Eskridge, William N., Jr. 1991b. "Reneging on History? Playing the Court/Congress/President Civil Rights Game." *California Law Review* 79 (May):613–84.

Ex Parte McCardle, 7 Wallace 506 (1869).

Ferejohn, John, and Charles Shipan. 1990. "Congressional Influence on Bureaucracy." *Journal of Law, Economics, & Organization* 6 (Special Issue):1–20.

Ferejohn, John, and Barry Weingast. 1992. "A Positive Theory of Statutory Interpretation." *International Review of Law and Economics* 12 (June):263–79.

Fiorina, Morris P. 1975. "Formal Models in Political Science." *American Journal of Political Science* 19 (February):133–59.

Gely, Rafael, and Pablo T. Spiller. 1990. "A Rational Choice Theory of Supreme Court Decision Making with Applications to the *State Farm* and *Grove City* Cases." *Journal of Law, Economics, & Organization* 6 (Fall):263–300.

George, Tracey E., and Lee Epstein. 1992. "On the Nature of Supreme Court Decision Making." *American Political Science Review* 86 (June):323–37.

Grove City College v. *Bell* 465 U.S. 555 (1984).

Hagle, Timothy M. 1992. "But Do They Have to See It to Know It: The Supreme Court's Obscenity and Pornography Decisions." *Western Political Quarterly* 45 (December):1039–54.

Hall, Melinda Gann. 1992. "Electoral Politics and Strategic Voting in State Supreme Courts." *Journal of Politics* 54 (May):427–46.

Herrera, Richard, Thomas Eperlein, and Eric R. A. N. Smith. 1995. "The Stability of Congressional Roll-Call Indexes." *Political Research Quarterly* 48 (June):403–16.

Ignagni, Joseph. 1994. "Explaining and Predicting Supreme Court Decision Making: The Burger Court's Establishment Clause Decisions." *Journal of Church and State* 36 (Spring):301–27.

Kiewiet, D. Roderick, and Matthew D. McCubbins. 1991. *The Logic of Delegation: Congressional Parties and the Appropriations Process*. Chicago: University of Chicago Press.

Knight, Jack, and Lee Epstein. 1996. "On the Struggle for Judicial Supremacy." *Law and Society Review* 30 (1):87–120.

Krehbiel, Keith. 1986. "Unanimous Consent Agreements: Going Along in the Senate." *Journal of Politics* 48 (August):541–64.

Krehbiel, Keith, and Douglas Rivers. 1990. "Sophisticated Voting in Congress: A Reconsideration." *Journal of Politics* 52 (August): 548–78.

Lampf v. *Gilbertson* 501 U.S. 350 (1991).

Lemieux, Peter H, and Charles H. Stewart III. 1988. "Advise? Yes. Consent? Maybe. Senate Confirmation of Supreme Court Nominations." Paper presented at the annual meeting of the American Political Science Association, Washington, D.C.

Marks, Brian A. 1988. "A Model of Judicial Influence on Congressional Policymaking: *Grove City College* v. *Bell*." Working papers in Political Science, P-88-7, The Hoover Institution, Stanford University.

Marks, Brian A. 1989. "A Model of Judicial Influence on Congressional Policymaking: *Grove City College* v. *Bell*." Ph.D. diss., Washington University.

Matthews, Donald R. 1960. *U.S. Senators and Their World*. Chapel Hill: University of North Carolina Press.

McGuire, Kevin. 1990. "Obscenity, Libertarian Values, and Decision Making in the Supreme Court." *American Politics Quarteryly* 18 (January):47.

McNollgast [Matthew D. McCubbins, Roger G. Noll, Barry R. Weingast]. 1990. "Positive and Normative Models of Procedural Rights: An Integrative Approach to Administrative Procedures." *Journal of Law, Economics, & Organization* 6 (Special Issue):307–32.

McNollgast [Matthew D. McCubbins, Roger G. Noll, Barry R. Weingast]. 1992. "Positive Canons: The Role of Legislative Bargains in Statutory Interpretation." *Georgetown Law Journal* 80 (February):705–42.

Murphy, Walter F. 1962. *Congress and the Court*. Chicago: University of Chicago Press.

Murphy, Walter F. 1964. *Elements of Judicial Strategy*. Chicago: University of Chicago Press.

Nelson, Garrison. 1993. *Committees in the U.S. Congress, 1947–92.*. Washington, DC: CQ Press.

O'Connor, Karen. 1983. "The Amicus Curiae Role of the U.S. Solicitor General in Supreme Court Litigation." *Judicature* 66 (December–January):256–64.

Oleszek, Walter J. 1989. *Congressional Procedures and Policy Process*. 3d ed. Washington, DC: CQ Press.

Plaut v. *Spendthrift Farm*, 93–1121 (1995).

Puro, Steven. 1981. "The United States as Amicus Curiae." In *Courts, Law and Judicial Processes*, ed. S. Sidney Ulmer. New York: Free Press.

Regents v. *Bakke*, 438 U.S. 265 (1978).

Riker, William H. 1984. "The Heresthetics of Constitution-Making: The Presidency in 1787, with Comments on Determinism and Rational Choice." *American Political Science Review* 78 (March): 1–16.

Rodriguez, Daniel B. 1994. "The Positive Political Dimensions of Regulatory Reform." *Washington University Law Quarterly* 72 (1):1–150.

Rohde, David, and Harold Spaeth. 1976. *Supreme Court Decision Making*. San Francisco: W. H. Freeman.

Rosenberg, Gerald. 1994. "Symposium: The Supreme Court and the Attitudinal Model." *Law and Courts* 4 (Spring):6–8.

Rosenberger v. *University of Virginia* 94–329 (1995).

Rutten, Andrew. 1993. "Accountability, Separation of Powers and the Judiciary: Where Are We in the Positive Theory of Courts?" Paper presented at the 2d annual conference of the W. Allen Institute of Political Economy, University of Rochester, Rochester.

Schubert, Glendon. 1959. *Quantitative Analysis of Judicial Behavior*. Glencoe, IL: The Free Press.

Schubert, Glendon. 1965. *The Judicial Mind: The Attitudes and Ideologies of Supreme Court Justices, 1946–1963*. Evanston: Northwestern University Press.

Schwartz, Edward P. 1992. "Policy, Precedent, and Power: A Positive Theory of Supreme Court Decision Making." *Journal of Law, Economics, & Organization* 8 (April):219–52.

Schwartz, Edward P., Pablo T. Spiller, and Santiago Urbiztondo. 1994. "A Positive Theory of Legislative Intent." *Law and Contemporary Problems* 57 (Winter):51–74.

Scigliano, Robert. 1971. *The Supreme Court and the Presidency*. New York: The Free Press.

Segal, Jeffrey A. 1984. "Predicting Supreme Court Decisions Probabilistically: The Search and Seizure Cases 1962–1981." *American Political Science Review* 78 (September):891–900.

Segal, Jeffrey A. 1988. "Amicus Curiae Briefs by the Solicitor General during the Warren and Burger Courts." *Western Political Quarterly* 41 (March):135–44.

Segal, Jeffrey A. 1995. "Marksist (and NeoMarksist) Models of Supreme Court Decision Making: Separation of Powers Games in the Positive Theory of Law and Courts." Paper presented at the annual meeting of the American Political Science Association, Chicago.

Segal, Jeffrey A., Charles M. Cameron, and Albert D. Cover. 1992. "A Spatial Model of Roll Call Voting: Senators, Constituents, Presidents and Interest Groups in Supreme Court Confirmations." *American Journal of Political Science* 36 (February):96–121.

Segal, Jeffrey A., and Albert D. Cover. 1989. "Ideological Values and the Votes of U.S. Supreme Court Justices." *American Political Science Review* 83 (June):557–65.

Segal, Jeffrey A., Lee Epstein, Charles M. Cameron, and Harold J. Spaeth. 1995. "Ideological Values and the Votes of U.S. Supreme Court Justices Revisited." *Journal of Politics* 57 (August):812–23.

Segal, Jeffrey A., and Cheryl D. Reedy. 1988. "The Supreme Court and Sex Discrimination: The Role of the Solicitor General." *Western Political Quarterly* 41 (September):553–68.

Segal, Jeffrey A., and Harold J. Spaeth. 1993. *The Supreme Court and the Attitudinal Model*. New York: Cambridge.

Segal, Jeffrey A., and Harold J. Spaeth. 1994. "Symposium: The Supreme Court and the Attitudinal Model." *Law and Courts* 4 (Spring):10–2.

Sharp, Michael J. 1988. *The Directory of Congressional Voting Scores and Interest Group Ratings*. New York: Facts on File Publications.

Shelley v. *Kraemer*, 334 U.S. 1 (1948).

Smith, Rogers. 1988. "Political Jurisprudence, the 'New Institutionalism,' and the Future of Public Law." *American Political Science Review* 82 (March):89–108.

Songer, Donald R., Jeffrey A. Segal, and Charles M. Cameron. 1994. "The Hierarchy of Justice: Testing a Principal-Agent Model of Supreme Court-Circuit Court Interactions." *American Journal of Political Science* 38 (August):673–96.

Spaeth, Harold. 1994. U.S. Supreme Court Judicial Database, 1953–1992 Terms. Ann Arbor, MI: ICPSR Study #9422.

Spiller, Pablo T., and Rafael Gely. 1992. "Congressional Control or Judicial Independence: The Determinants of U.S. Supreme Court Labor-Relations Decisions, 1949–1988." *RAND Journal of Economics* 23 (Winter):463–92.

Spiller, Pablo T., and Matthew L. Spitzer. 1992. "Judicial Choice of Legal Doctrines." *Journal of Law, Economics, & Organization* 8 (March):8–46.

Spiller, Pablo T., and Emerson Tiller. 1993. "Invitations to Override: Congressional Reversals of Supreme Court Decisions." University of California, Berkeley. Typescript.

United Air Lines v. *McMann* 434 U.S. 192 (1977).

United Steelworkers v. *Weber*, 443 U.S. 193 (1979).

Wenzel, James P. 1995. "Stability and Change in the Ideological Values in Supreme Court Decisions." Paper presented at the annual meeting of the Midwest Political Science Association, Chicago.

Wolpert, Robin. 1991. "Explaining and Predicting Supreme Court Decision Making: The Gender Discrimination Cases, 1971–1987." Paper presented at the annual meeting of the Midwest Political Science Association, Chicago.

American Political Science Review Vol. 91, No. 1 March 1997

Politics Matters: Political Events as Catalysts for Preadult Socialization

DAVID O. SEARS and NICHOLAS A. VALENTINO
University of California, Los Angeles

We propose that (1) the preadult socialization of longstanding, stable predispositions is catalyzed by exogenous political events; (2) such events socialize attitudes selectively, only in the specific domains they make salient; and so (3) longstanding predispositions tend to be socialized episodically rather than incrementally. This theory is applied to the socialization of partisanship during a presidential campaign, examining gains in information, affective expression, and attitude crystallization. Adolescents (aged 10 to 17) and their parents were interviewed in a three-wave panel study, at the beginning of a presidential campaign, at the end, and a year later. The campaign induced substantial preadult socialization gains regarding attitude objects central to the campaign (candidates and parties), particularly in the stability of preadults' partisanship. There were few gains in attitude domains peripheral to the campaign or during the postcampaign period. These findings suggest that periodic political events catalyze preadult socialization, generating predispositions that persist into later life stages.

Contemporary theory about mass political behavior has been marked by a continuing debate between those who, at some very general level, describe homo politicus as making political choices "rationally" on the basis of available information and those who emphasize a wide variety of distorting psychological influences. One arena of continuing empirical clash between these perspectives focuses on the long-term stability of basic political predispositions. At the extremes, a "lifelong openness" view, which holds that basic attitudes are always susceptible to change given compelling evidence, comfortably fits the standard of individuals rationally responding to current realities. In contrast, a "persistence" account holds that basic attitudes are acquired early and persist throughout life. This view depicts adults' attitudes as anachronistic, indifferent to the realities of the contemporary environment, and an obstacle to "rational" decision making based on the merits of current alternatives.[1]

THE DEBATES ABOUT PARTY IDENTIFICATION

Research stimulated by these contrasting views has for several decades centered on Americans' party identifications. There is general agreement that party identification is a powerful political predisposition and normally is quite stable throughout life (though with increasing recognition of perturbations; e.g., Fiorina 1996, Miller and Shanks 1996). But there is considerable debate about its origins and persistence. Both Hyman's (1959) assessment of political socialization and the Michigan school's dominant account of voting behavior, *The American Voter* (Campbell et al. 1960), conclude that party identification is typically acquired early in life and is highly stable thereafter. Yet, some of their contemporaries championed the lifelong openness view, with both Downs (1957) and Key (1966) believing adults are quite responsive to current political realities. In the years since, these alternative visions have led to debate on four points.

First, are preadults' political opinions really meaningful? It was easy enough to demonstrate that many children would express political opinions and, indeed, nearly achieve adult levels in partisan opinionation by early adolescence (Hess and Torney 1967, Sears 1975). But it was risky to infer just from this early willingness to express opinions that preadult socialization left highly stable, and powerful, attitudes well into adulthood. The lack of direct evidence for this inference precipitated a backlash, the argument being that preadult political opinions merely reflected inconsequential and transitory "nonattitudes" (Marsh 1971; Searing, Schwartz, and Lind 1973; see Sears 1989 for a review; Vaillancourt 1973).[2]

Much of the early work assumed that socialization proceeded incrementally with age as experience and cognitive readiness increased, consistent with both learning and developmental theories. So a second issue concerns the stage of life at which young people's attitudes finally reach adult levels of strength. One view was that partisanship does not change much after early adolescence (Hess and Torney 1967), while others

---

David O. Sears is Professor of Psychology and Political Science, and Nicholas A. Valentino is a doctoral candidate in the Department of Political Science, University of California, Los Angeles, Los Angeles, CA 90095-1563.

The research reported here was supported by National Science Foundation grant SES-791343522 and the preparation of the paper, in part, by grant SES-9022192, while the first author was a Fellow at the Center for Advanced Study in the Behavioral Sciences. We express our deepest appreciation to Steven H. Chaffee and Jack Dennis, who shared responsibility for the data collection with the first author, to Rick Kosterman and J. P. Weber for help with the data analysis, and to the anonymous referees for their careful responses. An earlier version of the paper was presented at the 1993 annual meeting of the American Political Science Association.

[1] Even highly stable predispositions can be interpreted as "rational," as in the arguments made for longstanding ideologies that further group interests. This contention usually rests, however, on assuming a nonrational acquisition process, such as the "ideology by proxy" argument in Campbell et al. (1960).

[2] Perhaps the negative evidence, too, was not as overpowering as originally suggested. Vaillancourt's tau-beta coefficient for party identification, over two months for 9- to 15-year-olds, was a fairly respectable .62, and Sears (1975) reported similar levels of stability in children of similar age.

suggested that socialization of party identification is largely complete by the time preadults leave the parental nest, with the individual's "first vote" seeming to mark real crystallization of political preferences (Campbell et al. 1960, Hyman 1959). Both positions are consistent with a persistence viewpoint. Later longitudinal data, however, suggested that partisanship continues to crystallize through the early adult years, not reaching adult levels of stability until the mid- to late twenties (Alwin, Cohen, and Newcomb 1991; Jennings and Markus 1984; Jennings and Niemi 1981). Similarly, the "dealigning" period of the late 1960s and 1970s had its major effect on young people entering the electorate, not on older voters (Miller and Shanks 1996), as would be expected from an "impressionable years" account (Sears 1975).

A third issue concerns persistence itself: Does party identification indeed persist with little further change throughout life, stoutly resisting the vagaries of the parties' political fortunes? The initial view of party identification as an early socialized "unmoved mover" in *The American Voter* was empirically based on the rather fragile foundation of voters' retrospective accounts, though later bolstered by data from several panel studies (Alwin, Cohen, and Newcomb 1991, Campbell et al. 1960, Converse and Markus 1979, Jennings and Markus 1984, Sears and Funk 1996). Later modifications of this view left the door open for occasional historical shifts in less "steady-state" periods than the 1950s, and consequent realignments of some social groups (such as better-educated conservative southern Democrats), but continued to argue that party identification generally is a highly stable, powerful predisposition (Converse 1969, Miller and Shanks 1996).

This view came under a series of revisionist attacks contending that individuals' party identification is continually and "rationally" adjusted through adulthood in response to such aspects of current political life as poor government performance (Fiorina 1981), newly emerging issues (Franklin 1984, Markus 1979, Niemi and Jennings 1991), political campaigns (Allsop and Weisberg 1988, Brody and Rothenberg 1988), macroeconomic changes (Markus 1992), or the specific office being contested (Fiorina 1996). The "impressionable years" account, too, left room for a rational response to current issues in early adulthood (see Beck and Jennings 1991, Markus 1979, Niemi and Jennings 1991). In return, counterrevisionists argued that individual party identification is normally extremely stable over time, once adjusted for measurement error, and that in any case the amount of year-to-year change is very small in absolute terms (Green and Palmquist 1994, Miller 1991).

Fourth, does preadult party identification originate in a largely apolitical process of family socialization, or does it reflect current political realities? The early literature centered on psychological rather than political processes, as in the ideas that preadults personalize authority or blindly imitate parental attitudes. The revisionists argued instead that even young adults' party identification reflects thoughtful consideration of current political life; for example, in the years after they leave the parental nest, their party identification becomes increasingly tied to their preferences on current issues, even at the expense of some defection from the parental party (Beck and Jennings 1991, Markus 1979, Niemi and Jennings 1991).

These debates leave us with several open questions: Are preadults' attitudes meaningful? When is socialization complete? How persistent are its residues? Is the initial socialization process as insulated from the external political environment as once thought? One goal of this paper is to expand our view of the origins of party identification by both extending the early work on political socialization and incorporating the revisionist insight that the process is more responsive to the external political environment than originally depicted. Specifically, we propose that preadult socialization often does leave as its residue strong, stable attitudes. But real political events have a central role even in this initial socialization process.

Understanding the origins of Americans' party identification is of unquestioned importance for understanding voting behavior in the United States. But party identification is just one of a large number of predispositions that have important political effects. Racial prejudices, nationalism or ethnic rivalries, loyalty to highly visible leaders, or attitudes toward the symbols of the state can also be crucial elements in political life. The extensive literature on the origins of Americans' party identification therefore can provide a useful case study for the development of a more general theory about the origins of sociopolitical predispositions (Sears 1983, 1993). Our second goal here is to contribute to that more general theory.

THE ROLE OF POLITICAL EVENTS

To assess the role of political events in socializing enduring predispositions, we first need a criterion for successful political socialization. In our view, individuals should be regarded as well socialized if they have well-informed, crystallized attitudes toward the important political objects of the day. The mere fact that a child expresses a strong opinion does not mean that a fully crystallized, "real" attitude underlies it. Much research indicates that both children and adults readily express political opinions, but these are often based on poor information and reflect somewhat inconsistent and unstable underlying attitudes. This, of course, leads to the concern that such freely expressed opinions are mere "nonattitudes" (Converse 1970, Vaillancourt 1973). So, at a conceptual level, our criterion for successful political socialization involves holding attitudes at approximately adult levels of affective expression, informational base, and crystallization. Merely expressing clear opinions is necessary but insufficient. Rather, adult levels of information, attitude constraint, and attitude stability must be present as well (Converse 1970, Searing, Schwartz, and Lind 1973, Vaillancourt 1973). And operationally, "socialization gains" would be reflected in increases in these dimensions of sociopolitical attitudes.[3]

Given our focus on the socialization of enduring predispositions, the key question is how children's "nonattitudes" develop into "real" attitudes. Presumably, fully crystallized attitudes are composed of a stable affective and cognitive mass in regard to the attitude object. To obtain that mass requires exposure to an extensive information flow, so the strongest socialization should be produced when the individual has been exposed to the most extensive information flow regarding the attitude object (Converse 1962; Sears 1983, 1993; Zaller 1992).

Political events are crucial elements in providing this information flow during the socialization process. To preadults, politics are usually of rather low visibility, with low ambient levels of exposure to relevant communication. As a result, most socializing communications, and the greatest socialization gains, are likely to be triggered by the intervention of exogenous political events. But these normally stimulate communication quite selectively, focusing only on a narrow range of specific attitude objects. Such events should become occasions for socialization of crystallized predispositions, therefore, but only toward the specific attitude objects they make salient.

This view has several implications. One is that politics matters, as the revisionists say, not only in adulthood but also in preadult socialization. The critical factor is which objects a political event makes salient. For example, during the incumbency of the popular presidents Eisenhower and Kennedy, children tended to acquire trusting attitudes toward the presidency; during the incumbency of the less popular Johnson and Nixon, children acquired less favorable attitudes (for a review, see Sears 1975).

A second implication is that political socialization will progress discontinuously over time. Political events tend to be episodic, regardless of whether they occur on some regular cycle, like U.S. elections, or at odd intervals, like wars or protests. So socialization should proceed by fits and starts rather than being steady and incremental. This fundamental discontinuity is important because it turns political socialization into a genuinely political process.

A third implication is that different attitudes are likely to have quite different growth curves (Hyman 1959, 61). Attitudes toward objects that attract strong information flows should approach adult levels of affective and cognitive mass more quickly than do those toward objects that are rarely addressed. It may be, for example, that basic religious and racial attitudes are learned quite early, because children receive much communication about them, while more esoteric political preferences (such as domestic spending priorities) are acquired much later. This may be one explanation for the familiar variation in attitude stability across political attitude objects. Attitudes toward high-salience objects, such as the parties or major candidates with long careers, tend to be highly stable, whereas attitudes toward low-visibility policy issues tend to be less stable (Converse 1970, Converse and Markus 1979, Sears 1983).[4]

A fourth implication is that generational effects should emerge on issues that become highly salient in one specific historical era. The socialization of the youthful cohort at that moment should be distinctively different from the socialization of cohorts who are young in other eras. Sometimes a full cohort will move en masse in one direction, but more often it will polarize internally around the symbolic events of its day, as in Mannheim's (1952) notion of "generational units."

In this view, socialization can depend on a preadult's life stage, but more because of its link to prior experience than because of developmental readiness. For our purposes we do not need to assume that the capacity for political learning increases consequentially with age after middle childhood. But political experience surely does tend to increase with age, even if not in any simple, linear fashion. And prior experience can substantially influence the effect of a political event. An event may have great effect on the inexperienced, as when one's nation goes to war with a country that was previously neither a close ally nor an enemy, but an event may have relatively little effect on more fully socialized individuals; for example, older adults' party identification may not be altered much by yet another election campaign. Preadults have less prior experience than adults in almost all political domains and so have more room for socialization gains. A political event is therefore likely to help preadults close this gap when adults already have fully crystallized attitudes toward the object in question.

In this study we treat presidential campaigns as a paradigmatic case of a political event that serves as an occasion for political socialization. Such campaigns are among the most communication-intense of ordinary political events. The mass media give them great publicity over the long primary and general campaign season, providing the occasion for considerable interpersonal communication as well. So campaigns, as periodic but very intensive events, should be important socializing opportunities for preadults.

Prior work on political socialization suggests that preadults' precampaign attitudes are likely to be high in expressed affect but based upon relatively little information and rather uncrystallized (see Sears 1975 for a review). They therefore should have room for substantial additional political socialization. Nevertheless, campaigns should have socializing effects quite selectively across attitude objects. The dominant content of communication during both the primary and general election season focuses on the parties, and on the major party candidates, leading us to expect the greatest preadult socialization gains for attitudes in those domains.[5] Because of media treatment of the campaign as a "horse race" (Patterson 1980), issues and basic ideology tend to be less salient, so we would expect less socialization of attitudes in these domains. Yet, this is simply one application of our more general point: *Whatever* content is emphasized by a political event becomes the focal point for socialization gains.

---

[3] A second possible criterion would add that the fully socialized individual also conforms to the specific content of the family's or local culture's conventional views. We do not feel this additional stipulation is central to having meaningful attitudes, however, and so restrict ourselves to the first.

[4] See Alwin and Krosnick (1991) and Krosnick (1991) for an alternative interpretation, which attributes such differences to differential measurement reliability.

THE HYPOTHESES

This reasoning leads us to four hypotheses.

HYPOTHESIS 1: *A presidential campaign should generate preadult political socialization gains in the attitude domains most central to the campaign, relative to precampaign baselines.*

HYPOTHESIS 2: *Preadults should show fewer socialization gains in the periods between presidential campaigns than during campaigns.*

HYPOTHESIS 3: *During the campaign period, preadults should show fewer socialization gains in attitude domains either less salient or wholly peripheral to the campaign than in domains central to the campaign.*

HYPOTHESIS 4: *The campaign should help close the initial socialization gap between preadults and adults in domains central to the campaign. Yet, it should not diminish the preadult-adult gap in domains peripheral to the campaign, or in any domains during the postcampaign period.*

We define socialization gains in terms of expressed affect, information, and attitude crystallization. Conceptualizing and operationalizing expressed affect and information are relatively straightforward, but attitude crystallization is another matter. The classic political behavior literature generally has used three tests to detect underlying attitude crystallization: stability, consistency, and the power of one attitude to determine other attitudes toward new or neutral objects (Sears 1975, 1983). Each dimension of crystallization typically has been indexed with simple aggregate-level correlations: stability, with correlations of the same attitude across time (Converse 1964); consistency, with correlations of attitudes toward different but ideologically linked objects at one time (Converse 1964); and power, with the correlation of a longstanding and highly stable predisposition (such as party identification) with attitudes toward a new or unfamiliar attitude object (such as candidate choice; Campbell et al. 1960).[6]

Simple bivariate correlations, however, are awkward for testing our hypothesized nonlinear effects over time and the interactions of time with cohort. And such correlations can potentially hide considerable individual change over time as distributions shift in correlated fashion (see Smith 1989, 224). Therefore, we add a measurement approach that provides a score for each individual on each of these three dimensions of crystallization (Barton and Parsons 1977, Sears and Citrin 1985, Wycoff 1980). In the analogous area of self-concept research, this has been shown to contribute information beyond the standard aggregate-level correlational approach (Pelham 1993). In practice, as will be seen, the aggregate-level correlations and individual-level scores almost always parallel each other; the few discrepancies that do emerge appear to be anomalies and not consequential for our main conclusions. But the use of measures at both aggregate and individual levels increases our confidence in the outcomes.

THE DATA

The data come from a three-wave panel study of a probability sample of Wisconsin families. Interviews were conducted just before the 1980 Wisconsin primary (in January to March 1980), at its conclusion (in October 1980), and a year later (in October and November 1981).[7] The population was defined as preadults aged 10 to 17 in the state of Wisconsin (we use the terms "preadults" and "adolescents" interchangeably, though recognizing that this range spans years conventionally described as falling from later childhood through adolescence). Using random-digit-dialing techniques, families were contacted; after ascertaining the age of preadults in the household, a random sample was selected of approximately 100 preadults aged from 10 to 17 years, each of whom was interviewed by telephone. In each household one parent was then randomly selected and interviewed. The first wave yielded 718 parent-offspring pairs, with a response rate of approximately 70%. The second wave yielded 501 pairs, and the third yielded 366 pairs; these latter are the respondents for our data analyses.

Because the population of interest was Wisconsin adolescents, the adult sample was not intended to represent the general adult population. Nevertheless, the adults in these 366 pairs do not differ greatly from adult samples in other comparable surveys of that era. As is usual with a telephone survey, it somewhat overrepresented better-educated adults: 42% had attended some college, which is a little higher than the 36% in the National Election Studies (NES) 1980 standard pre-post face-to-face survey but considerably below the 54% in the 1984 NES telephone survey (the rolling cross-section). It also contained slightly more females than usual (59%, because of female-headed single-parent households). The fullest descriptions of the study itself are provided by Chaffee and Schleuder (1986), Chaffee and Tims (1982), Kennamer and Chaffee (1982), and Owen and Dennis (1982).[8]

---

[5] This is not necessarily always true, of course. Some campaigns are fought more explicitly along party lines than others. Moreover, the balance of attention may shift somewhat between the party and candidate domains as the contest shifts from primary season to general election campaign. In both periods, however, information about candidate and party tends to dominate information about other attitude domains, both in volume and clarity.

[6] Most treat this latter correlation as reflecting the power of party identification over candidate choice, though there is some debate about the degree of recursivity in that relationship (see Markus 1983). For a related but somewhat different conceptualization of attitude crystallization (though described as "attitude strength"), based more on experimental research in social psychology, see the excellent collection edited by Petty and Krosnick (1995), especially Krosnick and Petty (1995).

[7] The data from this study are at the Social Sciences Data Archive, Institute for Social Science Research, UCLA, Los Angeles, CA 90095 (Elizabeth Stephenson, Data Librarian).

Analytic Design

The most salient stimuli in presidential campaigns are the candidates running for election and the two political parties, so the maximum preadult socialization gains from the campaign should accrue in these two attitude domains. Issues and general ideology are usually less salient features. The major issues of the 1980 campaign centered around energy policy, the economic puzzles surrounding simultaneously high inflation and unemployment, and military and defense policy (Plotkin 1981). Nevertheless, there was much uncertainty among the general public about the candidates' issue differences and even about their ideological stands; for example, as late as October, fewer than half the NES respondents were able to place Reagan as an ideological conservative (Markus 1982). Some other issue domains were quite peripheral to the 1980 campaign, such as racial and First Amendment issues. The fundamental aspects of system support (such as political trust or internal political efficacy) are usually peripheral to presidential campaigns.[9] In these six less visible domains, we would expect few socialization gains.

We assess the effects of the campaign by comparing wave 1, conducted at the outset of the campaign, with wave 2, conducted at its conclusion. On the one hand, if the campaign was a potent socializing event, then adolescents should show gains in the most central domains over these two waves. They should be less likely to show socialization gains in the less salient and peripheral domains or in any domains between wave 2 and wave 3, conducted a year later. On the other hand, if any gains result from the simple passage of time, maturation, and/or ongoing family socialization independent of the campaign, then gains may be observed in all domains and across all waves.

Affect and Information

Four scales measured strength of partisan affect. (1) Candidate opinionation was the mean percentage expressing either like or dislike for the four most salient candidates (Carter, Kennedy, Reagan, and Bush). (2) Candidate affective intensity was the mean intensity of opinion expressed toward these candidates. (3) Party opinionation was the mean percentage expressing opinions on four items evaluating the two parties. (4) Party affective intensity was the mean intensity of opinion on the four party items. The details of scale construction are given in the Appendix.

Three scales measured partisan information. (1) Candidates' party affiliation was the percentage of correct answers across four items on the major candidates. (2) Party symbols information was the percentage of correct responses in linking a party to each of fourteen traditional partisan symbols. (3) Party issue-placement was the percentage of correct answers in assigning a party to each of four specific issue positions.

Crystallization

As indicated earlier, the classic political behavior literature has indexed the underlying crystallization of an attitude in three ways: stability over time, consistency at one point, and "power" over attitudes toward new attitude objects. We present data in eight attitude domains (candidates, parties, ideology, campaign issues, racial policy, civil liberties tolerance, political trust, and internal political efficacy), using two measurement approaches (aggregate-level correlations and individual-level scores).

The advantage of so much data is that we can replicate hypothesis tests across numerous comparisons. The disadvantage is that presenting all these results would overwhelm the bounds of a single paper: There are potentially 48 available indicators of affective expression, information, and crystallization. In being selective, two rules of thumb were applied. First, we gave the fullest treatment to the attitude domains most central to the campaign. Second, we gave the fullest treatment to attitude stability, as the most appropriate statistic for the theoretical purpose at hand: the socialization of enduring predispositions. Our compromise, therefore, was to give the full treatment (all available indicators) to the most salient domains (candidates and party); the minimum treatment (stability only) to the peripheral domains (race, civil liberties, trust, and efficacy); and intermediate treatment (all indicators of crystallization—stability, consistency, and the "power" of party identification) to the domains of intermediate salience (ideology and issues). This totals a little under half the theoretical maximum.[10]

---

[8] The panel aspect of the study has been used previously mainly to study media use in politics (Chaffee and Miyo 1983, Chaffee and Schleuder 1986, Chaffee and Tims 1982, Kennamer and Chaffee 1982) and attitudes toward the party system (Dennis 1986). Owen and Dennis 1987, 1988, 1992) have used it for various cross-sectional purposes. Although these studies were focused primarily on other phenomena, they did turn up some incidental evidence relevant to our hypotheses. During the 1980 campaign, preadults showed increases in some aspects of partisan information (Dennis 1986, Kennamer and Chaffee 1982), in willingness to declare themselves "Independent" (Dennis 1986), and in the tendency to favor their own presidential candidate over his opponent (Chaffee and Miyo 1983, Kennamer and Chaffee 1982). Adults' candidate preferences were more stable than preadults' through the campaign year (Chaffee and Miyo 1983).

[9] Although the case can be made that political trust had more visibility in 1980 than often has been the case; see Citrin and Green (1986).

[10] In our judgment, we erred on the side of inclusion in two respects. Attitude constraint is normally assessed across different issues (Converse 1964), but in the present study, party and ideological consistency merely reflect consistent responses to the same attitude object (such as "Democrats" or "conservatives"). In these two cases,

Operationally, we began with the traditional correlations across individuals. For attitude stability, we constructed simple additive scales in each attitude domain, recoding all items in the same partisan direction where necessary, and then correlated (Pearson *r*) scale scores across adjacent waves within a domain. To test for attitude consistency within a given domain within a wave, we computed Cronbach's alpha using all items in the domain. To test for attitudinal consistency across domains, we computed Pearson correlations for the party identification scale with, respectively, the candidate evaluation, ideology, and campaign issue scales within each wave. The details of the scale construction are given in the Appendix.

We then replicated with the more novel individual-score approach required by the hypothesized across-wave effects for preadults (which should be nonlinear) and hypothesized differential effects for adolescents and adults (which call for cohort-by-wave interactions). To do so, we developed stability and consistency scores for individuals within domains as well as scores for consistency of party identification with attitudes in selected other domains. The *stability* measures consisted of the absolute difference between an individual's responses to a given item across two waves, summed across all items in that attitude domain (with a low score reflecting greater stability). For example, the stability of an individual's party identification from wave 1 to wave 2 would be indexed by the absolute difference between each party identification item in wave 1 and that item in wave 2, summed over items.[11] *Consistency* was indexed with the summed absolute differences between individual items and the overall scale score for that domain. A low score reflected more consistency. The *cross-domain consistency* measures consisted of the absolute difference between the individual's party identification scale score, on the one hand, and the candidate evaluation, ideology, or campaign issue scale scores, on the other. A low score again reflected more consistency.

We begin by simply presenting the stability, consistency, and cross-domain consistency correlations. We then move onto the individual score analysis, conducting mixed-design analyses of variance in which cohort (adolescent versus adult) was a between-subject factor, wave was a within-subject factor, and the cohort-by-wave interaction was a mixed factor. For both the wave main effect and the cohort-by-wave interaction, we also tested two specific comparisons: wave 1 versus wave 2, to test for change during the campaign period, and wave 2 versus wave 3, to test for change during the postcampaign period. We also tested the cohort main effect in wave 1 only.[12]

It should be noted that the aggregate-level techniques and individual-level measurement are statistically quite different, especially in the case of stability. The former only indexes the stability of a hypothetical underlying construct, using a composite scale, while the latter reflects the stability of each attitude, item by item, providing more precise information about the stability of individual attitudes. The use of two such sharply different measurement techniques actually leads to surprisingly few discrepancies, and these do not seem to be particularly meaningful. The power of this study, we believe, lies in the built-in replication across domains and measurement techniques.

THE STARTING POINT: PREADULTS' IMMATURE PARTISAN ATTITUDES

Most of the political socialization literature suggests that preadults freely express strong partisan opinions but have little information and weak underlying attitudes relative to adults. The statistical tests for these cohort effects are shown in column 1 of Table 1.

Indeed, preadults' levels of partisan opinionation were quite high at the outset of the presidential campaign, with the mean preadult expressing opinions on 83% of the candidate items and on 69% of the party items. This is shown graphically in Figure 1. The initial preadult-adult differences in expressed affect were generally small but statistically significant; preadults were significantly lower in candidate opinionation, candidate opinion intensity, and party opinionation but not party opinion intensity (see Table 1, lines 1–4). We see little reason to make much of this pattern.

These preadult partisan evaluations were initially based on rather little information, however, as shown in Figure 2. For example, at the beginning of the campaign, preadults identified the candidates' party correctly 49% of the time, on average, as against 82% for the adults. This preadult-adult difference, and those on the party symbol and party issue-placement scales, were all highly significant (shown in Table 1, lines 5–7).

Nor were preadults' strong initial partisan affects based on highly crystallized underlying attitudes. For example, their initial candidate evaluations had no partisan consistency prior to the campaign (the Cronbach's alpha for preadults was .00, as shown in Figure 3). Nor were those initial candidate evaluations highly correlated with the preadults' party identification (r = .19). Their partisan attitudes did show some substantial stability, however: r = .40 from wave 1 to wave 2 for the

---

consistency may be superficial and momentary rather than reflect real crystallization, but we have included them for completeness. Second, we have included the power of party identification over ideology and issues. Even among adults, however, the correlations between party identification and these latter two dimensions are not pure indicators of the power of party identification because they reflect some recursivity. As a result, we describe them as indexing attitude consistency across domains rather than power. The individual-level data for all four indicators are described fully in the tables, but to save space the aggregate-level correlational data are described in the text rather than in the figures. As will be seen, the main thrust of the findings remains the same whether these indicators are included or excluded from the analysis.

[11] As might be expected, these scores sometimes yielded extremely skewed distributions, with most individuals rather stable, and a few highly unstable. To compensate, range restrictions were imposed in some cases, based on examination of the wave 1 distributions for adults only, grouping together on average approximately 4% of most unstable individuals. See the Appendix for details.

[12] These two specific comparisons are nonorthogonal but test our hypotheses more directly than would strictly orthogonal comparisons (such as wave 1 versus wave 2 and waves 1 and 2 versus wave 3).

TABLE 1. *F* Values for Partisanship Domains

	Effects				
	Preadults vs. Adults	Preadults Only		Preadults and Adults	
Dependent Variables	Cohort	Wave 1 vs. Wave 2	Wave 2 vs. Wave 3	Cohort by Wave 1/ Wave 2	Cohort by Wave 2/ Wave 3
Affect					
1. Candidate opinionation	7.68**	57.98***	9.14**	5.27*	0.09
2. Candidate opinion intensity	14.73***	40.90***	11.42***	4.41**	0.02
3. Party opinionation	15.66***	43.39***	(1.11)	8.56**	0.05
4. Party opinion intensity	2.43	30.91***	(4.67)*	9.92**	(2.97)
Information					
5. Candidates' party	226.84***	132.50***	(1.44)	54.10***	0.00
6. Party symbols	300.78***	23.16***	29.01***	3.40	9.76**
7. Party issue-placement	9.86**	34.19***	44.59***	0.44	0.38
Attitude crystallization					
8. Consistency of candidate evaluations	9.34**	28.76***	0.09	0.15	0.26
9. Stability of candidate evaluations	18.34***	16.64***	NA	0.29	NA
10. Stability of party ID	59.68***	7.15**	NA	3.36	NA
11. Consistency of party ID with candidate evaluations	24.96***	17.71***	4.23*	3.95*	0.79
12. Consistency of party ID	0.09	(2.03)	6.27*	(0.11)	0.78

Note: "Cohort" effect compares preadults with adults in wave 1. Comparisons in columns 1, 4, and 5 use total sample of 732 respondents. Comparisons in columns 2 and 3 are among 366 adolescent respondents only. Effects in parentheses in columns 2 and 3 reflect socialization losses for preadults; in columns 4 and 5, smaller socialization gains for preadults than for adults. All p-values computed on 1/>100 df, with two-tailed tests.
*p < .05, **p < .01, ***p < .001.

partisan bent of their initial candidate evaluation, and $r = .54$ for their party identification.

The preadults initially fell well short of the adults in all these indicators of initial crystallization, like those for information. The consistency of adults' candidate evaluations was .31 (alpha); the consistency of party identification with candidate evaluations, $r = .63$; the stability of their candidate evaluations, $r = .67$; and the stability of their party identifications, $r = .88$. In the analyses of variance, preadults had significantly less crystallized partisan attitudes than did adults on all four dimensions, as shown in Table 1 (lines 8–11).

The exception is the consistency of party identification, which was only slightly (and nonsignificantly) lower for preadults than for adults (alphas of .80 and .87, respectively). This index, however, was at a virtual ceiling for both groups in every wave (alphas of at least .80 in each case). And, as indicated earlier, on conceptual grounds it is a doubtful index of genuine attitude crystallization because it assesses consistency of response to the same objects rather than across different attitude objects. For completeness, the data are shown in Table 1 (line 12) but to save space are not included in Figure 3.

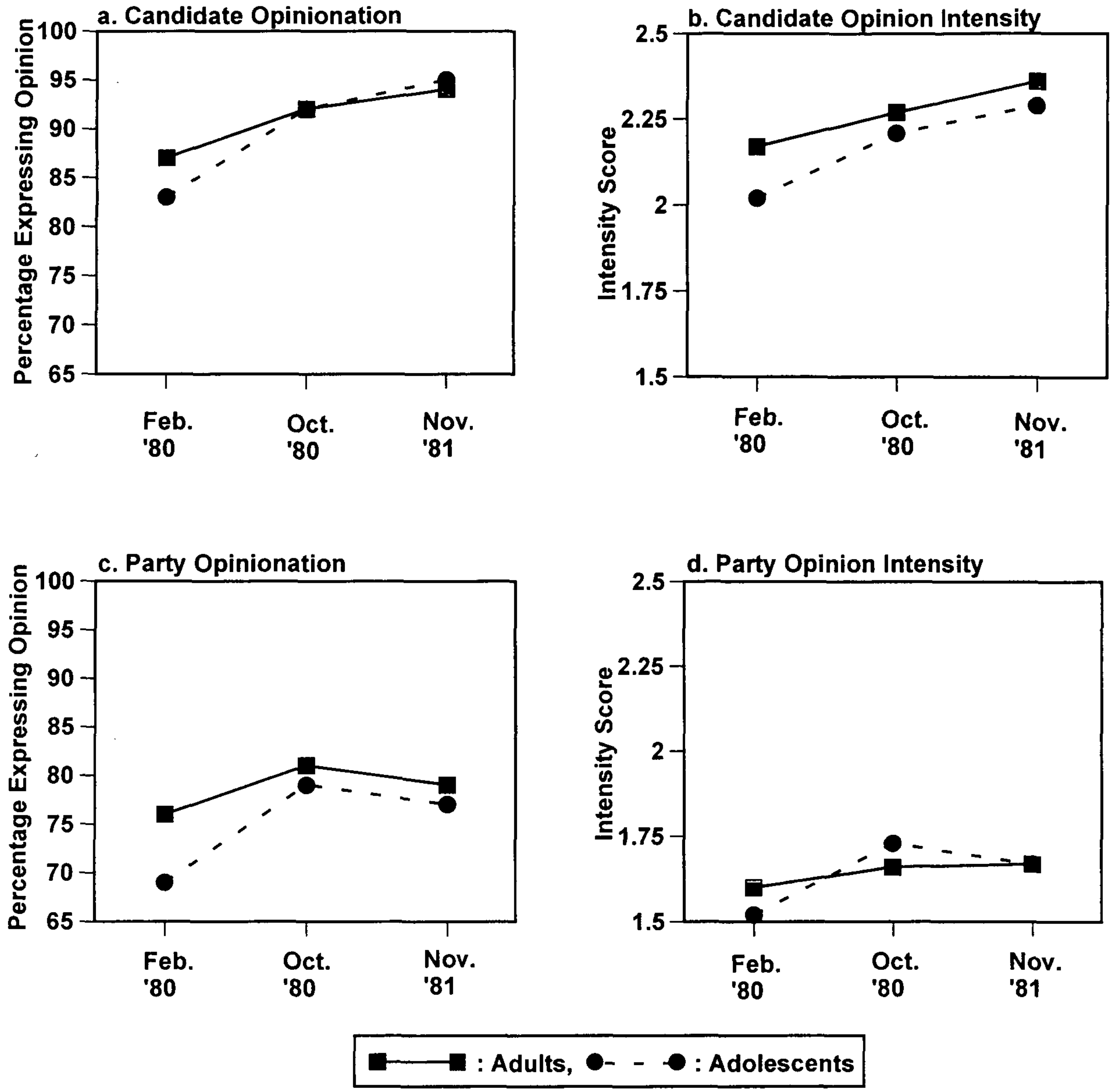
In short, the precampaign portrait of these preadults is familiar from other studies of political socialization: They freely expressed intense partisan affects but had relatively little factual information and fell considerably short of adult levels of partisan attitude crystallization. So, at least in this precampaign stage, the critics of political socialization research seem to be right on the money: The opinions, though often expressed, often seem to be poorly informed "nonattitudes," based on little real conviction, leaving much room for further socialization. It should be noted, however, that preadults' initial party identification represented something of an exception, demonstrating a fairly high level of stability; we will return to this point.

PREADULTS' SOCIALIZATION GAINS DURING THE CAMPAIGN

Partisanship in terms of both parties and candidates traditionally has been the central focus of any presidential campaign, though their relative emphases may shift somewhat from the primary to the general election campaign season. Therefore, we expect the clearest preadult socialization gains over the course of the campaign in the candidate and party domains (Hypothesis 1). The appropriate statistical tests are the wave 1 versus wave 2 main effect in both domains, among preadults only; these tests are shown in column 2 of Table 1.

Preadults' partisan affect did increase over the course of the campaign. Their candidate opinionation rose from an average of 83% to 92%, and their party opinionation from 69% to 79%. They expressed more intense partisan attitudes, as well, as shown in Figure 1. All four increases are highly statistically significant (see Table 1, lines 1–4). Their partisan information also increased during the campaign. Knowledge of the candidates' parties greatly increased, from 49% to 67%, with smaller increases in correct assignment of partisan symbols to party (from 39% to 44%) and party

FIGURE 1. Partisan Domains: Affect



issue-placement (from 41% to 52%). All three increases are shown in Figure 2, and each is highly statistically significant (see Table 1, lines 5–7).

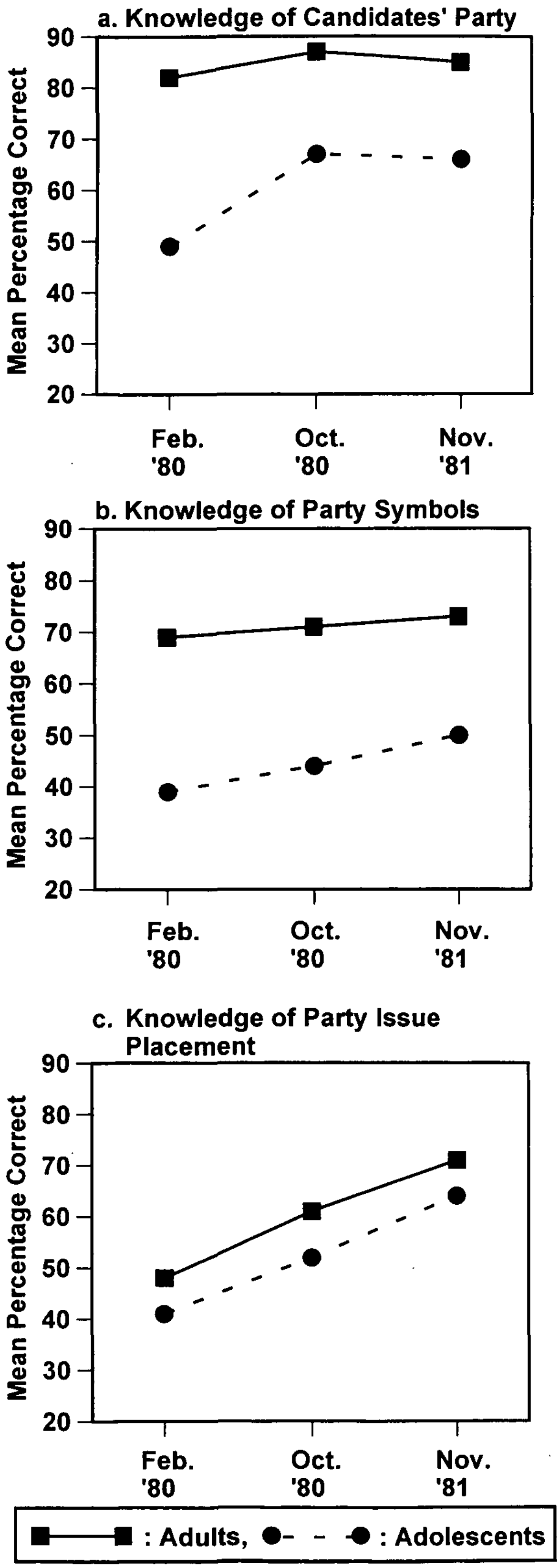
Partisan attitudes became more crystallized as well. Preadults' candidate evaluations became more consistently partisan; that is, more consistently pro-Republican or pro-Democrat. The Cronbach's alpha for the candidate consistency scale increased from .00 to .39 from wave 1 to wave 2. Preadults' candidate evaluations became markedly more stable: The Pearson *r* for the candidate evaluation scale was .40 from wave 1 to wave 2, as indicated above, but their end-of-campaign attitudes were much more stable—the Pearson *r* from wave 2 to wave 3 was .62. These campaign-driven increases in the crystallization of candidate partisanship are shown in terms of aggregate-level correlations in Figure 3. When we turn to the individual-level measures, these increases prove to be highly statistically significant, as shown in Table 1 (lines 8 and 9).[13]

Preadults' party identification also became more crystallized during the campaign. It increased in stability: The test-retest Pearson *r* for the party identification scale from wave 1 to wave 2 was .54, but from wave 2 to wave 3 it was .70. And candidate evaluations had become much more consistent with party identification by the end of the campaign. In wave 1, the two were not highly correlated (*r* = .19), but by the end of the campaign, in wave 2, they were very consistent (*r* = .52). Preadults who identified themselves as Republi-

---

[13] Presumably, this crystallization of candidate partisanship is partly a response to a change of external campaign focus from intraparty to interparty conflict between candidates, as well as a reflection of preadult learning. But party stimuli, too, are ubiquitous in the primary season.

FIGURE 2. Partisan Domains: Information



can increasingly favored Reagan and Bush and rejected Carter and Kennedy, while the reverse held for those who identified themselves as Democrat. These increases are shown in terms of aggregate-level correlations in Figure 3, and both are highly significant, as shown in Table 1 (lines 10 and 11). Finally, as we expected, party identification itself became somewhat more internally consistent over time, with the alpha rising from .80 to .85, but the difference is not statistically significant because the initial reading was close to the practical ceiling, as indicated earlier.

In sum, at the outset of the campaign the preadults freely expressed intense partisan affects, but their opinions were poorly informed and seem to have been only modestly crystallized. The campaign changed all that. Eleven of our twelve indicators of partisan socialization showed significant preadult socialization gains over the course of the campaign. Whereas the preadults' precampaign opinions often were poorly informed nonattitudes, their postcampaign partisanship gave evidence of considerably more maturity.

PARTISAN SOCIALIZATION STOPS AT THE END OF THE CAMPAIGN

If our theoretical reasoning is correct, then these socialization gains should have largely ceased at the end of the presidential campaign, with the postcampaign period inspiring no comparable advances (Hypothesis 2). The appropriate tests are the wave 2 versus wave 3 main effects within partisan domains, again for preadults only. It should be noted that this comparison is somewhat conservative in that it predicts larger changes during the first period, which spanned only nine months, than during the second, which spanned thirteen. These statistical tests are shown in column 3 of Table 1.

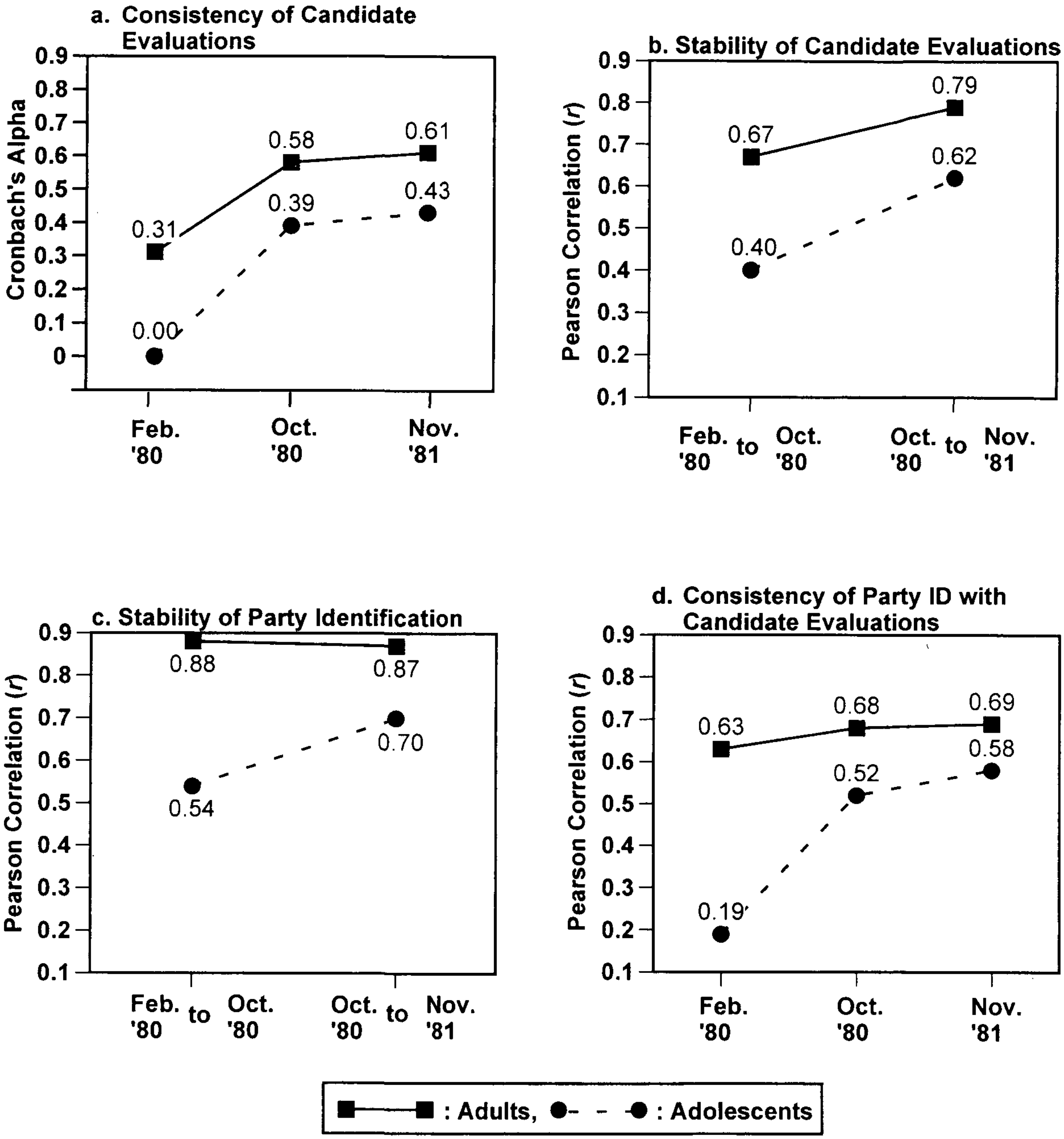
Preadults' partisan affect did, in fact, increase much less in the year following the campaign than it had during the campaign, as shown in Figure 1. It increased significantly on two dimensions and decreased significantly on one (see Table 1, lines 1–4). Partisan information continued to increase somewhat, as shown in Figure 2. Knowledge of candidates' party declined a little, while knowledge of party symbols and of parties' issue-placement continued to rise significantly, as shown in Table 1 (lines 5–7).[14] The aggressive policy agenda of the incoming Reagan administration apparently cast new light on its policy positions. Nevertheless, on most dimensions of partisan affect and information, the preadults showed larger and more systematic socialization gains during the campaign than in postcampaign period (as can be seen by comparing the *F* values in columns 2 and 3 of Table 1).

The crystallization of partisan attitudes also slowed during the postcampaign period. Figure 3 shows that the increase in the consistency of candidate evaluations came to a sharp halt after the campaign; the difference between wave 2 and wave 3 is nonsignificant (see Table

---

[14] Some of the party symbols concerned historical figures, such as Lincoln or Roosevelt, who are not central to party rhetoric in most campaigns. If such times are deleted, the remaining symbols resemble conventional campaign issues (such as poor people, conservative, business, labor) and yield findings much like those for the party issue-placement scale: All effects are still significant, but the postcampaign changes are somewhat greater.

FIGURE 3. Partisan Domains: Attitude Crystallization



1, line 8). The consistency of candidate evaluations with party identification did continue to rise slightly (and significantly) but far below the pace of its earlier change: The wave 1 to wave 2 increase was from $r = .19$ to $r = .52$, while the further advance to wave 3 was only to $r = .58$.[15] Note that we cannot use attitude stability to assess the effect of the postcampaign period, since the stability of wave 3 attitudes cannot be assessed without a fourth wave.

In summary, during the campaign, preadults showed strong socialization gains on virtually every index regarding both the major candidates and the political parties. But these gains were mostly limited to the campaign period. A year later, the socialization gains were considerably smaller, where they occurred at all, even though the postcampaign period was half again as long.

NO SOCIALIZATION GAINS IN LESS SALIENT ATTITUDE DOMAINS

If the presidential campaign was responsible for these socialization gains, then they should be evident only toward attitude objects central to the campaign. Preadults should have shown few socialization gains toward less salient attitude objects, either during or after the campaign (Hypothesis 3). The appropriate tests of the hypothesis simply repeat the tests of hypotheses 1 and 2 in attitude domains other than partisanship; that is, in the less salient domains of issue positions and ideology, and the peripheral domains of racial tolerance, civil liberties tolerance, political trust, and internal political efficacy.

---

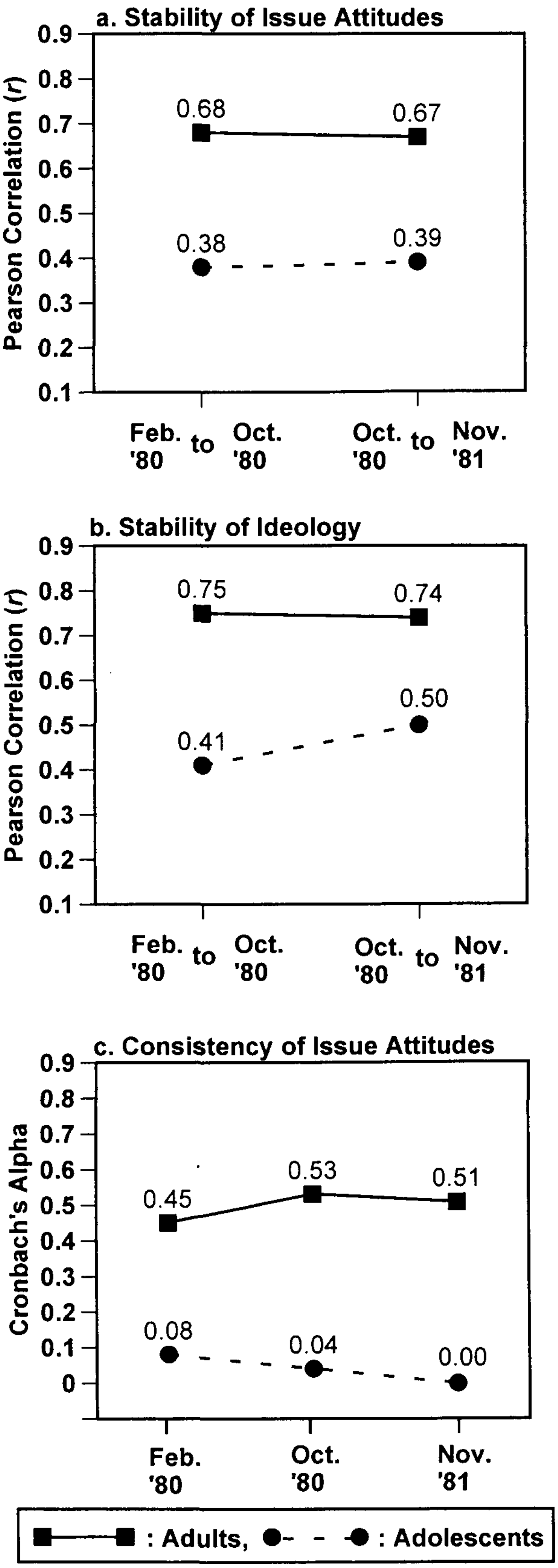
[15] The consistency of party identification advanced only slightly and less than it had from wave 1 to wave 2, according to the correlational statistics (alpha = .80, .85, and .87 in the three waves). This difference was significant, but all these values are almost at the practical ceiling.

To begin with, preadults showed the familiar precampaign deficiencies in socialization in these less salient domains as well. Their precampaign issue attitudes and ideologies displayed less stability and consistency than did those of adults, as shown in Figure 4. Four of the six cohort main effects are significant (see Table 2, column 1, lines 1–6). Similarly, preadults had much less stable initial attitudes than did the adults in the most peripheral domains (racial tolerance, civil liberties tolerance, political trust, and internal political efficacy), as shown in Figure 5. All four cohort differences were statistically significant, as shown in Table 2 (column 1, lines 7–10).

Preadults did not show systematic socialization gains during the campaign in these domains, as depicted in Figure 4. Most crucial, the stability of issue attitudes and ideology did not increase significantly. The consistency of issue attitudes actually declined. There were no systematic advances in the consistency of ideology, of issue attitudes with party identification, or of ideology with party identification, according to the aggregate-level correlations (according to the individual-level data reflected in column 2 of Table 2, however, two significantly decreased).[16] Nor did the campaign produce any systematic change in attitude stability in the four most peripheral domains. As shown in Figure 5, the correlational data yield mixed and small changes across these domains. Only one of the four effects is statistically significant in the analyses of variance of individual-level scores (also see column 2 of Table 2).

In short, preadults made few socialization gains during the campaign period in all these less visible attitude domains. One of the nine indicators of attitude crystallization yielded a significant increase, and two yielded a significant decrease. In contrast, as indicated earlier, all but one of the twelve indicators of party and candidate partisanship yielded significant gains. The effect of the campaign as a socializing event seems to have been largely limited to the domains most central to it.[17]

FIGURE 4. Less Salient Domains: Attitude Crystallization



---

[16] The wave 1 correlations were .35 (alpha), .12, and .07 (both Pearson *r*'s), respectively, and the wave 2 correlations were .28 (alpha), .12, and .23 (both Pearson *r*'s).

[17] Because we mainly rely on measures of attitude stability to index enduring underlying crystallization, we do not have much evidence on possible postcampaign socialization gains in these less visible domains. As shown in Table 2, one of our four indicators (ideology consistency) yields a significant increase, but we have the same reservations about this index as about party consistency; that is, it simply reflects consistent responses to the same attitude object (liberals versus conservatives), rather than testing for consistency of response to ideologically linked different attitude objects, as in the traditional measurement of attitude constraint (see Converse 1964).

TABLE 2. *F* Values for Domains Other than Partisanship

	Effects				
	Preadults vs. Adults	Preadults Only		Preadults and Adults	
Dependent Variables	Cohort	Wave 1 vs. Wave 2	Wave 2 vs. Wave 3	Cohort by Wave 1/ Wave 2	Cohort by Wave 2/ Waver 3
Less salient domains					
1. Stability of issue attitudes	30.88***	0.04	NA	0.01	NA
2. Stability of ideology	27.94***	2.36	NA	0.12	NA
3. Consistency of issue attitudes	12.43***	(18.72)***	0.00	(9.22)**	0.31
4. Consistency of ideology	16.15***	0.07	5.74*	2.97	0.09
5. Consistency of party ID with issue attitudes	(0.51)	(19.88)***	(0.07)	(11.20)***	(0.37)
6. Consistency of party ID with ideology	1.41	(2.11)	0.11	1.44	1.63
Peripheral domains: Stability					
7. Racial tolerance	27.98***	1.27	NA	1.29	NA
8. Civil liberties tolerance	65.67***	9.97**	NA	5.02**	NA
9. Political trust	21.52***	(0.24)	NA	1.07	NA
10. Internal political efficacy	5.48*	0.33	NA	0.79	NA

Note: "Cohort" effect compares preadults with adults in wave 1. Comparisons in columns 1, 4, and 5 use total sample of 732 respondents. Comparisons in columns 2 and 3 are among 366 adolescent respondents only. Effects in parentheses in columns 2 and 3 reflect socialization losses for preadults; in columns 4 and 5, smaller socialization gains for preadults than for adults. All *p*-values computed on 1/>100 *df*, with two-tailed tests.
$^*p < .05$, $^{**}p < .01$, $^{***}p < .001$.

LIFE STAGE EFFECTS: REACHING ADULT LEVELS OF SOCIALIZATION

Finally, did the presidential campaign play a crucial role in bringing preadults' partisan attitudes closer to adult levels? Hypothesis 4 specifies that the preadult-adult gap should have narrowed, especially in the domains central to the campaign and mainly during the campaign period. The relevant statistical test is the cohort (preadult versus adult)-by-wave 1/wave 2 interaction. This reflects the extent to which the two age cohorts changed differentially from wave 1 to wave 2. In other words, is the cohort difference at wave 2 significantly smaller than the cohort difference at wave 1? These tests are shown in Table 1, column 4.

The preadults improved relative to adults during the campaign for nearly all measures focused on party identification, as shown in figures 1 and 2. For both dimensions of party affect, the cohort-by-wave 1/wave 2 interaction is significant (see Table 1, lines 3 and 4). The interaction is marginally significant for information about party symbols. Perhaps most important is the increase in crystallization of party identification among preadults, who reduced the cohort gap on both key indicators. The gap in stability of party identification dropped sharply; stability from wave 1 to wave 2, through the campaign period, was $r = .54$ for preadults and $r = .88$ for adults, for a gap of $r = .34$. The preadults' postcampaign party identification was markedly more stable ($r = .70$), whereas the adults' stability scarcely changed ($r = .87$), for a gap of $r = .17$. The appropriate comparison is between the r^2s. The wave 1/wave 2 stability gap was .48, compared to .27 for wave 2/wave 3, just 56% of the original gap. In terms of the individual-level scores, this interaction approaches significance ($p < .10$; see Table 1, line 10).

The gap in the consistency of candidate evaluations with party identification—perhaps the latter's most important role—dropped even more substantially. At wave 1, the two indicators were quite weakly correlated for preadults ($r = .19$) and strongly correlated among adults ($r = .63$), leaving a huge gap of $r = .44$. It had narrowed greatly by the end of the campaign: Party identification was correlated with candidate evaluations at a much higher level for preadults ($r = .52$), whereas the adult level increased only slightly (to $r = .68$), resulting in a sharply reduced gap of $r = .16$. Again, the appropriate comparison is between the r^2s. The gap at wave 1 was .36, whereas at wave 2 it was .19, just 53% of the original gap. This cohort-by-wave 1/wave 2 interaction is statistically significant (see Table 1, line 11).

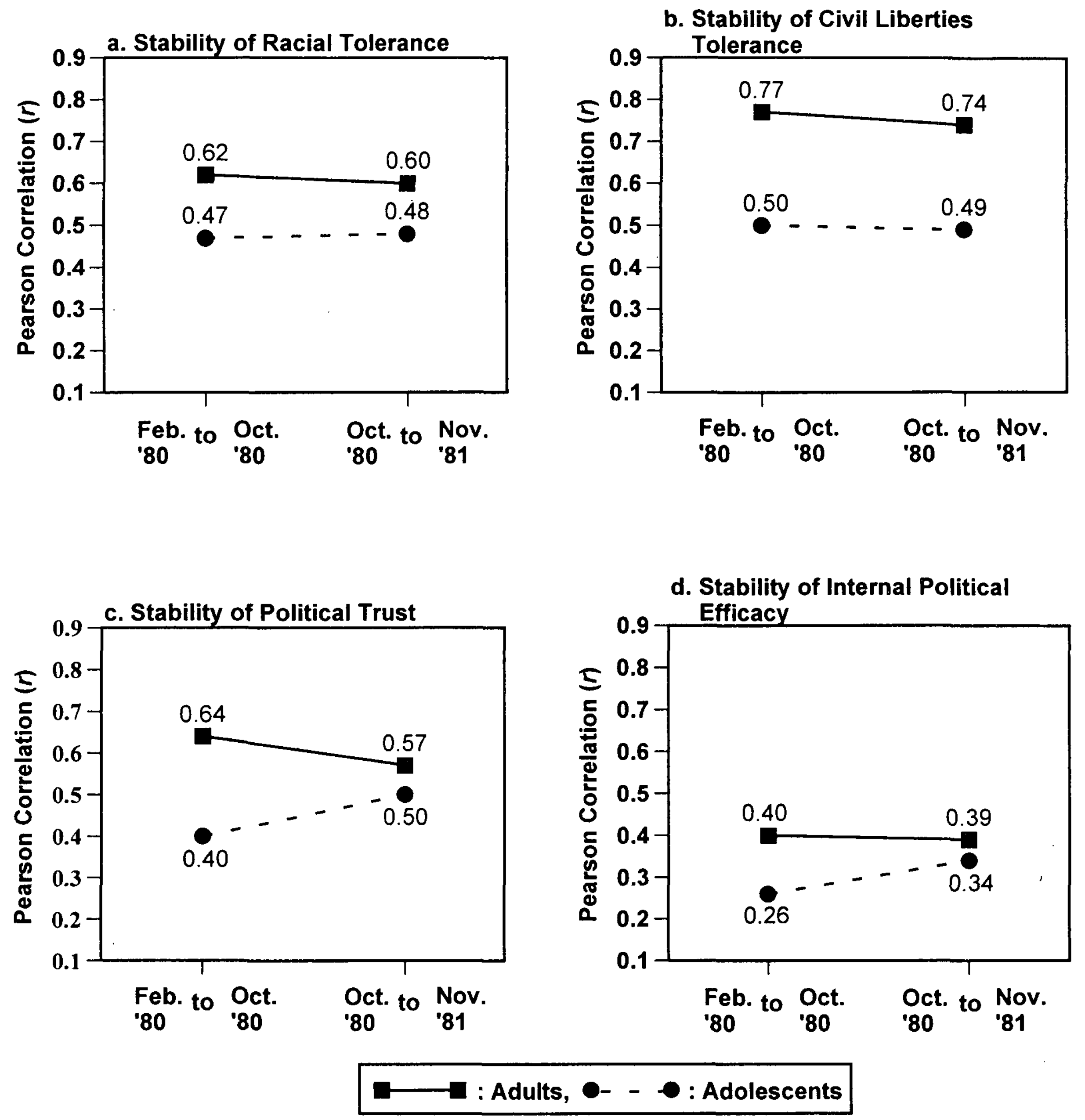
There were two exceptions. Accuracy of party issue-placement increased for preadults and adults largely in parallel, as shown in Figure 2. And the gap in consistency of party identification decreased but not significantly.[18] As with other tests of this latter indicator, all values were within such a small range that any differences probably are not meaningful.

Orientation toward the candidates did not show such a consistent reduction of the preadult-adult gap. The relevant interaction was significant for candidate affect (both opinionation and opinion intensity) and knowledge of candidates' party but not for either of the

---

[18] The gap was larger in wave 1 (alpha of .80 for preadults and .87 for adults) than in wave 2 (alpha of .85 and .88, respectively).

FIGURE 5. Peripheral Domains: Attitude Crystallization



indicators of attitude crystallization (consistency and stability of candidate evaluations), as shown in Table 1. The reason is apparent from Figure 3: The campaign crystallized candidate evaluations among adults just as much as it did among preadults. This is perhaps not surprising, since at the outset of most campaigns the parties are older and more familiar attitude objects than are most candidates, even to adults. As a result, the crystallization of preadults' and adults' attitudes toward the candidates increased in parallel; the campaign seems to have taught adults as well as preadults a good bit about the candidates.[19]

As expected, this narrowing of the gap in party identification was largely confined to the campaign period. The relevant tests are the cohort-by-wave 2/wave 3 interactions, which are shown in Table 1, column 5. In the candidate and party domains, only one

---

[19] Consistent with this reasoning, Markus (1982) has shown that adults posted gains through the 1980 campaign even on the minimal criterion of candidate recognition. Yet, candidate evaluations in the general public were highly volatile during that year; indeed, all three candidates were sharply "redefined" during the campaign—with Carter and Kennedy losing much support in the process. The parties' perceived positions were, in aggregate, much more stable through the campaign, as revealed in the 1980 NES panel (Petrocik, Verba, and Schultz 1981). Nevertheless, the adult-preadult gap in crystallization *did* drop during the campaign with respect to Carter and Reagan, who were the most visible candidates (Chaffee and Miyo 1983).

TABLE 3. Means for Individual Scores for Partisanship Domains

	Preadults			Adults		
	Wave 1	Wave 2	Wave 3	Wave 1	Wave 2	Wave 3
Affect						
1. Candidate opinionation	.83	.92	.95	.87	.92	.94
	(.21)	(.17)	(.14)	(.20)	(.19)	(.14)
2. Candidate opinion intensity	2.02	2.21	2.29	2.17	2.27	2.36
	(.55)	(.50)	(.44)	(.50)	(.49)	(.40)
3. Party opinionation	.69	.79	.77	.77	.81	.79
	(.27)	(.21)	(.22)	(.23)	(.19)	(.22)
4. Party opinion intensity	1.52	1.73	1.67	1.60	1.66	1.67
	(.71)	(.65)	(.63)	(.64)	(.58)	(.62)
Information						
5. Candidates' party	.49	.67	.66	.82	.87	.85
	(.35)	(.29)	(.28)	(.24)	(.19)	(.21)
6. Party symbols	.39	.44	.50	.69	.71	.73
	(.22)	(.22)	(.23)	(.24)	(.23)	(.22)
7. Party issue-placement	.41	.52	.64	.48	.61	.71
	(.30)	(.29)	(.30)	(.30)	(.29)	(.29)
Attitude crystallization						
8. Consistency of candidate evaluations	1.14	.97	.96	1.03	.88	.89
	(.39)	(.44)	(.46)	(.43)	(.45)	(.47)
9. Stability of candidate evaluations	1.02	.85	NA	.83	.69	NA
	(.60)	(.54)		(.51)	(.50)	
10. Stability of party ID	.87	.78	NA	.58	.57	NA
	(.56)	(.52)		(.44)	(.47)	
11. Consistency of party ID with candidate evaluations	1.55	1.36	1.27	1.30	1.22	1.18
	(.62)	(.71)	(.66)	(.59)	(.66)	(.70)
12. Consistency of party ID	.63	.67	.60	.62	.65	.61
	(.46)	(.44)	(.40)	(.39)	(.39)	(.40)

Note: Means for stability scores are cross-wave comparisons, so the first represents wave 1 to wave 2 score, and the second represents wave 2 to wave 3 score. High scores represent more affect and information and less crystallization. Standard deviations are in parentheses.

of the ten interaction terms is significant. Nor did the preadult-adult gap diminish systematically in the less salient attitude domains. Inspection of the correlation data reveals no systematic reduction in this gap, which declined in five cases and either expanded or showed no change in five others.[20] Turning to the individual-level data shown in Table 2, one of the ten interaction terms yielded significant changes in the direction that would indicate a positive campaign effect, and two moved in the opposite direction.

So the campaign produced important preadult socialization gains in the party and candidate domains. These gains succeeded in substantially reducing, but not eliminating, preadults' deficit in party identification relative to adults, most likely because the parties were the only attitude objects which were both central foci of the campaign and sufficiently longstanding that adults' attitudes were unlikely to change much. The campaign did not reduce the gap as greatly with regard to the candidates. The adults also seem to have gained somewhat more crystallized partisan attitudes, perhaps because the presidential candidates are episodic objects, often initially somewhat novel even to adults, at least by comparison to the older and more familiar established political parties. And in other domains, or outside the campaign period, the preadult-adult gap did not change materially.

SUMMARY AND CONCLUSION

This paper presented and tested three basic ideas about the preadult acquisition of a stable political predisposition. First, it often is acquired and strengthened through intensive exposure to political events, which in essence provide a catalyst for mass preadult political socialization. Second, such events are selective: They make certain attitude objects salient and socialize predispositions toward them, while attitudes toward other objects lie dormant, without further socialization. Third, since potentially socializing events tend to be periodic rather than continuous, political socialization may typically occur in bursts, during a period when political events make particular attitude objects salient, rather than through the gradual and incremental accretion of experience.

U.S. presidential campaigns are prototypical cases of such socializing political events. Because they provide unusually intensive political communication (both interpersonally and through the mass media), they represent important socialization opportunities. Indeed, in this study, the campaign period produced substantial socialization gains in the attitude domains most salient in the campaign, as reflected in greater affective expression, information, and attitude crystallization. But there were generally no such gains in less central domains, and considerably less change occurred during a comparable period following the campaign. The campaign reduced the initial gap between preadults and adults in party identification—a domain in which adults had little room for further improvement (presumably because of their long prior experience)—but not in more peripheral attitude domains. It also did not materially reduce the cohort gap in candidate evaluations, perhaps because most candidates are initially less familiar to most adults than are the parties, and therefore campaigns are likely to crystallize even adults' attitudes toward the candidates.

---

[20] Seven of these are shown in figures 4 and 5. For the others, the gap in ideological consistency and consistency of party identification with issue positions expanded, and it contracted in consistency of party with ideology.

Methodological Issues

Several methodological considerations deserve comment. First, the sample was not intended to be nationally representative, but it was not especially unusual, either. Wisconsin voted close to the national average in the 1980 presidential election, as it often does. As indicated earlier, the adult sample resembled the general population samples in the NES surveys of that period, aside from the usual biases of telephone surveys. Behaviorally, the adult respondents also closely resembled those in the NES 1980 panel study. The stability of our adults' party identifications from wave 1 to wave 2 was .88 (using a Pearson r), whereas in the NES it was .85 over the February to June span and .88 from June to September (Markus 1982). Similarly, scale scores for the partisanship of adults' candidate evaluations during the campaign were about as stable ($r = .67$) as were the stability of evaluations of Ted Kennedy (.72) or Richard Nixon (.58) in the NES panels during 1972–76 (Converse and Markus 1979).

Selective sample attrition is always a potential threat in panel studies, but here it would not explain the main findings well, given the differential change across domains and differential change across periods. And one could imagine that simply responding to the wave 1 interviews could itself crystallize attitudes, especially for politically naïve preadults, but this does not seem to have occurred to any great degree. There is no reason to expect reinterview effects to occur selectively across domains, and we find socialization gains only in domains central to the campaign. This lack of an interviewing effect is perhaps not surprising, given that the interviews were brief (half-hour) experiences separated by nearly a year.

Research on attitude stability and constraint has conventionally relied on aggregate-level correlations across individuals. We supplemented that treatment with assessment of within-individual stability or consistency. Since the arithmetic bases for the two measurement approaches are quite different, it is not surprising that they differ on occasion. Despite the lack of a standard, time-tested technique for the individual-level approach, however, the two yield closely parallel results. Among the large number of effects tested (there are 94 significance tests in tables 1 and 2), there are clear discrepancies in only three cases. This rate seems well within the bounds of chance, and in any case none is central to the main findings of the study.[21]

The attitude instability found in any panel study can potentially be due to change in real prior attitudes, a genuine lack of attitude crystallization, or to mere measurement unreliability (Alwin and Krosnick 1991, Krosnick 1991). Could the lower levels of attitude stability we find initially among preadults be due to lower measurement reliability? If so, then their improvement over time may be partly artifactual. Any possible unreliability-driven artifacts may have come about in four different ways. First, some other studies of changes over time in attitude consistency and stability have been compromised by the use of different items in each wave (see Smith 1989; Sullivan, Pierson, and Marcus 1978). This is not at issue here because we have used identical items across waves. Second, some types of response scales are inherently less reliable than others, which may artifactually generate differences across attitude domains (see Krosnick 1991). This seems unlikely here. The party and ideology items were very similar in construction but yielded quite different campaign effects. The items in the remaining domains almost all used simple five-point response scales (agree-disagree or like-dislike). Third, our aggregate-level correlational measures of stability are based on the test-retest correlations of composite scales. Apparent instability therefore may reflect mere scale unreliability and not the instability of individual attitudes. The individual-level stability measurers are based on individual items, however, not composite scales, and the findings are almost identical.

Finally, instability may simply reflect measurement unreliability at the item level. To check on this possibility, we recalculated our correlational stability estimates using the Wiley and Wiley (1970) procedure for estimating stability free of differential reliability (also see Heise 1969). The absolute levels of stability using the correction are quite a bit higher than the uncorrected stability correlations. The corrected stability estimates for preadults' candidate evaluations are .71 for wave 1 to wave 2 and .96 for wave 2 to wave 3 (as opposed to .40 and .62 for the uncorrected estimates). For party identification, the corrected stability estimates are .70 for wave 1 to wave 2 and .83 for wave 2 to wave 3 (as opposed to .54 and .70 for the uncorrected estimates). But none of the essential *differences* between periods or cohorts changes more than a hair. The campaign effect on candidate evaluations is .25 for the corrected and .22 for the uncorrected estimates; comparable effects for party identification are .13 and .16, respectively. The main findings in our two key domains, then, remain essentially unchanged using the Wiley and Wiley correction.[22]

---

[21] First, the consistency of preadults' wave 1 and wave 2 party identification with their issue preferences did not differ according to the correlational data ($r = .12$ in both cases; not shown in the figures), but the analysis of variance shows a significant decrease (see Table 2). Second, according to the correlations, the stability of the preadults' civil liberties tolerance does not change over our two periods; third, nor does the preadult-adult gap (see Figure 5); but both significantly improve according to the individual-level measures (see Table 2). In the first case, a lack of change would follow our theoretical expectations, while a postcampaign decline would simply be irrelevant to them. In the last two cases, the correlational results support our hypotheses, while the individual-level results do not. But even accepting the latter yields no systematic pattern of campaign-induced crystallization in the peripheral domains; the key finding is that there are actually slightly more significant decreases than increases over the campaign period (see columns 2 and 4 of Table 2). In short, no matter which of these discrepant findings is accepted, the overall pattern is the same: a clear lack of campaign effect in peripheral domains.

In short, it seems unlikely that the initially lower levels of attitude stability for preadults were due to measurement unreliability; they reflect genuinely lower levels of attitude crystallization. Although preadults' partisan precampaign opinions often reflected mere nonattitudes, by the end of the campaign they were often closer to the genuine article. Of course, we cannot be certain that the markedly higher levels of stability in preadults' partisan attitudes would themselves foreshadow longer term persistence. But the wave 2/wave 3 stability in preadults' party identification, corrected for unreliability, was .83 over one year, which is very high indeed in absolute terms.

Was 1980 a "typical" presidential year? By and large, it was not very unusual. There were strong primary contests in both parties, the incumbent came under intense attack for ineffective performance, the parties polarized ideologically, there was an initially strong third party candidacy that ultimately did not have much effect on the outcome, and the election did not materially shift party loyalties, though there was a discernible and decisive last-minute surge to the winner (Markus 1982).

The year following the campaign was somewhat less typical. We assumed that the campaign period would provide a more intensive flow of partisan information than would the postcampaign year, but in 1981 the incoming Reagan administration pressed its conservative agenda with unusual vigor. This got through to the general public to some extent, as shown in the increasing accuracy of party issue-placement through the postcampaign period in both cohorts (Figure 2). Nevertheless, the bulk of our evidence suggests that the 1980 campaign period had a more profound effect than did the 1981 postcampaign period, as we would expect to be true in most years. The unusual activity after the election actually makes 1980 a conservative test of our essential hypothesis, working against our supposition that politics will resume its normally rather low visibility to preadults after a campaign.

We also assumed that campaigns do not themselves normally induce major changes in adults' party identification. The Reagan and Bush presidencies did coincide with an important shift toward the Republicans, but the NES data suggest that did not happen during 1980 or from the 1980 preelection interview to the 1982 postelection interview (Abramson, Aldrich, and Rohde 1995; Markus 1982). Adult party identification was very stable at the individual level, as Figure 3 shows. Rather, the shift toward the GOP seems to have occurred primarily in the period following the 1984 election (Miller and Shanks 1996).

The Dynamics of Event-Driven Socialization

Political events can provide "occasions for socialization" in various ways, including direct contact with a campaign (such as bumper stickers or rallies), contact mediated through mass communications (as in news interviews, advertising, or debates), or contact mediated by interpersonal relationships (such as schoolroom mock elections, causal "who are you for?" queries from friends, or discussions with parents, perhaps themselves stimulated by shared exposure to television programming). One possible consequence of such indirect interpersonal exposure is that the "occasion" may promote greater agreement with parents and peers, especially among preadults with heavier exposure to the campaign (Valentino and Sears 1994).

Does crystallization proceed through acquisition of information, as suggested by the term "information mass," or by some more affective process, as implied by the term "affective mass" (e.g., Converse 1962, Sears 1983, Zaller 1992)? Our data suggest that information is not enough by itself. Preadults were clearly deficient in both information and attitude crystallization prior to the campaign, both of which increased during the campaign. But their information continued to increase after the campaign, without much further crystallization; the consistency of partisan attitudes increased only slightly during the year following the election. Affect also does not seem to be enough by itself: Even prior to the campaign, preadults came close to adult levels of expressing partisan affect, but they were far short in attitude crystallization. Instead, we speculate that the fuss and flurry of the campaign, with debate and discussion and disagreement, bound both affect and cognition more tightly together. Real attitude crystallization may depend on this "affective bonding" (Dennis, Chaffee, and Choe 1979) or increased affective-cognitive consistency (Chaiken, Pomerantz, and Giner-Sorolla 1995) rather than on more facts or the expression of more intense opinions about still more objects.

Describing socialization as "event-driven" implies that life stage is important primarily because it indexes personal experience, not because of maturational processes such as cognitive development. Two findings support this more experientially based account. First, we repeated all our analyses comparing the younger (age 10–13) with older (age 14–17) preadults. The data can be summarized quickly. The younger preadults began the campaign at a significantly lower level of socialization on every dimension, but the two age groups responded in parallel in all respects. In the two partisan domains, both gained in parallel as a result of the campaign. And neither changed in either the less visible domains or the postcampaign period. The precampaign age gap, therefore, is most likely due to the greater prior political experience of the older preadults. The parallel responses of the two age groups to these political events are most likely due to their similar political experiences during the span of this study. Development stage seems of less importance.

---

[22] Concretely, the Wiley and Wiley (1970) correction had only trivial effects on the wave and cohort effects. Among adults, the correction decreases by .01 the campaign effect in the stability of candidate evaluations and increases by .01 the effect in stability of party identification. Across the six less visible domains, the Wiley and Wiley correction reduced the difference in stability between the campaign and postcampaign period by an average of .02 for preadults and adults alike. Thus, these domains show no campaign effects in either the uncorrected or corrected data.

Differential experience, rather than developmental stage, also seems to account best for the trajectory of the preadult-adult gap. Both preadults and adults began the campaign with only modestly crystallized candidate evaluations, and both advanced in parallel through the campaign, leaving intact the preadult shortfall. In contrast, both groups began the campaign with more advanced party identification, presumably because of more past experience, with the adults close to asymptote. Adults did not advance through the campaign, while preadults' new experience helped reduce their shortfall. So the fate of the adult-preadult gap, too, seems to have depended more on life experience than on developmental stage.

We have treated a presidential campaign as one instance of the more general category of socialization-triggering political events. Is it a special case or typical? Both, in some respects. According to our theory, the most crystallized attitudes ought to develop toward the most recurrently visible objects on the public agenda (Sears 1983). That recurrent visibility is in some cases quite predictable. Some objects are guaranteed recurrent public attention; partisanship, for example, is placed before the public at predictable intervals because of the regular electoral cycle. Disruptions in the temporal cycle or changes in the party system should tend to disrupt the routine socialization of partisan preferences (Converse 1969). But other attitude objects may appear on the public agenda frequently and recurrently because of events that have nothing to do with the rhythm of institutional life or elite manipulation. The chronic racial tensions left by the legacies of slavery and a century of enforced second-class citizenship after Emancipation virtually guarantee racial issues a permanent place in the nation's political attention and, consequently, strongly socialized racial attitudes in the mass public (Sears 1983, Sears and Funk 1996).

Other attitude objects may become highly salient and leave strong socialization residues, despite their lack of recurrence. Such events as the French Revolution, the Great Depression, World War II, the civil rights movement, the Kennedy assassination, ghetto riots, the Vietnam War, or the antibusing or antiabortion protests in many cities seem to have had such effects (see Campbell et al. 1960; Centers 1950; Elder 1974; Markus 1979; Marwell, Aiken, and Demarath 1987; Sears and McConahay 1973; Wolfenstein and Kliman 1965). Jennings' (1987) careful empirical analysis of long-term attitude persistence among 1960s student protestors suggest exactly the selectivity we would expect: They remained firmly opposed to the Vietnam War, the centerpiece event, but drifted toward more moderate positions on less central issues, such as unemployment policy and the criminal justice system.

These processes may help explain the familiar domain differences in attitude stability, such as the greater stability of party identification than of ideological or issue positions (Converse 1964, Converse and Markus 1979, Sears 1983). In our data, the campaign enlarged such domain differences in preadults, further crystallizing their party identification but not their ideologies or issue partisanship. This finding suggests that the usual advantage to party identification results in part from the lasting socializing effect of a regular partisan electoral cycle. Other attitude objects simply do not receive the benefit of such regular and intense communication.

On Persistence Versus Openness

What is the implication of these findings for the debates between revisionists and counterrevisionists and, more generally, between more psychological and rational choice theories? We believe our data are persuasive in tracing back to the early years of adolescence a stable, inertial component of party identification whose origins are partially occasioned by the political events of the day. By the end of the campaign, preadults had a rather crystallized party identification; in most cases they did not have nonattitudes, by any test available to us.

This is contrary to at least the spirit of the "revisionist" critique and its general empirical emphasis. That assumes considerable potential for change in adults' party identification, focusing on their responsiveness to macroeconomic changes, the perceived performance of the parties, the emergence of new issues, or campaign events. Our emphasis, instead, is on the periodic leaps in crystallization of party identification through the first half of the life cycle and on considerable stability thereafter. In our data, party identification was more crystallized among the older preadults than among their younger counterparts, but both had room to crystallize further and did so in parallel. Party identification was considerably more crystallized among adults than in either set of preadults and, having little room to move further, underwent relatively little change during the campaign.

Nevertheless, our theory allows for later change in three ways. First, the campaign diminished the preadult-adult gap in the crystallization of party identification, but the gap did not completely disappear. Normally, that gap continues to diminish through the postadolescent years, and party identification tends to be highly stable after early adulthood (Jennings and Markus 1984). But the long-term persistence of such early attitudes is a variable, not a given, and it depends in part on continuity of the individual's political experience after adolescence (Alwin, Cohen, and Newcomb 1991; Converse 1969; Miller and Sears 1986; Miller and Shanks 1996; Niemi and Jennings 1991).

Second, new events will always make new issues salient later in life. Such issue turnover may, over time, induce some change in early predispositions. Indeed, there is evidence that as young adults age, their party identification becomes more closely associated with their own issue preferences, at the expense of a link to parental party identification (Beck and Jennings 1991, Markus 1979, Niemi and Jennings 1991). External events may accelerate that process, serving as "occasions for change" in adulthood, as they apparently did during the Civil War or the Great Depression (even though they may serve more often as reinforcing occasions for maintenance). Miller and Shanks (1996) argue that the events of the 1960s had major effects by intruding on the standard socialization process, both interrupting youthful adoption of partisan loyalties and instigating a long-term realignment of southern whites. This intrusion of catalytic political events may be the dynamic by which the persistence of preadult party identification declines in a dealigning or realigning period (Converse 1969).

But even when events cause substantial aggregate attitude changes among adults, individuals may not be rejecting previously socialized attitudes wholesale. As Green and Palmquist (1994) have suggested, individual changes may respond to prevailing political winds in correlated fashion. For example, Richard Nixon dramatically fell from public favor as a result of the Watergate scandal, but individuals' postresignation evaluations of him were highly correlated with their prescandal attitudes (Converse and Markus 1979). Similarly, Ward (1985) found strong correlations between parents' racial attitudes and those of their adult offspring some decades later—but with their specific content updated, the offspring were exercised not by housing desegregation, as their parents had been, but by busing. Presumably in both cases, external events produced individual-level attitude changes, but these were layered on top of a powerful inertial component dating from earlier experience.

We hope to have shown, then, that preadult political socialization is much influenced by real political events. The example we have used is the effects of a hotly contested presidential campaign on adolescents' attitudes. Within that context, the data seem to indicate that preadults progressed to genuine and stable partisan attitudes over the course of the campaign, with little change in the politically more fallow period of the postcampaign year. Our argument is intended to be a more general one, however, and we would expect similar effects to be found when other highly visible and contested events come forcefully to preadults' attention over a substantial time period.

APPENDIX: MEASUREMENT AND SCALE CONSTRUCTION

Partisan Information

Candidate Information. The mean number of five candidates correctly assigned to their party. "Please tell me if you now think of him as a Republican or as a Democrat." (Jimmy Carter, Ronald Reagan, George Bush, Ted Kennedy, John Connally).

Party Issue-Placement. The mean number of four issues on which the respondent correctly identified the party most identified with a particular position: "Which of the parties do you think is more in favor of (cutting down government spending and services; protecting the environment; giving women and minorities special treatment in getting jobs; spending more money for the armed forces and defense), the Republicans or the Democrats?"

Party Symbol Information. The mean number of fourteen party symbols correctly assigned to each party: "When I read each of these names or things, which party comes most to your mind—the Republicans or the Democrats?" (elephant, right of center, rich people, Abraham Lincoln, Richard Nixon, conservative, business, donkey, Franklin D. Roosevelt, liberal, labor union, poor people, Lyndon B. Johnson, left of center).

Attitude Items

Candidate Evaluations. Like-dislike on a five-point scale of (1) Jimmy Carter, (2) Ronald Reagan, (3) George Bush, and (4) Edward Kennedy: "Now I am going to ask you which candidates you like or dislike in the upcoming presidential election. For each candidate I name, tell me if you like him a lot, like him a little, dislike him a little, or dislike him a lot. If you don't know anything about him, just say so. First: how much do you like or dislike . . ." Respondents were given an option for like and dislike, as well as don't know.

Party Identification. (1) the standard NES party identification item; (2) the Dennis (1986) revision: "Do you ever think of yourself as a Republican or as a Democrat?" (If yes) "Which political party—the Republican or the Democratic—do you favor?" and "In your own mind, are you a very strong, fairly strong, or not a strong supporter of this party?" (If no) "Are you closer to the Republican or to the Democratic party?"; (3) trust in the two parties: "Now I'll name some groups and organizations that are active in politics and government. For each one, please tell me how often you think you can trust it or them to do what you feel is right. Can you almost always trust it to do what is right, can you trust it most of the time, about half the time, not very often, or almost never?" "The Republican Party?" "The Democratic Party?"

Political Ideology. (1) "When it comes to politics, do you ever think of yourself as being liberal, middle-of-the-road, conservative, or something like that?" (If yes) "Would you say that you are a very strong, fairly strong, or not a strong (liberal/conservative)?" (If no or middle-of-the-road) "If you had to choose, would you call yourself liberal, or conservative?"; (2) trust in liberals (see above); and (3) trust in conservatives (see above).

Issue Partisanship. Preferences on five campaign issues, with five-point agree-disagree response scales: (1) "Protecting our environment is more important than producing more energy." (2) "Women should be given *special* treatment in getting jobs." (3) "The government should spend less money on national defense and the armed forces." (4) "The government should spend less money on things like health and education." (5) "The government should build more nuclear power plants to produce electricity."

Racial Tolerance. (1) "Black people should be given special treatment in getting jobs." (2) "Over the past few years, blacks have gotten more than they deserve." (both with five-point agree-disagree scales) (3) "How about members of racist groups, like the Ku Klux Klan and the Nazi party . . . do you have no particular feelings about them, do you somewhat dislike them, or do you dislike them a lot?" (4) "Do you think a member of a racist group should be allowed to run for president?" (5) "Should a member of a racist group be allowed to make a speech in your community attacking other people's beliefs?" (6) "Should a member of a racist group be allowed to teach in a high school in your area?" (all with yes–no–don't know response alternatives).

Civil Liberties Tolerance. Six civil liberties items, using the last three from the racial tolerance scale and three similar items regarding Communists.

Trust in Government. Items on trust (see above) in (1) the U.S. Congress, (2) the state government in Madison, (3) the U.S. Supreme Court, (4) the government in Washington; and (5) whether government is run by a few big interests or for the benefit of all the people?

Internal Political Efficacy. Three NES agree-disagree items: (1) one person's vote does not matter, (2) the ordinary American *ought* to have a voice in what the government in Washington does, and (3) everyone who wants to *does* have a voice in what the government decides to do.

Partisan Affect

Opinionation. Each of the above attitude items dichotomized so that any response is coded 1, with "don't know" and other nonresponse categories coded 0.

Opinion Intensity. Extreme responses on any item are given a high value, moderate responses intermediate value, and undecideds coded as 0. All items are then averaged.

Attitude Crystallization: Aggregate-Level Correlational Measures

In each domain, attitude scales were constructed using the items just described, recoding all items in the same partisan direction where necessary and averaging across items. The specific items included in each scale were chosen a priori based on their manifest content, except for the political trust and internal political efficacy scales, which were developed from a factor analysis of wave 1 adult data on all system support items. If data were missing for half or more of the items on a given scale for a given respondent in a given wave, the respondent was considered missing for that scale and wave only.

Consistency of response to items within a domain was indexed with Cronbach's alpha. For the adults in wave 1, they were: candidate evaluations .31, party identification .87, campaign issues .45, ideology .64, racial tolerance .77, civil liberties tolerance .59, political trust .60, and internal efficacy .36. *Stability* of attitudes in the domain was indexed with the Pearson' correlation for its scale either from wave 1 to wave 2 or from wave 2 to wave 3.

Attitude Crystallization: Individual-Level Measures

A *consistency* score was computed for each individual at each wave. The absolute deviation of each item from the individual's scale score was summed and averaged.

A *stability* score was computed for each individual across adjacent waves by summing and averaging the absolute differences of responses to each item across two waves. It should be noted that the resulting distributions were highly skewed by a few cases in each domain, since most respondents were reasonably stable, and only a few jumped from one extreme to the other. To even out the effect of outlying cases, a range restriction was imposed. The cutoff points were determined by inspection of the distributions within the wave 1 adult data, without inspection of the other distributions. The scores for a small number of unstable outliers were reduced to a maximum value as far from the median on the unstable side as were the most stable scores on the stable side. This somewhat arbitrary algorithm normalized the distribution of these scales. It served to make the most extreme cases only slightly less extreme, with the recoded values still given the highest (most unstable) score possible for that scale. On average, 4% of the cases were so recoded in each domain.

REFERENCES

Abramson, Paul R., John H. Aldrich, and David W. Rohde. 1995. *Change and Continuity in the 1992 Elections*. Washington, DC: Congressional Quarterly, Inc.

Allsop, Dee, and Herbert F. Weisberg. 1988. "Measuring Change in Party Identification in an Election Campaign." *American Journal of Political Science* 32 (November):996–1017.

Alwin, Duane F., Ronald L. Cohen, and Theodore M. Newcomb. 1991. *Political Attitudes over the Life Span: The Bennington Women after Fifty Years*. Madison: University of Wisconsin Press.

Alwin, Duane F., and Jon A. Krosnick. 1991. "Aging, Cohorts, and the Stability of Socio-political Orientations over the Life Span." *American Journal of Sociology* 97 (July):169–95.

Barton, Allen H., and R. Wayne Parsons. 1977. "Measuring Belief System Structure." *Public Opinion Quarterly* 41 (Summer):159–80.

Beck, Paul A., and M. Kent Jennings. 1991. "Family Traditions, Political Periods, and the Development of Partisan Orientation." *Journal of Politics* 53 (August):742–63.

Brody, Richard A., and Lawrence S. Rothenberg. 1988. "The Instability of Partisanship: An Analysis of the 1980 Presidential Election." *British Journal of Political Science* 18 (4):445–65.

Campbell, Angus, Philip E. Converse, Warren E. Miller, and Donald E. Stokes. 1960. *The American Voter*. New York: Wiley.

Centers, Richard. 1950. "Children of the New Deal: Social Stratification and Adolescent Attitudes." *International Journal of Opinion and Attitude Research* 4 (3):315–35.

Chaffee, Steven H., and Yuko Miyo. 1983. "Selective Exposure and the Reinforcement Hypothesis: An Intergenerational Panel Study of the 1980 Presidential Campaign." *Communication Research* 10 (1):3–36.

Chaffee, Steven H., and Joan Schleuder. 1986. "Measurement and Effects of Attention to Media News." *Human Communication Research* 13 (Fall):76–107.

Chaffee, Steven H., and Albert R. Tims. 1982. "News Media Use in Adolescence: Implications for Political Cognitions." In *Communication Yearbook*, ed. M. Burgoon. New Brunswick, NJ: Transaction. pp. 736–58.

Chaiken, Shelly, Eva M. Pomerantz, and Roger Giner-Sorolla. 1995. "Structural Consistency and Attitude Strength." In *Attitude Strength: Antecedents and Consequences*, ed. Richard E. Petty and Jon A. Krosnick. Mahway, NJ: Lawrence Erlbaum Associates, Inc.

Citrin, Jack, and Donald P. Green. 1986. "Presidential Leadership and the Resurgence of Trust in Government." *British Journal of Political Science* 16 (4):431–53.

Converse, Philip E. 1962. "Information Flow and the Stability of Partisan Attitudes." *Public Opinion Quarterly* 26 (Winter):578–99.

Converse, Philip E. 1964. "The Nature of Belief Systems in Mass Publics." In *Ideology and Discontent*, ed. David E. Apter. New York: Free Press of Glencoe.

Converse, Philip E. 1969. "Of Time and Partisan Stability." *Comparative Political Studies* 2 (2):139–71.

Converse, Philip E. 1970. "Attitudes and Non-attitudes: Continuation of a Dialogue." In *The Quantitative Analysis of Social Problems*, ed. Edward R. Tufte. Reading, MA: Addison-Wesley.

Converse, Philip E., and Gregory B. Markus. 1979. "Plus ça Change . . . : The New CPS Election Study Panel." *American Political Science Review* 73 (March):32–49.

Dennis, Jack. 1986. "Preadult Learning of Political Independence: Media and Family Communication Effects." *Communication Research*, 13 (3):401–33.

Dennis, Jack, Steven H. Chaffee, and Sun Yuel Choe. 1979. "Impact on Partisan, Image, and Issue Voting." In *The Great Debates: Carter vs. Ford 1976*, ed. S. Kraus. Bloomington: Indiana University Press.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper & Row.

Elder, Glen H., Jr. 1974. *Children of the Great Depression*. Chicago: University of Chicago Press.

Fiorina, Morris P. 1981. *Retrospective Voting in American National Elections*. New Haven, CT: Yale University Press.

Fiorina, Morris P. 1996. "The Electorate at the Polls in the 1990s." In *The Parties Respond: Changes in American Parties and Campaigns*, ed. L. Sandy Maisel. Boulder, CO: Westview Press.

Franklin, Charles H. 1984. "Issue Preferences, Socialization, and the Evolution of Party Identification." *American Journal of Political Science* 28 (August):459–78.

Green, Donald P., and Brad Palmquist. 1994. "How Stable Is Party Identification?" *Political Behavior* 16 (December):437–66.

Heise, David R. 1969. "Separating Reliability and Stability in Test-Retest Correlations." *American Sociological Review* 34 (1):93–101.

Hess, Robert D., and Judith V. Torney. 1967. *The Development of Political Attitudes in Children*. Chicago: Aldine.

Hyman, Herbert H. 1959. *Political Socialization*. Glencoe, IL: Free Press.

Jennings, M. Kent. 1987. "Residues of a Movement: The Aging of the American Protest Generation." *American Political Science Review* 81 (June):367–82.

Jennings, M. Kent, and Gregory B. Markus. 1984. "Partisan Orientations over the Long Haul: Results from the Three-wave Political Socialization Panel Study." *American Political Science Review* 78 (December):1000–18.

Jennings, M. Kent, and Richard G. Niemi. 1981. *Generations and Politics*. Princeton: Princeton University Press.

Kennamer, J. David, and Steven H. Chaffee. 1982. "Communication of Political Information during Early Presidential Primaries: Cognition, Affect, and Uncertainty." In *Communication Yearbook*, ed. M. Burgoon. New Brunswick, NJ: Transaction. Pp. 627–50.

Key, V. O., Jr. 1966. *The Responsible Electorate*. Cambridge, MA: Harvard University Press.

Krosnick, Jon A. 1991. "The Stability of Political Preferences: Comparisons of Symbolic and Nonsymbolic Attitudes." *American Journal of Political Science* 35 (August):547–76.

Krosnick, Jon A., and Richard E. Petty. 1995. "Attitude Strength: An Overview." In *Attitude Strength: Antecedents and Consequences*, ed. Richard E. Petty and Jon A. Krosnick, Mahwah, NJ: Lawrence Earlbaum Associates, Inc.

Mannheim, Karl. 1952. "The Problem of Generations." In *Essays on the Sociology of Knowledge*, ed. P. Kecskemeti. London: Routledge & Keagan Paul.

Markus, Gregory B. 1979. "The Political Environment and the Dynamics of Public Attitudes: A Panel Study." *American Journal of Political Science* 23 (May):338–59.

Markus, Gregory B. 1982. "Political Attitudes during an Election Year: A Report on the 1980 NES Panel Study." *American Political Science Review* 76 (September):538–60.

Markus, Gregory B. 1983. "Dynamic Modeling of Cohort Change: The Case of Political Partisanship." *American Journal of Political Science* 27 (November):717–39.

Markus, Gregory B. 1992. "The Impact of Personal and National Economic Conditions on Presidential Voting, 1956–1988." *American Journal of Political Science* 36 (August):829–34.

Marsh, David. 1971. "Political Socialization: The Implicit Assumptions Questioned." *British Journal of Political Science* 1 (4):453–65.

Marwell, Gerald, Michael T. Aiken, and N. J. Demerath III. 1987. "The Persistence of Political Attitudes among 1960s Civil Rights Activists." *Public Opinion Quarterly* 51 (Fall):359–75.

Miller, Steven, and David O. Sears. 1986. "Stability and Change in Social Tolerance: A Test of the Persistence Hypothesis." *American Journal of Political Science* 30 (February):214–36.

Miller, Warren E. 1991. "Party Identification, Realignment, and Party Voting: Back to the Basics." *American Political Science Review* 85 (June):557–68.

Miller, Warren E., and J. Merrill Shanks. 1996. *The New American Voter*. Cambridge, MA: Harvard University Press.

Niemi, Richard G., and M. Kent Jennings. 1991. "Issues and Inheritance in the Formation of Party Identification." *American Journal of Political Science* 35 (November):970–88.

Owen, Diana, and Jack Dennis. 1987. "Preadult Development of Political Tolerance." *Political Psychology* 8 (4):547–62.

Owen, Diana, and Jack Dennis. 1988. "Gender Differences in the Politicization of American Children." *Women & Politics* 8 (2):23–43.

Owen, Diana, and Jack Dennis. 1992. "Sex Differences in Politicization: The Influence of Mass Media." *Women & Politics* 12 (4):19–41.

Patterson, Thomas E. 1980. *The Mass Media Election: How Americans Choose Their President*. New York: Praeger.

Pelham, Brett. 1993. "The Idiographic Nature of Human Personality: Examples of the Idiographic Self-concept." *Journal of Personality and Social Psychology* 64 (April):665–77.

Petrocik, John R., Sidney Verba, and Christine Schultz. 1981. "Choosing the Choice and Not the Echo: A Funny Thing Happened to *The Changing American Voter* on the Way to the 1980 Election." Presented at the 1981 annual meeting of the American Political Science Association, New York.

Petty, Richard E., and Jon A. Krosnick, eds. 1995. *Attitude Strength: Antecedents and Consequences*. Mahwah, NJ: Lawrence Erlbaum Associates, Inc.

Plotkin, Henry A. 1981. "Issues in the Presidential Campaign." In *The Election of 1980: Reports and Interpretations*, ed. Marlene Michels Pomper. Chatham, NJ: Chatham House Publishers, Inc. Pp. 38–64.

Searing, Donald D., Joel J. Schwartz, and Alden E. Lind. 1973. "The Structuring Principle: Political Socialization and Belief Systems." *American Political Science Review* 67 (June):415–32.

Sears, David O. 1975. "Political Socialization." In *Handbook of Political Science*, vol. 2, ed. Fred I. Greenstein and Nelson W. Polsby. Reading, MA: Addison-Wesley.

Sears, David O. 1983. "The Persistence of Early Political Predispositions: The Roles of Attitude Object and Life Stage." In *Review of Personality and Social Psychology*, vol. 4, ed. Ladd Wheeler and Philip Shaver. Beverly Hills, CA: Sage.

Sears, David O. 1989. "Whither Political Socialization Research? The Question of Persistence." In *Political Socialization, Citizenship Education, and Democracy*, ed. Orit Ichilov. New York: Teachers College Press.

Sears, David O. 1993. "Symbolic Politics: A Socio-psychological Theory." In *Explorations in Political Psychology*, ed. Shanto Iyengar and William J. McGuire. Durham, NC: Duke University Press.

Sears, David O., and Jack Citrin. 1985. *Tax Revolt: Something for Nothing in California*, enlarged ed. Cambridge, MA: Harvard University Press.

Sears, D. O., and C. L. Funk. 1996. "The Persistence and Crystallization of Political Attitudes over the Life-span: The Terman Gifted Children Panel." Presented at the annual meeting of the Society for Experimental Social Psychology, Sturbridge, MA.

Sears, David O., and John B. McConahay. 1973. *The Politics of Violence: The New Urban Blacks and the Watts Riot*. Boston: Houghton-Mifflin.

Smith, Eric R.A.N. 1989. *The Unchanging American Voter*. Berkeley, CA: University of California Press.

Sullivan, John L., James E. Piereson, and George E. Marcus. 1978. "Ideological Constraint in the Mass Public: A Methodological Critique and Some New Findings." *American Journal of Political Science* 22 (May):233–49.

Vaillancourt, Pauline M. 1973. "Stability of Children's Survey Responses." *Public Opinion Quarterly* 37 (Fall):373–87.

Valentino, Nicholas A., and David O. Sears. 1994. "Campaign Communication and the Preadult Socialization of Partisanship." Presented at the annual meeting of the Midwest Political Science Association, Chicago.

Ward, Dana. 1985. "Generations and the Expression of Symbolic Racism." *Political Psychology* 6 (March):1–18.

Wiley, David E., and James A. Wiley. 1970. "The Estimation of Measurement Error in Panel Data." *American Sociological Review* 35 (1):112–6.

Wolfenstein, Martha, and Gilbert Kliman, eds. 1965. *Children and the Death of a President*. Garden City, NY: Doubleday.

Wycoff, Mikel L. 1980. "Belief System Constraint and Policy Voting: A Test of the Unidimensional Consistency Model." *Political Behavior* 2 (Summer):115–46.

Zaller, John R. 1992. *The Nature and Origins of Mass Opinion*. New York: Cambridge University Press.

American Political Science Review Vol. 91, No. 1 March 1997

Political Institutions and Satisfaction with Democracy: A Cross-National Analysis of Consensus and Majoritarian Systems

CHRISTOPHER J. ANDERSON *Rice University and Northwestern University*
CHRISTINE A. GUILLORY *Rice University*

Do political institutions affect citizen satisfaction with democracy? If so, how? Using cross-sectional survey data for eleven European democracies together with data on the type of democracy in which individuals live, we demonstrate that the nature of representative democratic institutions (measured by Arend Lijphart's consensus-majority index of democracies) mediates the relationship between a person's status as part of the political minority or majority and his or her satisfaction with the way the system works. Specifically, we find that (1) the losers of democratic competition show lower levels of satisfaction than do those in the majority and (2) losers in systems that are more consensual display higher levels of satisfaction with the way democracy works than do losers in systems with majoritarian characteristics. Conversely, winners tend to be more satisfied with democracy the more a country's political institutions approximate pure majoritarian government.

Democracy is about winning and losing at election time. Yet, democratic governance is also about how the political system deals with the winners and losers of democratic contests after the election is over. Because some political systems compensate the minority while others allow the majority to implement policies unchallenged, the extent to which either group is satisfied with the workings of democratic governance varies systematically by type of democracy. Put differently, because winning and losing have different consequences as a result of differently structured democratic institutions, the way people feel about the way democracy works is affected by the kind of system in which they live.

In our view, there are two key elements to understanding attitudes toward the way democracy works. First, support for the system is influenced by whether people are part of the majority or minority, that is, whether they are among the winners or losers in electoral contests. Individuals who belong to the political majority are more likely to be satisfied with the way democracy works than are those in the minority. Second, and most important for our attempt to construct an explanation of attitudes toward democratic government that takes institutions into account, we posit that a country's political institutions and constitutional reality systematically mediate attitudes about the democratic process among winners and losers.

We contend that there are identifiable and relatively stable features of democratic life which serve to organize and constrain citizens' political experiences and which allow them to develop attitudes about the workings of the political system. People form attitudes about politics in systemic contexts whose institutional structures mediate preferences, define the choices available, and provide citizens with opportunities to be heard in the political process (Powell 1982, 1989). One feature of democratic life that is important for citizen attitudes toward the system is how democratic institutions treat those in the political majority and those in the minority.

Drawing on the insights generated by Arend Lijphart's work (1984, 1994) on the nature of democratic governance and representation, we examine how the nature of representative democracy influences the relationship between political minority and majority status, on the one hand, and satisfaction with democracy, on the other. On the basis of cross-national survey data and information about the nature of the democratic process in eleven European democracies, we argue and demonstrate empirically that consensual and majoritarian democratic institutions differentially and systematically affect citizen satisfaction with the way democracy works. We find that people on the losing side in an electoral competition show lower levels of satisfaction with the system than do those on the winning side. Moreover, there is an interaction between the institutional environment and a person's status as part of the political majority or minority. Losers in systems that are more consensual display higher levels of satisfaction with the way democracy works than do losers in systems with majoritarian characteristics. Conversely, winners tend to be more satisfied with democracy the more a country's political institutions approximate pure majoritarian government.

Our research links and extends two research traditions that seldom have been combined. First, we advance our understanding of the functioning of democratic institutions by showing how institutional differences intersect with electoral outcomes in conditioning citizen attitudes about the political system.

Christopher J. Anderson is Assistant Professor of Political Science, Rice University, Houston, TX 77005-1892, and Visiting Assistant Professor of Management, J. L. Kellogg Graduate School of Management, Northwestern University, Evanston, IL 60208-2001. Christine A. Guillory is a graduate student in Political Science, Rice University, Houston, TX 77005-1892.

We are grateful to James Alt, John Ambler, Raymond Duch, Mark Franklin, Carolyn Funk, and the anonymous reviewers for thoughtful comments on earlier ideas and drafts of this paper. Special thanks also go to Kathleen O'Connor and Carl Rhodes for their constructive criticisms and good advice. We are indebted to Arend Lijphart for supplying the institutional data and for providing valuable feedback. Finally, we thank Robert Adams and Patricia García-Monet for their help with computing and data management. The Eurobarometer data are available through the Inter-university Consortium for Political and Social Research (ICPSR).

Thus, we ask how institutions affect what people think about democracy. Second, we contribute to an understanding of the determinants of system support in democratic states by systematically combining a country's political institutions and individual-level independent variables across a number of democracies. We argue that we can better understand what and how citizens think about the way the system works if we combine information about individuals with information about the nature of representation in the system in which they live.

By combining the insights of different strands of research on system support, we follow M. Stephen Weatherford (1991, 252), who argues that "strengthening research on political legitimacy will depend on renewing efforts at middle-level theorizing, with the goal not of choosing between perspectives but of combining them more constructively." The combination of individual attitudes with systemic attributes advances our understanding of political support and the functioning of democratic systems because it allows for comparison across individuals, countries, and types of democracy.

The next section briefly reviews the role of attitudes toward democracy in research on the politics of Western democracies. Subsequently, we examine the role that political institutions play in such attitudes. We then develop a model of citizen satisfaction with democracy that includes political institutions and individual-level variables, such as economic performance evaluations and political interest. Using survey data from eleven European democracies, we test this model of citizen evaluations of the political system, and we assess the significance of our findings in light of theories of system support and satisfaction with democracy. We conclude by suggesting avenues for future research.

SYSTEM SUPPORT IN WESTERN DEMOCRACIES

Survey research has documented high enthusiasm for democratic ideals and democratic government among citizens in the newly established democracies of Central and Eastern Europe (Mishler and Rose 1996). Yet, there has not been a similar resurgence of positive attitudes toward democracy in the West in the aftermath of the Cold War. In fact, after the much-heralded victory of liberal democracy around the globe in 1989–90, satisfaction with democracy and intermediary political institutions declined considerably in West European democracies (Kaase and Newton 1995). One explanation for this downward trend contends that the disappearance of communism as an alternative form of government no longer allows incumbents in the West to cover up some important weaknesses of democratic political institutions (Kaase 1995). Since the end of the Cold War, the "easy" comparison with communism has given way to comparisons among the group of pluralist democracies with regard to economic performance and optimal structures of democratic organization (see also Moe and Caldwell 1994). Furthermore, economic difficulties—most notably high unemployment rates—have magnified the loss of enthusiasm for democratic politics among people in Western Europe because the shortcomings of democratic governance have been put in sharper relief than previously.

Concern with the optimal functioning of democratic institutions and citizen attitudes about them is not new in research on the politics of advanced industrial societies. It has long been assumed that low levels of citizen support can pose serious problems for democratic systems because both their functioning and maintenance are intimately linked with what and how people think about democratic governance (Lipset 1959; Powell 1982, 1986). Moreover, earlier research repeatedly observed apparent institutional weaknesses as important influences on citizen satisfaction with democratic politics. During the 1970s, scholars concluded that democratic systems were suffering from overload because they appeared increasingly incapable of dealing with an expansion in citizens' demands (Brittan 1975, Huntington 1974). Similarly, in the late 1970s and 1980s, analysts found that representative democracy was not responsive enough to demands articulated by a citizenry with increased participatory inclinations (Abramson and Inglehart 1995, Barnes and Kaase 1979, Dalton 1996, Jennings and van Deth 1989).

Researchers and commentators frequently view the ensuing dissatisfaction with political institutions among Western mass publics as resulting from institutional inadequacies, which presumably limit democracy's capacity to cope with citizens' demands. To reduce such institutional deficits, scholars argue for an extension of more direct forms of popular participation in democratic decision making (Barber 1984, Held 1987). As a result, along with documenting a trend toward higher levels of political sophistication and a shift toward political values that emphasize involvement in the democratic process (Abramson and Inglehart 1995, Dalton 1996, Inglehart 1990), they have more closely investigated opportunities for people to have input into the political process.

The connection between political institutions and citizen attitudes toward democracy is a subject of particular relevance to contemporary debates about democratic performance because it involves the extent to which those attitudes and, by implication, the potential for protest or instability are mediated by a country's political institutions. Such questions are relevant to our understanding of both established and new democracies. With the end of the Cold War, older democracies have to cope with new realities, and new democracies are attempting to establish and stabilize various forms of democratic government. We argue, however, that the opportunity to participate in the democratic process or the quality of political outputs constitutes only part of the link between institutions and attitudes in contemporary democracies. It is not clear that more opportunities for input and access to the system will automatically lead to higher levels of satisfaction with democracy; nor is it self-evident that particular types of democratic institutions lead to superior outputs

(Crepaz 1996). Because the same set of democratic institutions can have different consequences for different groups among those governed by them, and in particular for those in the political minority and majority, we explore how long-standing and institutionally defined differences across and within political systems mediate citizen support for the system.

WINNERS AND LOSERS IN CONSENSUAL AND MAJORITARIAN DEMOCRACY

In the absence of knowledge about the specific nature of the representative process in a democracy, we need to make a crucial distinction between different categories of citizens when it comes to attitudes toward the system: "Since the struggle for political office is bound to create winners and losers, this necessarily generates ambivalent attitudes towards authorities on the part of the losers" (Kaase and Newton 1995, 60; Nadeau and Blais 1993). Democracy is about winning and losing within the context of set rules to which those participating in political contests adhere. Thus, people who voted for a governing party—either *the* governing party or one among several in a governing coalition—are almost by definition more likely to believe that the government is interested in and responsive to their needs. They are inclined to be satisfied with the government's performance (Lambert et al. 1986) and with the way the system works (Citrin and Green 1986, Gabriel 1989, Kornberg and Clarke 1994, Kuechler 1986, Nadeau and Blais 1993). Put differently, because the political system is a friendlier place for people who identify with the governing party, we hypothesize that losers are less satisfied with the way democracy works than are winners.

Winning and losing mean different things in different political systems, however. In fact, some systems are designed to protect democratic minorities from unrestrained rule by the majority. Arend Lijphart's work (1984, 1994) on the nature of democratic systems serves as the basis for our theory of how political institutions affect satisfaction with the way democracy works. Lijphart analyzes constitutional reality across a number of Western democracies as well as the underlying factors that drive the operation of democracy in particular countries. On the basis of five factors—minimal winning cabinets, executive dominance, effective number of parties, number of issue dimensions, and electoral disproportionality—he develops a typology of democratic systems that places countries on a continuum from most consensual to most majoritarian in nature (Lijphart 1984; Lijphart et al. 1988).

The overarching principle that distinguishes majoritarian and consensual forms of democracy is the answer to the question: "Who rules?" The answer given by majoritarian systems is "the majority of the people," whereas in consensual systems it is "as many people as possible" (Lijphart 1984, 4). At the extremes, majoritarian government is about unfettered rule by the majority on the basis of an unwritten constitution without provisions for minority veto, whereas a pure form of consensus democracy is organized on the basis of a rigid, written constitution with formal veto powers for minorities.[1]

The classic example for the majoritarian model of democracy is the British Westminster model, whereas the Netherlands and Belgium typically are mentioned as prototypes of the consensual model. Under the British system, the winners of elections are in a strong position to implement their preferred policies largely unchallenged. Under one-party government and cabinet decision making in an environment of an unwritten constitution, asymmetric bicameralism (strong House of Commons, weak House of Lords), unitary and centralized government, plurality electoral rules, and no room for elements of direct democracy, the majority rules, and the minority opposes. In contrast, political systems like those found in the Netherlands and Belgium provide electoral losers with significant rights to participate in governmental decision making. These institutional designs involve executive power sharing (multiparty coalition government is the norm), balanced bicameralism, federalism and decentralization, electoral rules based on proportional representation, and written constitutions that include minority veto power and opportunities for referendums (Lijphart 1984).

Thus, when it comes to citizen access to and participation in the political process, it is important to note that some countries' institutions are designed to afford greater opportunities for both winners and losers of democratic competition to be represented in the political arena and to implement their preferred policies. Given that consensual systems provide the political minority with a voice in the decision-making process, we expect that the more consensual the set of political institutions in a country, the greater is the extent to which negative consequences of losing elections are muted. Conversely, the more majoritarian the country's institutions, the more winners get to have a say and impose their will on the minority.[2]

Note, however, that this argument does not imply that citizens in one type of system are, on average, more likely to be satisfied with the system than are people in another, regardless of their status as part of the political majority or minority. Instead, we argue that the nature of a country's institutions and status as part of the political majority or minority interact in their effect on satisfaction with democracy. Figure 1 plots this hypothesized interactive effect.

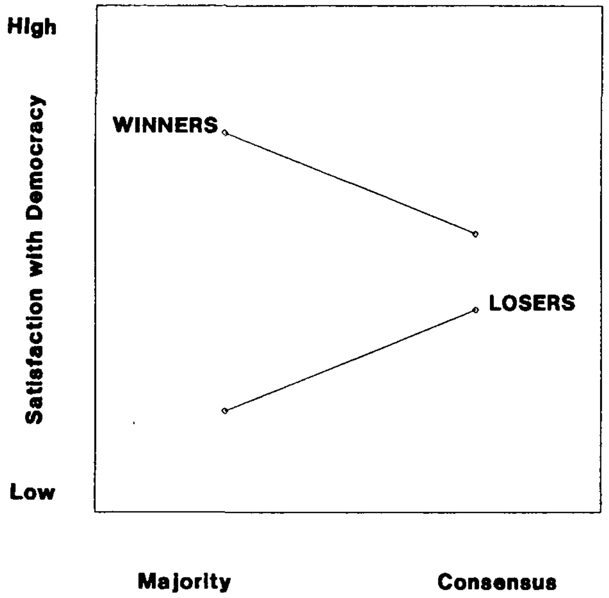
The more consensual the system, the higher should be the level of support for the system among losers and

---

[1] The (reversed) consensus/majority index used in this study is based on Lijphart's (1984) typology and scores countries from most majoritarian to most consensual. Higher values denote that a country is more consensual. The data for the individual countries were updated through 1990 (also see Appendix B for descriptive statistics). The scores for the countries are as follows: Belgium 0.80, Denmark 0.71, France −0.24, Greece −0.70, Ireland −0.62, Italy 1.03, Netherlands 1.08, Portugal 0.22, Spain −0.58, United Kingdom −1.56, and West Germany 0.28.

[2] Note that no predictions are made regarding the quality of the decisions made or of the eventual policy outcomes obtained. For the effects of consensus versus majority democracy on policy outcomes, see Crepaz (1996).

FIGURE 1. Hypothesized Satisfaction with Democracy in Different Systems



the lower it should be among winners. Conversely, as we move toward a more majoritarian system, winners should display higher levels of satisfaction with the system, whereas losers should be much less satisfied with the way the system works.[3] In the aggregate, this should translate into a narrower gap in satisfaction with the system between winners and losers as we move toward a more consensual system. This proposition states that the nature of representation in a country, as conceptualized by Lijphart, systematically mediates the relationship between political majority/minority status and attitudes toward the way democracy works (see Kaase 1995).

Related evidence suggests that consensus versus majority democracy is a useful tool for understanding the congruence of citizen and policymaker interests. Huber and Powell (1994) have shown, for example, that citizen and party preferences are more likely to be congruent in multiparty systems based on proportional representation than in (approximate) two-party systems based on majority electoral laws. As the distinction drawn by Huber and Powell essentially maps onto Lijphart's majoritarian-consensual typology, we expect satisfaction with the system to be greater when there is a greater likelihood of congruence of party and citizen preference. Moreover, there is evidence that two-party (majoritarian) systems, which typically are built on electoral laws that discourage the emergence of additional parties, may alienate voters whose policy positions are not represented in one of the two parties (Miller and Listhaug 1990; see also Weil 1989).

RESEARCH DESIGN

To date, a country's political context rarely has been incorporated explicitly into explanations of system support or satisfaction with democracy and political institutions. In fact, much of the research on the determinants of system support in Western democracies is notably institution-free because it has focused on the study of attitudes using variables measured exclusively at the level of individuals. Such analyses suggest that citizens are supportive of political institutions because of who they are and what they think about different aspects of the political universe. The few exceptions include a small number of studies suggesting that explanations of system support need to take the nature of the political system into account. Miller and Listhaug (1990) find, for example, that people in systems with new parties and opportunities to express discontent with the existing political arrangements are more likely to have confidence in the system. Schmitt (1983), in a study of ten European countries, and Harmel and Robertson (1986), in their nine-nation study, find that people in systems with more durable governments are more supportive of the existing political arrangements.

The focus on individual attributes is largely a consequence of the single-country design employed by these studies. Given that political contexts vary widely among countries, any truly comparative investigation into citizen attitudes toward democracy that seeks to replace country names with variable names requires that variation in institutional structures be taken into account (Przeworski and Teune 1970). While we do not wish to claim that individual characteristics play a secondary role in explanations of support for democratic institutions, we do wish to point out that single-country studies do not easily allow for the construction of theories seeking to explain variations in citizen attitudes using factors measured at the level of the political system.

Because the goal of this study is to explain citizen satisfaction with democracy across a number of democracies, we need surveys that specify such individual-level variables as vote in the last election, demographic information, citizen perceptions of economic conditions, and attitudes toward the political system. Moreover, we require information about countries' political structures since institutional arrangements are not variable in single-country studies, except in cases of significant institutional change over time. For the survey data we use Eurobarometer surveys conducted in the member states of the European Union in 1990 (Reif and Melich 1993).[4] In order to examine the relationships between individual attitudes and systemic properties, these surveys are complemented by information about the nature of democratic politics in a country, specifically, Lijphart's index of consensus and majority democracy.

---

[3] The effects of political institutions on satisfaction with democracy for the country as a whole, however, that is, among both winners and losers combined, is determined by how many more winners there are than losers in any particular country.

[4] The particular set of surveys used is the Eurobarometer 34.0, which was conducted in October/November 1990. It was based on random national samples of about 1,000 respondents each, totaling 10,670 persons aged 15 years and older in the (then) 12 member states of the European Community: Belgium, Denmark, France, Great Britain, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, and West Germany. The inclusion here of West Germany in fall 1990—the period of unification—is not particularly problematic, as West Germans' satisfaction consistently has been high compared to other European countries. We excluded East Germans from the sample because they were unlikely to have developed attitudes

Our research design calls for matching up-to-date measures of political institutions with mass surveys. Given that the Eurobarometer we selected was conducted in fall 1990, it coincides with our indicators of consensus and majoritarian democracy, which measure the constitutional reality in these countries up until 1990. Moreover, since this Eurobarometer is one of the few to include questions about respondents' economic situation, it also allows us to control for an independent variable previously found to be an important determinant of system support.

THE DEPENDENT VARIABLE: SATISFACTION WITH DEMOCRACY

When analyzing support for the political system, many, if not most, studies refer to the work of David Easton (1965, 1975), who distinguishes between diffuse and specific support.[5] Researchers have pointed out that Easton's two categories do not exhaust the possible varieties of political attitudes toward democratic governance (Lambert et al. 1986; Thompson 1970; Weatherford 1984, 1987, 1992; Westle 1989). The object of citizen support does not have to be, and probably cannot be, reliably separated in terms of the system and the system's outputs (Craig 1993, Hibbing and Theiss-Morse 1995). The difficulties of independently measuring diffuse and specific support are enormous, and separate indicators of the two generally are found to be highly correlated (Kaase 1988, Loewenberg 1971). Thus, the distinction between diffuse and specific support mainly has been relevant at the conceptual level, not in the world of empirical social research (Fuchs 1993).

In this paper, we do not seek to resolve the problems arising from operationalizing Easton's distinction (see Weatherford 1992 for an overview). Because it is difficult to classify citizen attitudes toward the political system either as purely diffuse support or as an exclusively evaluative, output-oriented specific support, we believe that those attitudes typically include both an affective and an evaluation dimension. We thus assume that "support reflects a sort of emotionally-biased running tally that citizens keep on the performance of a system" (Kuechler 1991, 280). We also do not seek to understand citizen attitudes toward democratic constitutions as the written rules of the game or as ideal versions of them. Following Weil (1989), who argues that the informal rules of the game in a democracy (and their consequences) are the most important determinants of legitimacy beliefs in Western democracies, we are interested in gauging people's responses to the process of democratic governance. In other words, we are interested in analyzing citizen attitudes toward a country's "constitution in operation" (Lane and Ersson 1991, 194) or its "constitutional reality" (Fuchs, Guidorossi, and Svensson 1995, 328).

Given the state of the debate about the conceptualization and measurement of system support broadly conceived, we rely on a straightforward definition of political support as satisfaction with the way democracy works. The relevant survey measure asked citizens whether they are very satisfied, fairly satisfied, not very satisfied, or not at all satisfied with the way democracy works. Combining the very and fairly satisfied respondents and the not very and not at all satisfied into two categories, Figure 2 shows the distribution of satisfaction with democracy across the countries investigated in this study, ordered according to the Lijphart index. Satisfaction ranged from 83.8% in West Germany to 21.7% in Italy, with a mean of 59.2% and a standard deviation of 17.2%. In eight of the eleven countries, more than 50% of respondents reported that they were very or fairly satisfied with the way democracy works in their country; the exceptions were France, Italy, and Greece.

Satisfaction with democracy measures system support at a low level of generalization. It does not refer to democracy as a set of norms but to the functioning of democracy.[6] Moreover, while this attitude is measured by responses to a question that is directed at the system, it also invites "an evaluative rather than a purely emotional response. Thus, it measures neither diffuse nor specific support in the Eastonian sense, but a form of support not recognized or inadequately conceptualized by Easton" (Kuechler 1991, 279; see also Fuchs and Klingemann 1995, Westle 1989). The phrasing of the question is a good match with the conception of support adopted for this study (see Appendix A).

Clarke and Kornberg (1992, 47, n. 24) and Kornberg and Clarke (1992, 114–6; 1994) report on a variety of tests designed to establish construct validity of the satisfaction with democracy question as an indicator of system support. They find that satisfaction with democracy is clearly an indicator of actual system support and *not* coterminous with support for the incumbent government. Similarly, Fuchs (1993, 242) examines the validity of the satisfaction with democracy indicator and finds that the results constitute "a successful validation of the indicator as a measuring instrument for a generalized attitude towards the political system on the legitimacy dimension." Weil (1989, 692–3) also

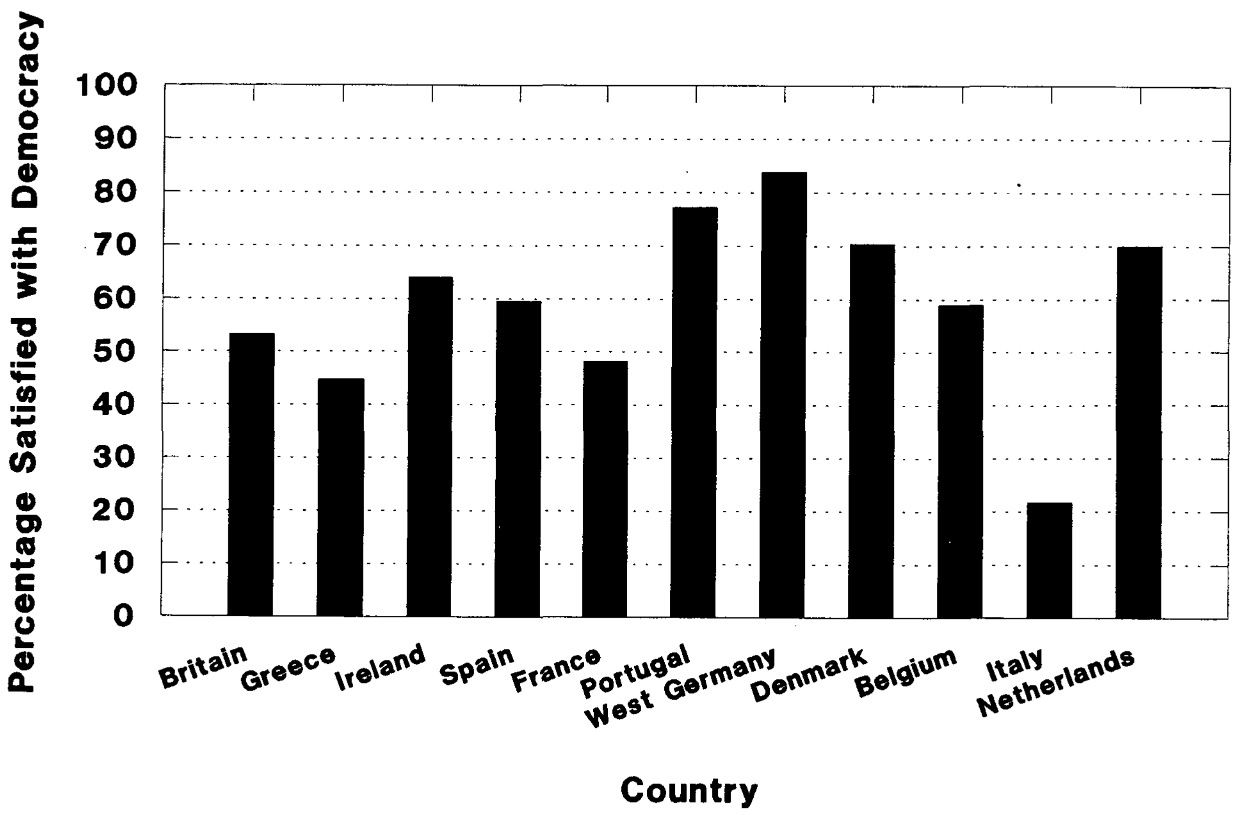
---

toward democracy as conceptualized for the purposes of this study. We also excluded respondents from Northern Ireland and from Luxembourg, the former because of the special political situation there, and the latter because of the small sample size.

[5] Diffuse support is a long-standing predisposition that "refers to evaluations of what an object is or represents—the general meaning it has for a person—not of what it does" (Easton 1965, 273, cited in Kuechler 1991, 278). Specific support derives from a citizen's evaluation of system outputs; it is performance based and may be short term.

[6] Studies that have employed this measure as a dependent variable include Clarke, Dutt, and Kornberg 1993; Fuchs, Guidorossi, and Svensson 1995; Harmel and Robertson 1986; Kornberg and Clarke 1994; Kuechler 1986, 1991; and Lockerbie 1993 (see also Powell 1986, who employs satisfaction with democracy as an independent variable).

FIGURE 2. Satisfaction with Democracy, by Country



Note: Countries are ordered according to the Lijphart index (horizontal axis). Britain is most majoritarian, the Netherlands most consensual. Values on the vertical axis are based on responses to the question: "On the whole, are you very satisfied, fairly satisfied, not very satisfied or not at all satisfied with the way democracy works in (your country)?" The values combine the percentage of respondents saying they were "very" or "fairly" satisfied.

provides some indirect construct validity for the satisfaction with democracy indicator by reporting high positive correlations with political trust.

This dependent variable is useful because of our research design. The Eurobarometer data used here are among the few available sources that permit an analysis of mass political support across a meaningful number of contemporary democracies (Kaase 1988, Weil 1989). Moreover, we can match survey and institutional data very precisely. Although researchers have pointed to problems with the question wording that may not be shared by other measures of attitudes toward the political system,[7] the Eurobarometer data set is the only one that permits a systematic investigation of political institutions and citizen attitudes across a large enough number of countries (Fuchs, Guidorossi, and Svensson 1995; Kaase 1988).

THE INDEPENDENT VARIABLES

The independent variables in this study are *Type of Democracy, Majority/Minority Status, Economic Performance Evaluations, Interest in Politics,* and *Demographic Characteristics*. Each is discussed below.

Type of Democracy

We measured type of democracy with the help of Lijphart's index of consensus and majority democracy, which was calculated on data from (roughly) 1945 to 1990.[8] It thus constitutes an ideal and up-to-date measure of democratic reality in these countries that can be used in conjunction with the 1990 Eurobarometer data. For ease of interpretation, we reversed the original index, which is centered around zero. The more positive the values, the more consensual is the democracy. The Netherlands scores highest at 1.08, whereas Britain is the most majoritarian democracy, with a value of −1.56. The mean value is 0.04, and the standard deviation is 0.85.

Majority/Minority Status

We classified respondents as belonging to the political majority or minority with the help of a survey question asking for which party the individual voted in the last national election (see Appendix A). We then combined these responses with information about the party or parties that controlled the parliaments of the countries at the time the survey was conducted. If the respondent's past vote choice matched the actual party in power, that is, if the person was among the winners of the electoral contest, we scored that individual as zero. Those on the losing side were scored one.

---

[7] See, for example, the political support-alienation scale employed by Muller, Jukam, and Seligson (1982). We believe, however, that the advantages of comparability of an identically worded survey item across a number of systems clearly outweighs the disadvantages of potentially ambiguous meaning.

[8] The values for Greece, Portugal, and Spain reflect constitutional reality over a shorter span, given these countries' authoritarian histories until the mid-1970s.

Economic Performance Evaluations

Any study of satisfaction with the way democracy works would be incomplete without citizen evaluations of system outputs. This observation is directly related to Easton's work, much of which is concerned with outputs generated by the political system. Studies that examine the connection of system outputs with political support typically accept that the two are related because "the government is assumed to possess the tools and abilities to solve social problems" (Weatherford 1984, 189). Thus, performance evaluations shape the reputation of political institutions and of the political system as a whole. Because the policy process involves multiple governmental agents and a lengthy gestation period, citizens have more evidence available about the system as an institutional design for problem solving than about specific political actors (Weatherford 1987).

For the purposes of this study, we focus on economic performance as a system output that affects citizen satisfaction with the way democracy works. Such a focus is valuable because the politics and economics of democratically governed societies have long been intertwined.[9] Substantial evidence suggests that economic performance and public perceptions of both personal and national economic conditions are related to system support (Clarke, Dutt, and Kornberg 1993; Finkel, Muller, and Seligson 1989; Franz 1986; Kornberg and Clarke 1992; Listhaug and Wiberg 1995; Weatherford 1984, 1987). Yet, the relationship between economic performance and system support may not necessarily be clear-cut or direct. Monroe and Erickson (1986) find, for example, that poor economic conditions can affect support for the system, but this effect is mediated by whether citizens hold the government responsible for the economy, and the effect is filtered by perceptions of party differences on economic priorities or managerial skills. Related evidence suggests that both sociotropic and egocentric evaluations of the economy are related to system support (Citrin and Green 1986, Lockerbie 1993). Other performance-based factors not necessarily tied to economic performance per se include citizen satisfaction with policy outputs in general. Several scholars have noted that trust in the government declines with an increase in policy dissatisfaction or when citizens' policy preferences go unrepresented (Craig and Maggiotto 1981; Ho 1991; Miller and Borelli 1991; Miller, Goldenberg, and Erbring 1979; Miller and Listhaug 1990), while others note the predominance of incumbent personalities and political events in the evaluation of system support (Citrin and Green 1986, Howell and Fagan 1988).

Independently of individuals' sociodemographic characteristics and attitudes or of a country's institutional structure, we argue that those who evaluate economic performance negatively also are less satisfied with the way democracy works. Moreover, it is reasonable to expect that political majority/minority status also mediates the relationship between economic evaluations and satisfaction with democracy. Winners are expected to discount negative information about the economy because of their status as supporters of the incumbent government. The effects of negative evaluations on satisfaction with democracy therefore should be attenuated among the winners compared to the losers. Conversely, there should be a ceiling effect for winners such that the incremental increase in satisfaction in the case of positive economic evaluations will be larger for losers than for winners. Overall, the effects of economic evaluations on satisfaction with democracy should thus be smaller for winners than for losers. We measure economic assessments by responses to two Eurobarometer questions that asked respondents to evaluate national and personal economic conditions, that is, to form sociotropic and egocentric evaluations of economic performance (Appendix A).[10]

Interest in Politics

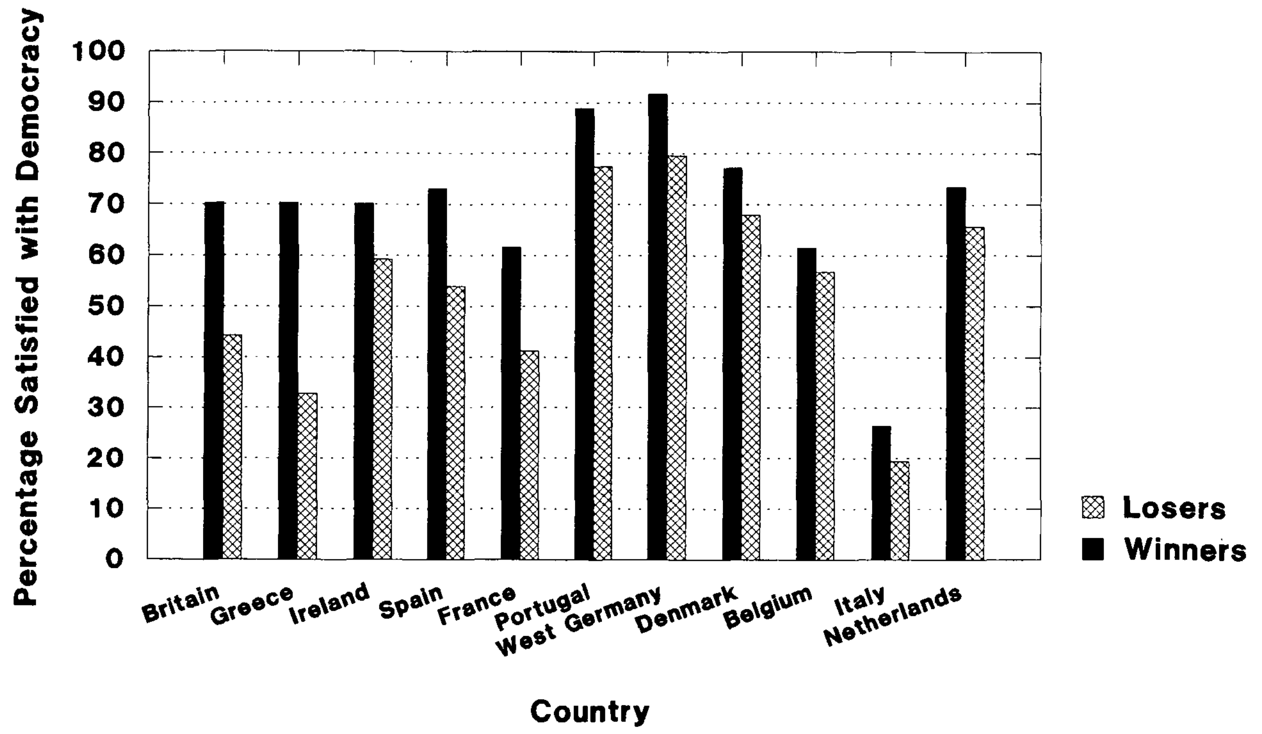
With regard to interest in politics, an extensive literature has demonstrated that it is related to political efficacy and political support (Almond and Verba 1965, Lambert et al. 1986, Weatherford 1991). In turn, citizens who understand the political process and believe that their participation can influence policymaking are likely to take a more optimistic view of democratic governance. Therefore, we hypothesize that political interest and satisfaction with democracy are associated, conceivably in a relationship that can work both ways. We do not account for the possible simultaneity here but hypothesize only that those who are more interested in politics also are more likely to be satisfied with the way democracy works.

---

[9] It is, for example, widely accepted political lore that democratic governments are more likely to fall when economic performance is less than satisfactory (Norpoth 1991). Moreover, an extensive literature has examined the relationship between economic performance and democratic stability (Lipset 1959, Powell 1982). We assume that economic performance is a crucial performance-related output produced by the political system. It is a particularly appropriate indicator of system outputs in Western Europe, where the economy has always been a highly salient political issue (Alt 1979, Anderson 1995). During the late 1980s and early 1990s, in particular, the period under investigation here, high rates of unemployment continued to plague European economies.

[10] These survey questions evoked retrospective evaluations from respondents, which is suitable for our interest, that is, an assessment of past performance rather than future expectations. For a discussion of retrospective versus prospective economic performance evaluations, see Clarke and Stewart 1994; Lewis-Beck 1988; and MacKuen, Erikson, and Stimson 1992.

FIGURE 3. Satisfaction with Democracy among Winners and Losers, by Country



Note: Countries are ordered according to the Lijphart index. Britain is most majoritarian, the Netherlands most consensual. Satisfaction is scored as described in the note to Figure 2. "Winners" and "losers" are based on a comparison of respondents' votes in last general election to governing party.

Demographic Variables

Finally, we also control for the usual sociodemographic variables: education, income, gender, and age (Appendix A). Descriptive statistics for the variables used in this study are shown in Appendix B.

THE EFFECTS OF INDIVIDUAL-LEVEL VARIABLES ON SATISFACTION WITH DEMOCRACY

The Effects of Majority/Minority Status at the Aggregate Level

Are winners more satisfied than losers with the way democracy works? Figure 3 offers some preliminary evidence at the aggregate level. To examine differences in satisfaction between winners and losers at the level of the country, we compared satisfaction with the way democracy works between those who supported the incumbent government in the last election (winners) and those who did not (losers) across the eleven countries, ordered according to the Lijphart index.

Figure 3 shows clearly that there is a gap in satisfaction between winners and losers, regardless of the general level of support for the system. Although there is variation across countries in the strength of the relationship, the gap exists in every country examined.

Multivariate Single-Country Models

We also examined the performance of the individual-level variables in a series of multivariate single-country models. Using least-squares estimations, we regressed the satisfaction scale on the individual-level variables separately for each country.[11] Table 1 shows the results.

Majority/Minority Status. We find that the effects of political minority/majority status remain even when we control for a number of other factors. The effects are powerful and consistently in the expected direction. The relationship holds in all eleven countries, indicating that losers are almost always significantly less satisfied with the way democracy works than are winners. The effects are strongest in Britain, France, and Greece and are weakest in Belgium, Denmark, and the Netherlands.

Economic Performance Evaluations. Economic performance evaluations also play an important role in satisfaction with democracy. The coefficients for both sociotropic and egocentric evaluations of the economy are consistently significant (except for egocentric evaluations in Denmark and the Netherlands) and are in the expected direction. The effect of sociotropic assess-

---

[11] We show only the results of OLS regressions. We also obtained maximum likelihood estimates with Probit, using a dichotomous dependent variable (1 = satisfied; 0 = not satisfied). Note, however, that such estimates do not deviate from the ones presented here.

TABLE 1. Effects of Individual-Level Variables on Satisfaction with Democracy in Eleven Countries

Independent Variable	Belgium	Denmark	France	Great Britain	Greece	Ireland	Italy	Netherlands	Portugal	Spain	West Germany
Majority/minority status (losers high)	−0.093	−0.097	−0.335**	−0.451**	−0.458**	−0.273**	−0.169**	−0.096*	−0.127**	−0.260**	−0.218**
	(0.063)	(0.060)	(0.060)	(0.063)	(0.067)	(0.070)	(0.059)	(0.050)	(0.054)	(0.067)	(0.047)
National economic performance (sociotropic)	0.182**	0.124***	0.209**	0.173**	0.199**	0.106**	0.169**	0.099**	0.229**	0.131**	0.145**
	(0.034)	(0.030)	(0.032)	(0.033)	(0.032)	(0.034)	(0.032)	(0.030)	(0.027)	(0.033)	(0.028)
Personal economic performance (egocentric)	0.140**	0.038	0.100**	0.087**	0.133**	0.074*	0.112**	0.045	0.124**	0.076*	0.111**
	(0.038)	(0.031)	(0.031)	(0.027)	(0.033)	(0.038)	(0.039)	(0.031)	(0.030)	(0.043)	(0.034)
Interest in politics	0.033	−0.022	0.043	−0.032	0.057*	−0.001	0.027	0.042	0.002	0.045	0.067**
	(0.035)	(0.035)	(0.033)	(0.036)	(0.032)	(0.037)	(0.033)	(0.031)	(0.027)	(0.033)	(0.027)
Income	−0.013	0.017*	0.007	0.005	0.007	0.028*	0.001	0.010	0.011	0.012	0.014*
	(0.014)	(0.007)	(0.009)	(0.010)	(0.010)	(0.012)	(0.012)	(0.008)	(0.007)	(0.018)	(0.007)
Education	−0.013	0.010*	0.016*	−0.013	−0.002	0.003	−0.009	−0.003	0.000	−0.005	−0.004
	(0.010)	(0.005)	(0.009)	(0.010)	(0.006)	(0.016)	(0.007)	(0.005)	(0.005)	(0.008)	(0.006)
Gender (female high)	0.066	0.048	−0.001	−0.040	−0.028	−0.172**	−0.063	0.019	−0.099*	−0.002	0.013
	(0.063)	(0.054)	(0.058)	(0.059)	(0.058)	(0.069)	(0.057)	(0.050)	(0.044)	(0.062)	(0.045)
Age	0.001	−0.003*	−0.003	0.000	0.000	0.001	−0.002	−0.001	0.003*	0.005**	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
Intercept	1.682**	2.224**	1.401**	2.497**	1.688**	2.126**	1.499**	2.238**	1.682**	1.969**	2.017**
	(0.288)	(0.229)	(0.239)	(0.276)	(0.236)	(0.317)	(0.235)	(0.214)	(0.187)	(0.265)	(0.200)
Adjusted R^2	0.09	0.04	0.15	0.15	0.25	0.07	0.08	0.03	0.21	0.08	0.13
N	645	863	742	691	751	597	729	830	773	668	817

Note: Unstandardized OLS estimates; standard errors are in parentheses. All significance tests are one-tailed: $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

ments is greater than egocentric ones in each case, demonstrating that perspectives on the national economic situation are more powerful determinants of satisfaction with democracy than are those on personal economic conditions. These results are sensible given that the question used to measure satisfaction with the system presumably prompts individuals to assess the performance of national political institutions. The strongest sociotropic effects were in Portugal, France, Greece, and Belgium, whereas the weakest were in Denmark, Ireland, and the Netherlands. Egocentric evaluations were strongest in Belgium, Portugal, and Greece and had virtually no influence in Denmark and the Netherlands.

Political Interest. Political interest was a statistically significant and positive predictor of satisfaction with the way democracy works in only two of the eleven countries (West Germany and Greece).

Demographic Variables. Demographic factors had no consistent effects in the eleven countries on satisfaction with the way democracy works. Although the coefficients for income, education, gender, and age reached statistical significance in some cases, the substantive influence of these variables was small and not consistently in the same direction. Income displayed significant coefficients in Denmark, West Germany, and Ireland, suggesting that individuals with higher income are more satisfied with the way democracy works in their country. Education was statistically significant in Denmark and France, suggesting that individuals with a higher level of formal education display a higher level of satisfaction. The coefficient for gender is negative and reaches statistical significance only in Ireland and Portugal, denoting that women in these two countries are less satisfied than men with the way democracy works. Age had significant effects in three countries, leading to the conclusion that older respondents in Portugal and Spain displayed a higher level of satisfaction with democracy than did younger respondents, whereas the opposite was true in the Danish case. All three effects were extremely small, however.

Differences across Consensual and Majoritarian Democracies. In what can be termed the traditional consensus democracies (Denmark, Belgium, and the Netherlands), we found that political majority/minority status had weaker effects on satisfaction with democracy than in Britain (the classic case of the Westminster model), in Greece (which scores as the second most majoritarian country on the Lijphart index), and in France (which has a mixed parliamentary/presidential system).

The weakest egocentric economic effects were found in Denmark and the Netherlands, that is, in countries that score high on the consensus scale, whereas egocentric economic evaluations had the strongest influence in Greece, Portugal, and Belgium. Based on this preliminary evidence, we can surmise that personal economic conditions have less effect on satisfaction with democracy in European countries with consensual political institutions. A separate section below more fully explores the effects of the institutional variable.

The Pooled Model

To test whether these relations hold generally, we estimated an identical model with the pooled sample. Because of possible fixed effects across the countries in the study, we estimated the model with several techniques that control for heteroskedasticity in the data, including ordinary least squares (OLS), with and without dummy variables for the countries, and generalized least squares (GLS). Moreover, to guard against the possibility that the results are driven by the use of least-squares techniques on an ordinal dependent variable, we collapsed satisfaction with democracy into two categories (1 = satisfied; 0 = dissatisfied) and reestimated the models with a maximum likelihood technique (Probit) appropriate for dichotomous dependent variables. The results are shown in Table 2.

The results obtained with different estimation methods are very similar. Although the coefficients across maximum likelihood and least-squares estimations are not strictly comparable, with small exceptions the same variables turn out to be significant across the five models, both statistically and substantively. Thus, the results obtained with different statistical techniques led us to conclude that the results are very robust.

The substantive conclusions drawn from the pooled model are similar to those presented in the previous section. Majority/minority status is a powerful determinant of satisfaction with democracy. Moreover, assessments of economic performance are significantly and positively related to satisfaction with democracy, with sociotropic evaluations more influential than egocentric ones. Finally, individuals of high status (measured by income) display a higher level of satisfaction with the way democracy works.

WINNERS AND LOSERS IN CONSENSUAL AND MAJORITARIAN SYSTEMS

So far, the individual-level analyses have provided support for our contention that winners and losers have systematically different attitudes about the way democracy works in their country, even when we control for evaluations of personal and national economic conditions, interest in politics, and a number of demographic factors. To test whether political institutions mediate this relationship between political majority/minority status and satisfaction with democracy, this section examines those variables in relation to the system of governance. We performed an aggregate analysis at the country level and tested multivariate individual-level models of satisfaction with democracy that include measures of type of democracy along with individual-level variables.

Aggregate Analysis

Because losers are less likely to be satisfied with the way democracy works than are winners, and because, as shown in figures 2 and 3, there are significant cross-national differences in the level of system support, we expect that the gap in satisfaction between winners and losers can be explained at least partially by institutional variation. We do not argue that overall levels of satisfaction with democracy depend on institutional differences, but that the difference in satisfaction between winners and losers is driven by the nature of representation in a country. To control for cross-national differences in satisfaction levels, yet at the same time examine differences between winners and losers by type of system, we calculated the differences in satisfaction between winners and losers and plotted the results by type of democracy, as shown in Figure 4.

The graph depicts a strong relationship between type of system (consensual versus majoritarian) and differences between winners and losers regarding their satisfaction with the way democracy works (Pearson's $r = -0.77; p < 0.006$). The gap in satisfaction is larger the more majoritarian the democracy, and it is smaller the more consensual the political institutions.

Multivariate Analysis

To examine whether losers (winners) who live in consensual (majoritarian) systems exhibit a higher level of satisfaction with democracy than do those who live in majoritarian (consensual) democracies, once we control for significant differences at the individual level within countries, we estimated pooled multivariate models that included the consensus/majority index separately for winners and losers, again with the help of least-squares and maximum likelihood techniques. The results for losers are shown in Table 3, those for winners in Table 4.[12]

The results are robust regardless of the type of estimation employed. The same variables turn out to be statistically and substantively significant across OLS, GLS, and Probit estimations. Moreover, their relative ranking (e.g., sociotropic effects being stronger than egocentric effects) is identical in all three models. Thus,

---

[12] There are two ways to deal with qualitative independent variables that represent two populations and that are hypothesized to have an interactive effect with another independent variable on the dependent variable of choice: stratifying the sample into two subsamples or specifying the behavioral differences within the overall model (Hanushek and Jackson 1977, 101). There are drawbacks to both procedures. The use of interaction terms assumes that the other behavioral relationships are the same across observations; that is, that the other independent variables have essentially identical effects in both populations. Stratification reduces the sample size. For the purposes of our analysis, we employed separate regressions instead of interactive terms because we had reason to assume that the other behavioral relationships were dissimilar across observations. The different effects of economic performance evaluations and political interest for winners and losers (tables 3 and 4) indicate that this was an appropriate concern. Moreover, a reduction in sample size did not constitute a problem in our case, given the large sample. Furthermore, since separate regressions are easier to interpret, we opted to present these results. To ensure that this approach was reasonable, we also estimated the interactive relations, the results of which are not shown here. They are fully consistent with our main findings concerning the interaction of institutions and political majority/minority status reported in the text.

TABLE 2. Effects of Individual-Level Variables on Satisfaction with Democracy: Pooled Model Estimates

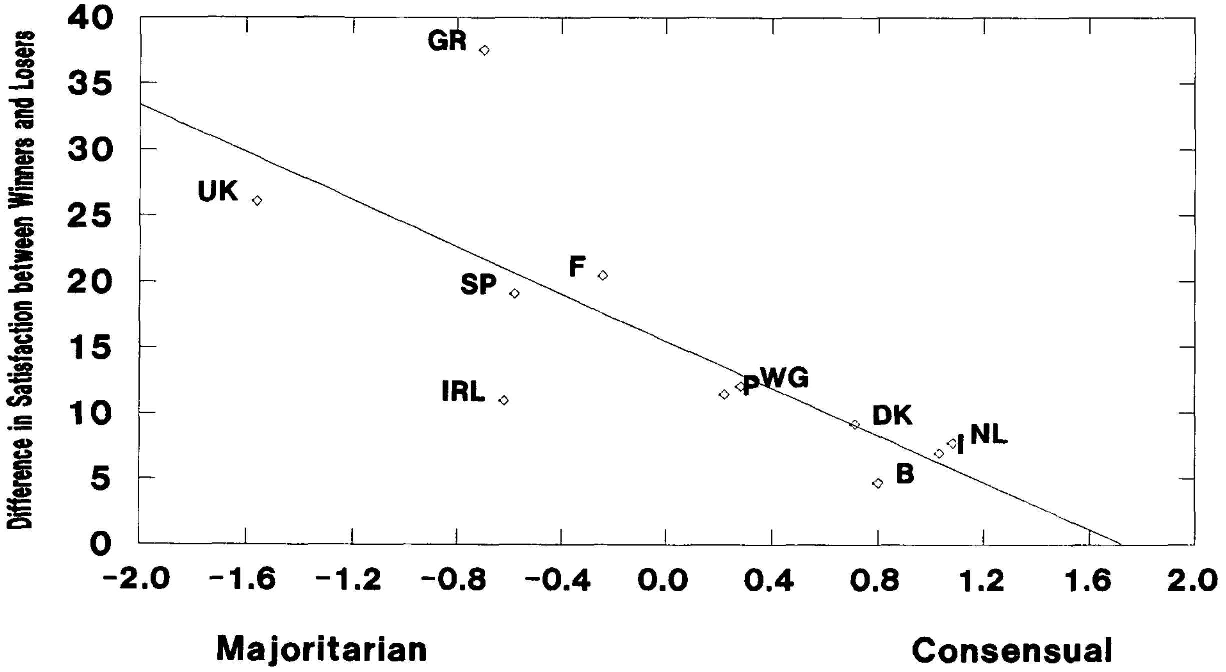
	Model				
Independent Variables	1 (OLS)	2 (OLS incl. country dummies)	3 (GLS)	4 (Probit)	5 (Probit incl. country dummies)
Majority/minority status (losers high)	−0.190**	−0.238**	−0.201**	−0.293**	−0.401**
	(0.018)	(0.018)	(0.019)	(0.031)	(0.033)
National economic performance (sociotropic)	0.210**	0.169**	0.200**	0.325**	0.265**
	(0.009)	(0.009)	(0.010)	(0.016)	(0.017)
Personal economic performance (egocentric)	0.101**	0.102**	0.113**	0.127**	0.136**
	(0.010)	(0.010)	(0.011)	(0.017)	(0.018)
Interest in politics	0.042**	0.020*	0.034**	0.038*	0.018
	(0.010)	(0.010)	(0.010)	(0.017)	(0.018)
Income	0.024**	0.008**	0.017**	0.041**	0.012**
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)
Education	0.000	−0.002	0.001	−0.002	−0.004
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Gender (female high)	−0.005	−0.019	−0.002	−0.025	−0.045
	(0.018)	(0.017)	(0.018)	(0.030)	(0.031)
Age	0.002**	0.000	0.002**	0.002*	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Belgium		−0.463**			−0.741**
		(0.040)			(0.076)
Denmark		−0.086*			−0.244**
		(0.037)			(0.073)
France		−0.513**			−0.870**
		(0.039)			(0.073)
Great Britain		−0.281**			−0.561**
		(0.040)			(0.076)
Greece		−0.470**			−0.861**
		(0.039)			(0.075)
Ireland		−0.400**			−0.684**
		(0.040)			(0.077)
Italy		−0.970**			−1.642**
		(0.040)			(0.079)
Netherlands		−0.267**			−0.518**
		(0.037)			(0.072)
Portugal		−0.201**			−0.178*
		(0.039)			(0.078)
Spain		−0.212**			−0.542**
		(0.040)			(0.077)
Intercept	1.476**	2.222**	1.550***	−1.173**	0.039
	(0.070)	(0.078)	(0.076)	(0.119)	(0.143)
N	8,116	8,116	8,117	8,117	8,117
R^2/pseudo R^2	0.14	0.22	0.14	0.12	0.19
Log likelihood				−4966.94	−4624.62
Correctly predicted			64.4%	67.1%	

Note: Models 2 and 5 include $N - 1$ country dummies; the reference category is West Germany. Unstandardized estimates; standard errors in parentheses. All significance tests are one-tailed: *$p < .05$, **$p < .01$, ***$p < .001$.

we are confident that the results are reliable and not a consequence of the estimation method used.

The results reported in the tables provide largely consistent support for our hypotheses. Since higher values on the Lijphart index indicate that a system is more consensual, the positive coefficient for the consensus/majority index variable suggests that losers who live in consensual systems show a higher level of satisfaction with the way democracy works than do those who live in majoritarian systems. In other words,

FIGURE 4. Differences in Satisfaction of Winners and Losers, by Type of System



Note: B = Belgium; DK = Denmark; F = France; GR = Greece; I = Italy; IRL = Ireland; NL = Netherlands; P = Portugal; SP = Spain; UK = United Kingdom; and WG = West Germany. Values on the vertical axis are based on the question: "On the whole, are you very satisfied, fairly satisfied, not very satisfied or not at all satisfied with the way democracy works in (your country)?" The values show the percentage point differences in satisfaction between winners and losers ("very" and "fairly" satisfied less "not very" and "not at all" satisfied).

the more consensual the system, the more likely are losers to be satisfied with the way democracy works in their country (Table 3).

The coefficients for the consensus/majority index are positive in all three types of estimations and are statistically significant in two of the three (GLS and Probit). Moreover, when we control for the most significant outlier on the dependent variable (Italy) by including a country dummy variable in the OLS estimation, the results for the consensus index become highly significant, both substantively and statistically.[13] Thus, independent of economic performance evaluations or political interest, the type of system in which losers live affects their propensity to be satisfied with the way democracy works. It is noteworthy that the individual-level variables maintain their significant relationship with the dependent variable even when we incorporate information about the type of democracy in which people live.

The results shown in Table 4 indicate that the consensus/majority index has an even larger substantive and statistically highly significant association with satisfaction among the winners of democratic contests. The more consensual the democracy, the *less* satisfied are winners with the workings of the system. Conversely, the closer the democracy to a pure majoritarian type, the more satisfied are winners. Given that the individual-level variables again maintain their significance, we conclude that the type of system is a somewhat more important determinant for satisfaction with democracy among those who belong to the majority than among those who belong to the minority.

Note also that the results presented in tables 3 and 4 support the interaction hypothesis about the effects of political status on the relationship between economic performance evaluations and satisfaction with democracy. The effects of economic assessments on satisfaction with the way democracy works are less powerful for winners than for losers.

CONCLUSION

The study of what citizens think about the political system requires the combination of information about political institutions and about individuals and their attitudes. Based on micro- and macrolevel evidence from eleven European democracies, we found that demographic and attitudinal differences, such as economic performance evaluations and political interest, as well as the type of system in which people live systematically affect citizen satisfaction with democracy. Thus, the results provide ample testimony for the view that people are influenced by the particular political context to which they are exposed.

Overall, our analysis of citizen satisfaction with democracy shows that the level of satisfaction is influ-

---

[13] The unstandardized regression coefficient for the consensus/majority index was 0.125 when we included a dummy variable for Italy in the model (standard error 0.016; $p < .001$), while the coefficients for the other variables did not change appreciably.

TABLE 3. Models of Citizen Satisfaction with Democracy among Losers: Pooled Model Estimates

Independent Variables	Model 1 (OLS)	Model 2 (GLS)	Model 3 (Probit)
Political system type (consensus high)	0.009 (0.015)	0.069** (0.017)	0.043* (0.024)
National economic performance (sociotropic)	0.233** (0.012)	0.220** (0.013)	0.344** (0.020)
Personal economic performance (egocentric)	0.107** (0.013)	0.119** (0.014)	0.135** (0.021)
Interest in politics	0.019 (0.012)	0.011 (0.013)	0.006 (0.021)
Income	0.025** (0.003)	0.020** (0.003)	0.040** (0.005)
Education	0.003 (0.003)	0.002 (0.003)	0.001 (0.004)
Gender (female high)	−0.003 (0.023)	−0.001 (0.023)	−0.037 (0.037)
Age	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Intercept	1.224** (0.086)	1.333*** (0.092)	−1.457** (0.144)
N	5,060	5,061	5,061
R^2/pseudo R^2	0.14	0.13	0.12
Log likelihood			−3177.80
Correctly predicted		62.1%	

Note: Unstandardized estimates; standard errors are in parentheses. All significance tests are one-tailed: $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

enced by whether people belong to the political majority or minority. Those who voted for the incumbent government in the most recent election are significantly more satisfied with the way democracy works than are those who did not. Furthermore, political institutions—measured by Lijphart's index of consensual and majoritarian institutions—mediate the extent to which there is a gap in satisfaction with democracy between winners and losers. The more consensual the democracy, the more likely it is that losers are satisfied with the functioning of democracy and the less likely it is that winners are satisfied. This relationship holds even when we control for powerful economic and other individual-level effects, such as political interest and socioeconomic status. Thus, the research presented here not only is consistent with earlier individual-level work on system support but also adds an institutional dimension hitherto overlooked.

This analysis generates important and generalizable insights into the nature of attitudes about political systems and enhances our understanding of institutions of representative democracy. The results show that those who study attitudes about governance from a comparative perspective are well advised to pay attention to the political context in which people form those attitudes. On a theoretical level, this paper reveals that explanations of citizen satisfaction with democracy are more comprehensive when we incorporate both individuals and institutions into the analysis. In other words, we can better understand differences across individuals and countries regarding satisfaction with democracy if we can identify not only who citizens are and what they think but also the kind of democracy in which they live.

The findings presented here provide additional evidence that institutional variation is an important mediator of public opinion toward political authorities. As recent research on government support has shown, institutional variation is an important element in understanding citizens' ability to assign credit and blame to incumbents for economic performance (Anderson 1995, Powell and Whitten 1993). Regarding the study of democratic institutions, our analysis also documents important and systematic consequences for different kinds of democracies at the level of mass publics. Aside from affecting policy outcomes (Crepaz 1996, Lijphart 1994), cabinet stability and conflict (Powell 1986), or the congruence of elite and mass policy preferences (Huber and Powell 1994), to name just a few, different forms of democratic organization also have consequences for public attitudes toward democracy as a form of government.

TABLE 4. Models of Citizen Satisfaction with Democracy among Winners: Pooled Model Estimates

Independent Variables	Model 1 (OLS)	Model 2 (GLS)	Model 3 (Probit)
Political system type (consensus high)	−0.123** (0.017)	−0.076** (0.018)	−0.158** (0.031)
National economic performance (sociotropic)	0.173** (0.014)	0.161** (0.015)	0.293** (0.026)
Personal economic performance (egocentric)	0.088** (0.017)	0.091** (0.018)	0.109** (0.030)
Interest in politics	0.085** (0.016)	0.084** (0.017)	0.100** (0.028)
Income	0.016** (0.004)	0.012** (0.004)	0.038** (0.008)
Education	0.000 (0.003)	0.000 (0.003)	−0.005 (0.006)
Gender (female high)	−0.008 (0.028)	−0.006 (0.028)	−0.007 (0.050)
Age	0.003** (0.001)	0.003** (0.001)	0.004* (0.002)
Intercept	1.532** (0.109)	1.625*** (0.116)	−1.235** (0.196)
N	3,055	3,056	3,056
R^2/pseudo R^2	0.11	0.10	0.09
Log likelihood			−1762.10
Correctly predicted		70.3%	

Note: Unstandardized estimates; standard errors are in parentheses. All significance tests are one-tailed: $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

Our general findings are likely to hold in a variety of systems outside the European contexts we examined. If it is true that political winners and losers evaluate the performance of the system differently depending on how the system treats them, analogous phenomena should be observable in other democracies. Moreover, the nontrivial effects of economic performance evaluations on satisfaction with democracy indicate that system performance consistently matters in a variety of systems, although there is some debate about how much weight citizens in new and unstable systems give to such assessments (Duch 1995).

Before extrapolating our findings to new democracies, however, several caveats are in order. First, we have not considered the role time plays in the relationship of political institutions and mass publics. Our argument implies that citizens have to experience the functioning of the system before evaluating it on the basis of their status as a political winner or loser. This also implies that systems have to function over some period before people make judgments about them based on system type. Thus, the results presented here may hold only if enough time has passed to generate a sufficient understanding of the nature of the system among the population. This could mean that public opinion about a new democracy immediately following a transition may not be much affected by the kind of democratic system established, but this may matter more after some time has passed. Future studies are needed to establish whether and how the type of democracy affects system support in newly established democracies.

Second, the relationships shown here are subject to change over time. On the basis of these findings we might expect the gap in satisfaction with democracy between winners and losers to increase over time if those in the minority never become the majority. Moreover, one should see more individual-level fluctuation in satisfaction with democracy among winners and losers in a majoritarian system than in a consensual system after a change in government, given the documented differences in how the type of democracy affects citizens' evaluations.

The third caveat applies to those interested in constitutional engineering and reform. The recent change in the electoral system in New Zealand may provide a quasi-experimental setting that could produce important insights into how a change in democratic institutions affects citizen attitudes about democracy. By moving from a first-past-the-post electoral system to a proportional one, such as that used in Germany, New Zealand may find that the way governments form, how policy is made, and who is part of the democratic majority and minority were altered in significant ways after the general election of 1996. Since this change in all likelihood will move New Zealand more closely toward a consensual democracy, it is an important test case of how the type of democracy affects the way people think about the system. Although our results indicate that people think about the political system in consensual and majoritarian terms (see also Jones 1996), they do not suggest there is a magic formula that can be used to produce high levels of citizen satisfaction with the system. The mediating nature of institutions as conceptualized here requires constitutional engineers to determine whether it is more important to reward winners with the power to implement their ideas or to compensate losers with some kind of formal influence on policy decisions. And while it is clear that those who are dissatisfied with system outputs are less satisfied with the system itself, what may matter equally is the kinds of people who want things from the government, given the differences across countries in the inclusiveness and consensuality of the democratic process. Our results show that losers are more likely to be satisfied with the way democracy works—despite their minority status—if there are mechanisms for procedural justice in the democratic process and opportunities for input into the decisions made by the government. Institutional reforms that allow those in the political minority more access to the decision-making process, while ensuring that winning is still meaningful and allows for the implementation of policies preferred by the majority, may go a long way toward increasing citizen satisfaction with democracy and toward ensuring the viability of democratic systems in the long term.

APPENDIX A

Several items on the 1990 Eurobarometer survey provided data for this study.

Satisfaction with Democracy. "On the whole, are you very satisfied, fairly satisfied, not very satisfied or not at all satisfied with the way democracy works in (your country)?" Very satisfied (4), fairly satisfied (3), not very satisfied (2), not at all satisfied (1).

Loser. "Which party did you vote for in the last general election?" If matches with governing party (0), if matches with opposition party (1).

Income. "Here is an income scale. We would like to know in what group your family is, counting all wages, salaries, child allowances, pensions, and any other income that comes in. Just give me the letter of the group your household falls into before tax and other deductions." Scale ranging from 1 to 12.

Education. "How old were you when you stopped full-time education?"

Age. Respondent's actual age.

Sex. Male (1) or Female (2).

Interest in Politics. "To what extent would you say you are interested in politics?" A great deal (4), to some extent (3), not much (2), not at all (1).

National Economic Performance. "Compared to 12 months ago, do you think that the general economic situation in this country is . . .?" A lot better (5), a little better (4), stayed the same (3), a little worse (2), a lot worse (1).

Personal Economic Performance. "Compared to 12 months ago, do you think the financial situation of your household now is . . .?" A lot better (5), a little better (4), stayed the same (3), a little worse (2), a lot worse (1).

APPENDIX B

Descriptive Statistics

Variable	Mean	Std. Deviation	Minimum	Maximum
Consensus/majority index	0.04	0.85	−1.56	1.08
Satisfaction with democracy	0.56	0.84	1.00	4.00
Loser	0.65	0.48	0.00	1.00
National economic performance evaluations	2.78	1.03	1.00	5.00
Personal economic performance evaluations	2.97	0.93	1.00	5.00
Interest in politics	2.35	0.94	1.00	4.00
Income	6.71	3.60	1.00	12.00
Education	17.28	4.55	6.00	74.00
Age	42.43	18.04	15.00	99.00
Sex	1.50	0.50	1.00	2.00

REFERENCES

Abramson, Paul, and Ronald Inglehart. 1995. *Value Change in Global Perspective*. Ann Arbor: University of Michigan Press.

Almond, Gabriel A., and Sidney Verba. 1965. *The Civic Culture*. Boston: Little, Brown.

Alt, James. 1979. *The Politics of Economic Decline*. New York: Cambridge University Press.

Anderson, Christopher J. 1995. *Blaming the Government: Citizens and the Economy in Five European Democracies*. Armonk, NY: M. E. Sharpe.

Barber, David. 1984. *Strong Democracy: Participatory Politics for a New Age*. Berkeley: University of California Press.

Barnes, Samuel, and Max Kaase, eds. 1979. *Political Action: Mass Participation in Five Western Democracies*. Beverly Hills, CA: Sage.

Brittan, Samuel. 1975. "The Economic Contradictions of Democracy." *British Journal of Political Science* 5 (April):129–59.

Citrin, Jack, and Donald Green. 1986. "Presidential Leadership and the Resurgence of Trust in Government." *British Journal of Political Science* 16 (October):431–53.

Clarke, Harold D., Nitish Dutt, and Allan Kornberg. 1993. "The Political Economy of Attitudes toward Polity and Society in Western European Countries." *Journal of Politics* 55 (November): 998–1021.

Clarke, Harold D., and Allan Kornberg. 1992. "Support for the Canadian Federal Progressive Conservative Party since 1988: The Impact of Economic Evaluations and Economic Issues." *Canadian Journal of Political Science* 25 (March):29–54.

Clarke, Harold D., and Marianne C. Stewart. 1994. "Prospections, Retrospections, and Rationality: The 'Bankers' Model of Presidential Approval Reconsidered." *American Journal of Political Science* 38 (November):1104–23.

Craig, Stephen C. 1993. *The Malevolent Leaders: Popular Discontent in America*. Boulder, CO: Westview Press.

Craig, Stephen C., and Michael A. Maggiotto. 1981. "Political Discontent and Political Action." *Journal of Politics* 43 (May):514–22.

Crepaz, Markus M. L. 1996. "Constitutional Structures and Regime Performance in 18 Industrialized Democracies: A Test of Olson's Hypothesis." *European Journal of Political Research* 29 (January): 87–104.

Dalton, Russell J. 1996. *Citizen Politics: Public Opinion and Political Parties in Advanced Industrial Democracies*, 2d ed. Chatham, NJ: Chatham House Publishers.

Duch, Raymond. 1995. "Economic Chaos and the Fragility of Democratic Transition in Former Communist Regimes." *Journal of Politics* 57 (February):121–58.

Easton, David. 1965. *A Systems Analysis of Political Life*. New York: John Wiley.

Easton, David. 1975. "A Re-Assessment of the Concept of Political Support." *British Journal of Political Science* 5 (October):435–7.

Finkel, Steven E., Edward N. Muller, and Mitchell A. Seligson. 1989. "Economic Crisis, Incumbent Performance and Regime Support: A Comparison of Longitudinal Data from West Germany and Costa Rica." *British Journal of Political Science* 19 (July):329–51.

Franz, Gerhard. 1986. "Economic Aspirations, Well-being and Political Support in Recession and Boom Periods: The Case of West Germany." *European Journal of Political Research* 14 (January): 97–112.

Fuchs, Dieter. 1993. "Trends of Political Support." In *Political Culture in Germany*, ed. Dirk Berg-Schlosser and Ralf Rytlewski. New York: St. Martin's.

Fuchs, Dieter, Giovanna Guidorossi, and Palle Svensson. 1995. "Support for the Democratic System." In *Citizens and the State*, ed. Hans-Dieter Klingemann and Dieter Fuchs. New York: Oxford University Press.

Fuchs, Dieter, and Hans-Dieter Klingemann. 1995. "Citizens and the State: A Changing Relationship?" In *Citizens and the State*, ed. Hans-Dieter Klingemann and Dieter Fuchs. New York: Oxford University Press.

Gabriel, Oscar W. 1989. "Regierungswechsel und politische Unterstützung: Implikationen des Parteienwettbewerbs für die Struktur politischer Unterstützung in der Demokratie." *Politische Vierteljahresschrift* 30 (March):75–93.

Hanushek, Eric, and John Jackson. 1977. *Statistical Methods for Social Scientists*. San Diego, CA: Academic Press.

Harmel, Robert, and John Robertson. 1986. "Government Stability and Regime Support: A Cross-National Analysis." *Journal of Politics* 48 (November):1029–40.

Held, David. 1987. *Models of Democracy*. Cambridge, MA: Polity Press.

Hibbing, John, and Elizabeth Theiss-Morse. 1995. *Congress as Public Enemy: Public Attitudes toward American Political Institutions*. New York: Cambridge University Press.

Ho, Szu-yin. 1991. "A Study of Political Trust in the 1980 American Election." *Issues and Studies* 27 (June):102–19.

Howell, Susan E., and Deborah Fagan. 1988. "Race and Trust in Government: Testing the Political Reality Model." *Public Opinion Quarterly* 52 (Fall):343–50.

Huber, John, and G. Bingham Powell. 1994. "Congruence between Citizens and Policymakers in Two Visions of Liberal Democracy." *World Politics* 46 (April):291–326.

Huntington, Samuel. 1974. "Postindustrial Politics: How Benign Will It Be?" *Comparative Politics* 6 (January):163–91.

Inglehart, Ronald. 1990. *Culture Shift in Advanced Industrial Society*. Princeton, NJ: Princeton University Press.

Jennings, M. Kent, and Jan van Deth, eds. 1989. *Continuities in Political Action*. New York: de Gruyter.

Jones, Mark P. 1996. "Assessing the Public's Understanding of Constitutional Reform: Evidence from Argentina." *Political Behavior* 18 (March):25–49.

Kaase, Max. 1988. "Political Alienation and Protest." In *Comparing Pluralist Democracies*, ed. Mattei Dogan. Boulder, CO: Westview Press.

Kaase, Max. 1995. "Demokratie im Spannungsfeld von politischer Kultur und politischer Struktur." In *Jahrbuch für Politik, Vol. 5*, ed. Werner Link, Eberhard Schuett-Wetschky, and Gesine Schwan. Baden-Baden: Nomos.

Kaase, Max, and Kenneth Newton. 1995. *Beliefs in Government*. New York: Oxford University Press.

Kornberg, Allan, and Harold D. Clarke. 1992. *Citizens and Community: Political Support in a Representative Democracy*. New York: Cambridge University Press.

Kornberg, Allan, and Harold D. Clarke. 1994. "Beliefs about Democracy and Satisfaction with Democratic Government: The Canadian Case." *Political Research Quarterly* 47 (September):537–63.

Kuechler, Manfred. 1986. "A Trend Analysis of System Support in West Germany, 1976–1986." Presented at the annual meeting of the Southern Political Science Association, Atlanta.

Kuechler, Manfred. 1991. "The Dynamics of Mass Political Support in Western Europe: Methodological Problems and Preliminary

Findings." In *Eurobarometer: The Dynamics of European Public Opinion, Essays in Honor of Jacques-René Rabier*, ed. Karlheinz Reif and Ronald Inglehart. New York: St. Martin's.

Lambert, Robert D., James E. Curtis, Steven D. Brown, and Barry J. Kay. 1986. "Effects of Identification with Governing Parties on Feelings of Political Efficacy and Trust." *Canadian Journal of Political Science* 19 (December):705–28.

Lane, Jan-Erik, and Sven Ersson. 1991. *Politics and Society in Western Europe*, 2d ed. London: Sage.

Lewis-Beck, Michael. 1988. *Economics and Elections: The Major Western Democracies*. Ann Arbor: University of Michigan Press.

Lijphart, Arend. 1984. *Democracies*. New Haven, CT: Yale University Press.

Lijphart, Arend. 1994. "Democracies: Forms, Performance, and Constitutional Engineering." *European Journal of Political Research* 25 (January):1–17.

Lijphart, Arend, Thomas Bruneau, P. Nikiforos Diamandouros, and Richard Gunther. 1988. "A Mediterranean Model of Democracy? The Southern European Democracies in Comparative Perspective." *West European Politics* 11 (January):7–25.

Lipset, Seymour Martin. 1959. *Political Man: The Social Bases of Politics*. Garden City, NY: Doubleday.

Listhaug, Ola, and Matti Wiberg. 1995. "Confidence in Political and Private Institutions." In *Citizens and the State*, ed. Hans-Dieter Klingemann and Dieter Fuchs. New York: Oxford University Press.

Lockerbie, Brad. 1993. "Economic Dissatisfaction and Political Alienation in Western Europe." *European Journal of Political Research* 23 (April):281–93.

Loewenberg, Gerhard. 1971. "The Influence of Parliamentary Behavior on Regime Stability." *Comparative Politics* 3 (January):177–200.

MacKuen, Michael B., Robert S. Erikson, and James A. Stimson. 1992. "Peasants or Bankers? The American Electorate and the U.S. Economy." *American Political Science Review* 86 (September): 597–611.

Miller, Arthur H., and Stephen A. Borrelli. 1991. "Confidence in Government during the 1980s." *American Politics Quarterly* 19 (April):147–73.

Miller, Arthur H., Edie N. Goldenberg, and Lutz Erbring. 1979. "Type-Set Politics: Impact of Newspapers on Public Confidence." *American Political Science Review* 73 (March):67–84.

Miller, Arthur H. and Ola Listhaug. 1990. "Political Parties and Confidence in Government: A Comparison of Norway, Sweden and the United States." *British Journal of Political Science* 29 (July):357–86.

Mishler, William, and Richard Rose. 1996. "Trajectories of Fear and Hope: Support for Democracy in Post-Communist Europe." *Comparative Political Studies* 28 (January):553–81.

Moe, Terry M., and Michael Caldwell. 1994. "The Institutional Foundations of Democratic Government: A Comparison of Presidential and Parliamentary Systems." *Journal of Institutional and Theoretical Economics* 150 (March):171–95.

Monroe, Kristen, and Lynda Erickson. 1986. "The Economy and Political Support: The Canadian Case." *Journal of Politics* 48 (August):616–47.

Muller, Edward N., Thomas Jukam, and Mitchell Seligson. 1982. "Diffuse Political Support and Antisystem Political Behavior." *American Journal of Political Science* 26 (May):240–64.

Nadeau, Richard, and André Blais. 1993. "Accepting the Election Outcome: The Effect of Participation on Losers' Consent." *British Journal of Political Science* 23 (October):553–63.

Norpoth, Helmut. 1991. *Confidence Regained: Economics, Mrs. Thatcher, and the British Voter*. Ann Arbor: University of Michigan Press.

Powell, G. Bingham. 1982. *Contemporary Democracies: Participation, Stability and Violence*. Cambridge, MA: Harvard University Press.

Powell, G. Bingham. 1986. "Extremist Parties and Political Turmoil: Two Puzzles." *American Journal of Political Science* 30 (May):357–78.

Powell, G. Bingham. 1989. "Constitutional Design and Citizen Electoral Control." *Journal of Theoretical Politics* 1 (April):107–30.

Powell, G. Bingham, and Guy Whitten. 1993. "A Cross-National Analysis of Economic Voting: Taking Account of the Political Context." *American Journal of Political Science* 37 (May):391–414.

Przeworski, Adam, and Henry Teune. 1970. *The Logic of Comparative Social Inquiry*. New York: Wiley-Interscience.

Reif, Karlheinz, and Anna Melich. 1993. Eurobarometer 34.0: Perceptions of the European Community, and Employment Patterns and Child Rearing, October–November 1990. Conducted by INRA (Europe), Brussels. Ann Arbor: Inter-university Consortium for Political and Social Research.

Schmitt, Hermann. 1983. "Party Government in Public Opinion: A European Cross-National Comparison." *European Journal of Political Research* 11 (December):353–76.

Thompson, Dennis. 1970. *The Democratic Citizen*. Cambridge: Cambridge University Press.

Weatherford, M. Stephen. 1984. "Economic 'Stagflation' and Public Support for the Political System." *British Journal of Political Science* 14 (April):187–205.

Weatherford, M. Stephen. 1987. "How Does Government Performance Influence Political Support?" *Political Behavior* 9 (March): 5–28.

Weatherford, M. Stephen. 1991. "Mapping the Ties that Bind: Legitimacy, Representation and Alienation." *Western Political Quarterly* 44 (June):251–76.

Weatherford, M. Stephen. 1992. "Measuring Political Legitimacy." *American Political Science Review* 86 (March):149–66.

Weil, Frederick. 1989. "The Sources and Structure of Legitimation in Western Democracies: A Consolidated Model Tested with Time-Series Data in Six Countries since World War II." *American Sociological Review* 54 (October):682–706.

Westle, Bettina. 1989. *Politische Legitimität: Theorien, Konzepte, empirische Befunde*. Baden-Baden: Nomos.

American Political Science Review Vol. 91, No. 1 March 1997

Institutional Arrangements and the Creation of Social Capital: The Effects of Public School Choice

MARK SCHNEIDER, PAUL TESKE, and MELISSA MARSCHALL
State University of New York at Stony Brook
MICHAEL MINTROM *Michigan State University*
CHRISTINE ROCH *State University of New York at Stony Brook*

While the possible decline in the level of social capital in the United States has received considerable attention by scholars such as Putnam and Fukuyama, less attention has been paid to the local activities of citizens that help define a nation's stock of social capital. Scholars have paid even less attention to how institutional arrangements affect levels of social capital. We argue that giving parents greater choice over the public schools their children attend creates incentives for parents as "citizen/consumers" to engage in activities that build social capital. Our empirical analysis employs a quasi-experimental approach comparing parental behavior in two pairs of demographically similar school districts that vary on the degree of parental choice over the schools their children attend. Our data show that, controlling for many other factors, parents who choose when given the opportunity are higher on all the indicators of social capital analyzed. Fukuyama has argued that it is easier for governments to decrease social capital than to increase it. We argue, however, that the design of government institutions can create incentives for individuals to engage in activities that increase social capital.

The delivery of services by local governments involves a complex relationship between the institutions that supply them and the citizens who use them. To improve the delivery of public services, many reformers argue that governments should imitate private markets by increasing the number of suppliers and by "empowering" citizens to shop across this expanded choice set. In this model, "citizen/consumers" become better *consumers* of public services by becoming more informed about their options and by more carefully selecting services that meet their preferences.

We suggest that the benefits of such market-like reforms can extend beyond the consumer behavior that has been the focus of previous analysis. Specifically, we argue that by expanding the options people have over public services, citizen/consumers can also become better *citizens*, and by so doing, increase the nation's stock of social capital. We test this hypothesis in the context of public school choice—a set of reforms that increases the control parents have over the selection of schools their children attend. These reforms are of long standing in some communities and are emerging in many others. In this research, we show that the design of public institutions charged with delivering education can affect the formation of social capital.

SOCIAL CAPITAL AND LOCAL CITIZENSHIP

An intense scholarly debate recently has emerged concerning the role of social capital in economic and political development (e.g., Brehm and Rahn forthcoming; Fukuyama 1995; Granato, Inglehart and Leblang 1996a, 1996b; Inglehart 1990; Jackman and Miller 1996a, 1996b; Lipset 1995; Putnam 1993, 1995a, 1995b; Swank 1996; Tarrow 1996).[1] One theme in this debate is that social capital may be important to strong democracies for the same reasons that it is important for the functioning of strong economies: High levels of social capital engender norms of cooperation and trust, reduce transaction costs, and mitigate the intensity of conflicts.

While political scientists have only recently adopted the concept of social capital, the term has been used by sociologists for some time (see, e.g., Bourdieu 1980, Loury 1977). Coleman (1988, 1990) brought the term into wider circulation and argued (1988, S101) that social capital is generated as a byproduct of individuals engaging in forms of behavior that require sociability. In his study of 20 subnational governments in Italy, Putnam (1993) argued that the quality of governance is determined by the level of social capital within a region. Fukuyama concurs (1995, 356):

> The ability to cooperate socially is dependent on prior habits, traditions, and norms, which themselves serve to structure the market. Hence it is more likely that a

---

Mark Schneider is Professor of Political Science; Paul Teske is Associate Professor of Political Science; Melissa Marschall is a Ph.D. candidate, Department of Political Science; and Christine Roch is a graduate student, Department of Political Science, State University of New York at Stony Brook, Stony Brook, NY 11794-4392. Michael Mintrom is Assistant Professor, Department of Political Science, Michigan State University, East Lansing, MI 48824-1032.

The research reported in this paper was supported by a grant from the National Science Foundation, Number SBR9408970. Paul Teske thanks the National Academy of Education's Spencer Foundation Post-Doctoral Fellowship for support on this project.

[1] The current debate in political science is focused on somewhat different issues than we address here. However, our research *is* directly relevant to one central theme of that debate–the role of government in creating social capital. In critiquing what he sees as a critical omission by Putnam (1993), Tarrow (1996, 395) asks: "Can we be satisfied interpreting civic capacity as a home-grown product in which the state has no role?" Similarly, Jackman and Miller (1996a, 655) argue that a political institutional approach that endogenizes civic culture can help explain differential political and economic development.

> successful market economy, rather than being the cause of stable democracy, is codetermined by the prior factor of social capital. If the latter is abundant, then both markets and democratic politics will thrive, and the market can in fact play a role as a school of sociability that reinforces democratic institutions.

While comparisons across nations and the identification of trends over time are obviously important, less scholarly work has focused on how government policies affect the stock of social capital. This is especially true for the analysis of the formation of social capital at the local level, where a small but growing body of work has developed addressing the link between government policies and social capital. Stone and his colleagues have been examining the role of "civic capacity," a concept similar to social capital, in local economic development and the politics of education (see, e.g., Stone 1996). Berry, Portney, and Thomson (1993) examined the importance of local community activity in the formation of social capital. And, in the context of education, Astone and McLanahan (1991), Coleman and Schneider (1993), and Lee (1993) have examined social capital as a function of the interactions among administrators, teachers, parents, and children.

We follow the approach of Berry, Portney, and Thomson, who emphasize the importance of communities where neighbors talk to each other about politics. In these face-to-face meetings, these authors argue that "democracy moves politics away from its adversarial norm, where interest groups square off in conflict and lobbyists speak for their constituents. Instead, the bonds of friendship and community are forged as neighbors look for common solutions to their problems" (1993, 3). (Also see Mansbridge 1980 on "unitary democracy" and Barber 1984 on "strong democracy.") Berry, Portney, and Thomson's emphasis on "face-to-face" interactions parallels Fukuyama's (1995) focus on "spontaneous sociability" and Putnam's (1993) emphasis on the role of networks and membership in voluntary and social organizations as supports for representative democracy (see also the review by Diamond 1992).

In this article, we go beyond documenting levels of social capital by identifying the effects of institutional arrangements governing the delivery of education, the most important public good local governments provide, on the formation of social capital. Whereas scholars have recognized the importance of schools in creating social capital for the next generation (see, e.g., Henig 1994, 201–3), for us, schools are also arenas in which social capital can be generated among today's parents.

We explore the relationship between schools and social capital by considering how school choice can influence parental behavior. Specifically, we examine how school choice may increase levels of voluntary parental involvement in the schools, face-to-face discussions between parents, and levels of parental trust in teachers—behaviors that have all been identified as components of social capital. We test these relationships empirically using a quasi-experimental design that allows us to isolate the link between school choice and citizen behavior. Fukuyama has argued that "social capital is like a ratchet that is more easily turned in one direction than another; it can be dissipated by the actions of governments much more readily than those governments can build it up again" (1995, 62). We show that institutional arrangements that increase parental control over the schools their children attend may be able to reverse that ratchet.

Some scholars are skeptical that government policies expanding choice can increase social capital. For example, Anderson argues that expanded citizen choice, at best, will cultivate only a "passive understanding" of the demands of democratic participation and that this "consumer's skill" is not a sufficient basis for "competent citizenship" (1990, 197–8). Carnoy (1993, 187) and Henig (1994, 222) both argue that school choice will increase the social stratification between parents who are more involved and interested in their children's education and those who are not, fundamentally reducing the ability of communities to address collective problems. And Handler (1996, 185) notes that while choice plans require parents to choose, they cannot force parents to become actively engaged in school activities.

In contrast, other scholars argue that choice and related reforms will foster social capital. As Ravitch (1994, 9) notes: "The act of choosing seems to make parents feel more responsible and become more involved." And Berry, Portney, and Thomson (1993, 294) cite the shift to parental control over local schools in Chicago in the late 1980s as a rare example of a successful attempt to get low-income parents more involved in local public affairs (also see Handler 1996).

In the analysis that follows, we show that reforms introducing choice can affect the level of social capital within communities. While our findings are limited to one particular aspect of local communities—schools—they provide important evidence that government or community-initiated policies can indeed ratchet up the preexisting levels of social capital and enhance the social fabric necessary for building and maintaining effective democracy. And, we demonstrate that this can be done both in suburban communities, where most Americans now live, and in inner-city neighborhoods, where the stock of social capital may be most depleted and where its absence may have the most deleterious effects (e.g., Berry, Portney, and Thomson 1993; Wilson 1987).

SCHOOL CHOICE

School choice is perhaps the most widely discussed approach to addressing persistent problems in primary and secondary education in the United States. School choice advocates, liberals and conservatives alike, contend that changing the institutions governing school organization will improve student performance by changing the incentives faced by educators and by changing the behavior of students and parents (see Handler 1996, 9).[2]

---

[2] Classic theoretical treatments include: Chubb and Moe 1990; Coons and Sugarman 1978; Fantini 1973; Friedman 1955, 1962;

It is possible to define school choice in such a way that it is already the norm. Many families already use residential location to choose the public schools their children attend. Even after the residential decision is made, many private alternatives to public education are available and about 10% of parents nationwide choose that option. School choice, however, is typically construed to involve policies that reduce the constraints that traditional public schooling arrangements place on schools and students. (For a discussion of distinctions among choice approaches, see Witte and Rigdon 1993.) Most important, school choice policies are designed to break the one-to-one relationship between residential location and the schools students attend.[3]

Responding to intense policy debates and the growing recognition of the problems of American schools, over the past two decades a growing number of local school districts have changed the institutional frameworks governing the provision of local education giving parents expanded choice over the schools their children attend. We take advantage of this diffusion of the innovation in school choice policy, employing a quasi-experimental approach comparing parental behavior in two pairs of school districts that are demographically similar but vary on institutional arrangements. We analyze the effects of choice on the formation of social capital in a matched pair of inner-city school districts, one with a long history of extensive choice and one without much choice. We then replicate this analysis in two suburban school districts. In each matched pair, the populations are similar demographically, but the institutional arrangements allowing parental choice over the schools their children attend differ.

Our analysis is based on interviews of approximately 300 parents of children in public school grades K–8 across four districts. (Appendix A describes the sample design.) Two of these are inner-city districts in New York City: District 1, which has only recently introduced limited choice, and District 4, which has offered programs of choice for 20 years. The other two are suburban communities in New Jersey: Morristown, which strictly maintains assignment to neighborhood schools, and Montclair, which has had a program of choice since the 1970s.

We begin with a discussion of the two New York school districts, describing in detail the evolution of choice in District 4. We then present an empirical analysis of effects of choice on social capital in the New York setting. Finally, we replicate the analysis using our New Jersey sample.

District 4: A School Choice Innovator

District 4 is located in East or "Spanish" Harlem, one of the poorest communities in New York City. The district serves roughly 12,000 students from pre-kindergarten through the ninth grade. In the early 1970s, the district's performance was ranked the lowest of 32 city public school districts in math and reading scores. Choice was part of a response to this poor performance.

Fliegel (1990) described the evolution of school choice in District 4 as resulting from "creative noncompliance" with New York City rules and regulations. The factors shaping the emergence of the District 4 can be traced back to the late 1960s when the administration of New York City's public school system was decentralized to allow for greater community control. Thirty-two separate community school districts were established, each of which was governed by an elected community school board and by the central Board of Education. High schools remained under the authority of the Board of Education. Decentralization was supposed to promote greater parental participation, but it has also led to problems with corruption, overpoliticization, and poor performance (Cookson 1994, 50–1).

District 4 took full advantage of decentralization, in large part due to the entrepreneurial efforts of Anthony Alvarado, district superintendent from 1972 until 1982. As Boyer (1992, 41–2) notes, Alvarado bent rules, attracted outside grants, and won support from powerful teacher and principal unions. When Alvarado took over as superintendent, District 4 ran 22 schools in 22 buildings. In 1974, the first alternative school, Central Park East Elementary, was developed, followed by an alternative program for seventh and eighth graders with serious emotional and behavioral problems and by the East Harlem Performing Arts School, a program for fourth through ninth graders. These schools were open to parental choice and, as minischools, they were located within existing buildings where space was available. These schools were given greater flexibility over staffing, use of resources, organization of time, and forms of assessment.

The differences between the administration of these alternative schools and the traditional schools led to complaints of favoritism from some teachers and principals in the traditional schools. In response, new opportunities were offered to develop alternate schools using funding from the Magnet Schools Assistance Act (Wells 1993, 56). The district also exceeded its annual budget for many years as these alternative schools were being developed (Henig 1994, 164).

The focus on educational goals was shaped by Seymour Fliegel, appointed District 4's first director of alternative schools in 1976, who developed small schools designed to provide students, parents, and professional staff with flexibility and a sense of school "ownership" (Fliegel 1990, 209). Fliegel also used choice to encourage this sense of ownership. During the late 1970s and the 1980s more than 20 alternative schools were developed, many with distinctive curricular themes. As the number of schools increased, the

---

Jencks 1966. For reviews of school choice in practice, see Cookson 1994; Clune and Witte 1990; and Wells 1993.

[3] These policies include publicly provided vouchers that can be used in a variety of schools, both public and private (see, e.g., Lee 1991), the introduction of magnet schools (see, e.g., Blank 1990), the introduction of charter schools (see, e.g., Wohlstetter, Wenning, and Briggs 1995), and public school choice plans such as those we analyze here.

TABLE 1. District 4 and District 1 Population and Sample Demographics

	District 4		District 1	
	Population	Sample	Population	Sample
Number of students	13,806	333	12,519	295
Number of schools	50	46	24	24
Hispanics	63%	68%	63%	71%
Blacks	33%	26%	12%	11%
Whites	2%	2%	10%	10%
Asian	1%	1%	13%	2%
Percentage in poverty	54%	NA	49%	NA
Income <$20,000 per year	NA	67%	NA	66%
Employed	35%	38%	48%	43%
High school degree or more	48%	65%	63%	65%
Single parent	NA	61%	NA	46%
Female	50%	90%	55%	87%

Source: For district information: *School District Data Book Profiles, 1989–90.*
NA: Since both districts are administrative units for the New York City school system rather than, e.g., census designated units, some demographic data are not available.

differences between schools became more apparent. With many new schools and the potential for parents and students to make meaningful choices, Smith and Meier (1995, 94) suggest that it "became hopeless" to tell parents or teachers that their assignments would be determined bureaucratically. Thus, in 1982, the district decided to provide all parents with choice. Sixteen neighborhood elementary schools remained intact, with space reserved first for those living in the designated zones. While the emphasis was placed on providing choice at the junior high school level, the district also created a considerable number of alternative elementary schools, many of them bilingual (Smith and Meier 1995, 94).

In District 4, all students must make an explicit choice about the junior high school they will attend. Each sixth-grader receives a copy of a booklet describing the alternative junior high schools. Parents and students attend orientation sessions led by the directors of various alternative schools and are encouraged to visit the schools (Wells 1993, 55). Students and their parents rank and discuss their six choices of junior high schools. Sixty percent of the students in the district are accepted into their first-choice school, 30% into their second-choice school, and 5% into their third-choice school. The remaining 5% are placed in schools thought to be most appropriate for them (Boyer 1992, 52–3). To ensure that all students have viable choices, District 4 administrators monitor the popularity of the various alternative schools, closing or restructuring less popular schools (Wells 1993, 55).

District 1: Limited Choice

Our other New York City research site is District 1 on Manhattan's Lower East Side. Largely Hispanic and poor, the residents of District 1 share many characteristics with those of District 4. District 1 was created out of the Two Bridges School District, one of most active districts in New York City's fights over school decentralization in the 1960s. Despite this high initial level of community activism, the schools have foundered over the years. Following the success of District 4, District 1 began experimenting with school choice, and in 1992 created a small number of alternative schools.[4]

As a result of entrepreneurial efforts to develop choice, District 4 has developed a reputation in the city and in the nation as an innovative, successful district. A sense of mission is evident among parents, teachers, and administrators. While there is some dispute about how much of the success can be attributed to choice per se (see Henig 1994, 124–44), there is no question that performance in District 4 improved from its original low level as choice was implemented. In contrast, despite the high level of community activities during the push for decentralization, District 1 has faced considerable administrative turnover and turmoil for the last few years.

We report some comparative data on the districts in Table 1. Both districts are geographically compact, have large numbers of students from very poor families (more than eight of ten students are eligible for free lunches), and have a majority Hispanic student population.

The Survey Respondents

We contracted Polimetrics Laboratory for Political and Social Research, a survey research facility at Ohio State University, to interview 400 residents in each district in spring 1995, sampling parents (or the person in a household who "makes the decisions about the education of children"). To focus on the schools controlled by the districts, the sample frame was limited to parents with children in grades K–8.[5] To randomize, respondents were asked to answer school-specific questions based on the experience of their child in grades K–8 whose birthday came next in the calendar year.

---

[4] In 1993, the New York City Board of Education established a new policy of *interdistrict* choice. If space is available (usually it is not), students can go to schools outside of their district. The Board did not mandate choice programs within districts.

[5] Recall that high schools in New York are run by the central Board of Education.

As Table 1 illustrates, the sample of public school parents in each district is fairly representative of the student population on many key demographic variables. (We chose to interview parents of children who live in the districts but attend private schools as these parents are exercising a form of choice. However, they are not included in the analyses presented below. In District 1, 26% of the respondents sent their child to private school, compared to 17% in District 4.)[6]

Overwhelmingly, we sampled females, both because there are many single mothers in these districts and because we asked to speak with the person in the family who makes the decisions about school. More than 60% of the households were headed by a single parent in District 4, compared to 46% in District 1, and in both districts, more than 85% of the respondents were female.

CONSTRUCTING THE MODELS

With this background in place we now turn to our major goal: to assess the degree to which giving parents more control over the schools their children attend increases their level of social capital. In our analysis we use four measures of social capital, three of which are directly derived from Putnam (1993) and Fukuyama (1995) and the fourth a logical extension.

The first measure is whether the parent is a member of the PTA. Putnam uses declining participation in PTAs as one of his indicators of the erosion of social capital.[7] Second, we analyze a slightly broader measure of parental involvement in the schools, asking parents if in the past year they had engaged in any volunteer activities for their child's school. The third measure we investigate is the number of other parents our respondent talked with about school matters. We use this measure to reflect the "spontaneous sociability" Fukuyama emphasizes as underlying social capital and the importance of "face-to-face democracy" emphasized by Berry, Portney, and Thomson (1993). Our final measure reflects the level of trust parents have in their child's teacher to do the "right thing" for their child.[8] For Fukuyama the general level of trust in society is the critical dimension of social capital, since it lubricates economic, political and social transactions. In this research, we concentrate on a single domain-specific dimension of trust (trust in teachers). These activities not only are central to building social capital, they are also critical to building good schools (see, e.g., Anson et al. 1991).

In our selection of independent variables, we measure elements of motivation, resources, time constraints, and school policies that Kerbow and Bernhardt (1993, 116) argue are critical features of parental involvement in the schools. Thus we employ variables related to individual demographic characteristics as well as those related to the schools children are attending.

Three different types of institutional arrangements exist in the two central city districts in our study. The oldest and most traditional form of school organization is the neighborhood model, in which children are assigned to schools based on residential location. The second is universal choice, which characterizes the intermediate school system (grades 6–8) in District 4. Under this type of arrangement all parents must choose a school for their children (i.e., there is no "default" school). Finally, an "option demand" system of choice (see Elmore 1991), which exists in both districts but is much more developed in District 4, allows parents to select a school other than their neighborhood school. We refer to those parents who have decided to exercise choice as "active choosers." About 20% of our sample fall into the universal choice category (all in District 4), while about 9% of all of the sampled parents in New York are active choosers.

Active choosers present us with the same fundamental problem faced by any research on the behavior of parents in school choice settings—parents choosing alternative schools may not be a random selection of all parents in a school district. And, if parents who self-select alternative schools are also high on social capital then our results will be biased. While other studies have acknowledged this problem and made various efforts to control for selection bias (Chubb and Moe 1990; Coleman and Hoffer 1987; Coleman, Hoffer and Kilgore, 1982; Smith and Meier 1995), we correct for it by constructing a two-stage nonrandom assignment model, in which the first equation models the assignment process and the second equation the "outcome." The method, described in Appendix B and based on the work of Heckman (1978), Heckman, Hotz, and Dabos (1987) and Lord (1967, 1969), corrects for both the nonrandom selection process and other econometric problems associated with the use of dichotomous dependent variables (see Achen (1986) and Alvarez and Brehm (1994) for discussions of the applicability of this method in political science).[9]

---

[6] The table in Appendix A shows that telephone interviewers had greater difficulty completing interviews in District 1 than District 4; however, as evident in Table 1 our samples of public school parents are nonetheless representative of the population of the districts as a whole.

[7] We recognize a limitation inherent in the cross-sectional nature of our research design. Ideally, research on changes in social capital would employ a longitudinal, interrupted time-series analysis, involving panel responses. In this ideal research design, data would be collected prior to institutional changes and, by interviewing the same subjects over time, researchers could isolate the specific effect of institutional changes. Unfortunately, few researchers had the foresight or the resources to conduct such a study; trade-offs must inevitably be made. For example, Putnam (1993) used aggregate-level and (some would say) problematic measures of social capital (see, e.g., Jackman and Miller 1996a) and went beyond his data to explore historical differences in the development of Italian regions. The trade-off in our case is that while we can not gather detailed individual-level data on parents in these districts before they chose a school, we do have detailed individual measures today that our cross-sectional design allows us to test while controlling for individual-level demographic and socioeconomic factors. With replication across four different institutional settings, our quasi-experimental design provides a strong cross-sectional test of the causal relationships postulated in the existing social capital literature.

[8] Participation in the PTA and in voluntary activities is a dichotomous variable, with 1 indicating membership in the PTA (52% report membership) or voluntary activity (66% report such activity). As Verba, Schlozman, and Brady (1995, 74–9) note and our data confirm, levels of voluntary activity in social organizations are considerably higher in America than is participation in electoral activities. The number of parents a respondent reported talking with is a continuous variable based on the midpoints of categories presented (mean = 4.5; s.d. = 4.6). Trust in teachers is operationalized as a dichotomous variable (1 = trusts teachers most of the time or always [77% report this level of trust]; 0 = never or only sometimes).

By limiting the possibility that parents likely to make active choices are also likely to engage in other activities that we refer to as part of social capital, the use of this methodology is critical to our argument that making an active choice influences parental behavior.

As noted in detail in Appendix B, we begin with an explicit assignment equation:

$$Active\ choosers = a + B[Demographics] + B[Values] + B[Diversity] + error, \quad \mathbf{(1)}$$

where *Active Choosers* is a dichotomous variable indicating whether a parent has elected an alternative school or program for their child (1 = yes, 0 = no); *Demographics* is a vector consisting of a set of dummy variables for self-identified racial group membership (black, Hispanic, Asian—white is the excluded category), a continuous variable measuring years of schooling of the parent, a continuous variable reflecting the length of residence in the school district, and a 7-point scale measuring frequency of church attendance (1 = never, 7 = once per week). We also include two dichotomous variables reflecting the gender of the respondent (1 = female) and whether or not the respondent is employed (1 = yes). The racial, gender, and employment variables reflect the resources and demographic factors that may influence activities related to social capital. Parental education level may be particularly important—Putnam (1995b, 667) reports that it "is by far the strongest correlate . . . of civic engagement in all its forms." The length of residence variable reflects the argument advanced by Brehm and Rahn (forthcoming) and by Putnam, who both argue that mobility decreases social capital. In addition, Teske et al. (1993) found that length of residence affected knowledge of school policies. Church attendance is a control variable representing an alternative form of interaction and involvement with the local community.

The *Values* and *Diversity* variables indicate whether a parent thought either particular values or diversity as school attributes were important in their choice of schools. In our survey parents were asked to name up to four attributes they thought were most important in a school. Two attributes in particular, the values espoused by the school and the diversity of the student body, were considered important by parents of children in alternative public schools but not by parents of children in neighborhood public schools.[10] We therefore include these variables in the assignment equation for theoretical reasons, as they are important predictors of active school choosers. We have no theoretical reason, however, to expect these variables to affect social capital and, indeed, they are not empirically related to the activities we have measured. These are used as exclusions in our outcome equation and provide the necessary leverage for estimating the system of equations.[11]

Thus, as described in greater detail in Appendix B, we estimate this assignment equation and the predicted value of the active chooser variable is used in estimating the following outcome equation:

$$Social\ capital = a + B[\text{“}Predicted\text{”}\ active\ choosers] + B[School\ factors] + B[Demographics] + error, \quad \mathbf{(2)}$$

where *Demographics* are as noted in equation 1 and *Values and Diversity* are excluded. *"Predicted" Active Choosers* is the estimated values from equation 1, transformed into a linear functional form following Goldberger (1964; also see Achen 1986, Heckman 1978). *School Factors* measure other aspects of the school environment. These factors include a variable measuring the enrollment in the school the child attends, as smaller schools are often considered to be better arenas for building social capital (Harrington and Cookson 1992); a dummy variable (= 1) when the respondent had made a universal choice at the junior high level in District 4; and a measure of parental dissatisfaction with her child's school.[12] Previous research (e.g., Witte 1991) has demonstrated that parental dissatisfaction is negatively correlated with levels of parental involvement and participation in school activities.

When the dependent variable in the outcome equation is continuous, as in our analysis of the number of parents with whom a respondent has talked about schools, the two-stage estimation technique is fairly straightforward. When the dependent variable is a dichotomous variable, however, another round of corrections is necessary because the disturbances are heteroskedastic (see Appendix B; also see Achen 1986,

---

[9] While it is also plausible that there could be a two-way or reciprocal relationship between social capital and school choice, the timing of our research design makes this unlikely: Parents made their school choice in spring 1994. They were not interviewed until spring 1995, during which time they answered questions about activities during the previous school year. Thus, they chose first and engaged in the activities we measured later.

[10] Smith and Meier find that religion and race help explain why some parents choose private schools for their children (1995, 71–2). Our values and diversity variables for the public schools are closely related to these concepts. Alternative schools in New York tend to emphasize themes and pedagogical approaches that are based on particular social, educational, or civic values. Diversity has a somewhat different meaning in districts where two-thirds of the children are Hispanic.

[11] To estimate two stage models there must be at least one exclusion in the assignment equation. In other words, we must find at least one variable that significantly influences assignment but not the outcome (Achen 1986, 38). We use these two variables, diversity and values, as exclusions.

[12] Our specific measure, indicating whether or not the parent has often thought about moving her child to another school, is a dummy variable coded 1 = yes, the parent has thought about moving her child to a different school. We expect a negative relationship between this measure and our measures of involvement in the schools.

40–7). In our analysis of the other three measures of social capital we report these generalized two-stage least squares (G2SLS) results. Note that since the results are generalized linear probability estimates, the coefficients have a straightforward interpretation: They represent the change in the probability of finding an event given a unit change in the independent variable.

THE EFFECTS OF CHOICE IN THE CENTRAL CITY

With these corrections in place, we are now able to estimate the effects of school choice on the behavior of parents controlling for the nonrandom "assignment" across alternative schools.[13] We present the results in Table 2. Turning first to PTA membership, reported in the first column, we find strong evidence that school choice affects this widely used measure of social capital: Ceteris paribus, participation in the PTA among active choosers is 13% higher than among nonchoosers ($p < .05$), the largest effect in our model, apart from gender.

The effects of some other variables are worth noting. First, note that as the length of residence increases, so does participation in the PTA ($p < .05$, using a one-tail test). Similarly, frequency of church attendance increases participation in the PTA. These findings confirm empirically the arguments presented by Putnam and Fukuyama, as well as findings by education researchers (Kerbow and Bernhardt 1993, Muller and Kerbow 1993). Note too that participation in the PTA increases with the level of parental education—individual human capital and social capital flow together.

In the second column of Table 2, we turn to more general patterns of participation in voluntary events. Here we find that active choosers are over 12% more likely to engage in such activities than are nonchoosers. Paralleling the results reported for PTA membership, church attendance and longer residence are associated with volunteering, as is more years of parental education.

We have shown that active participation in school choice increases levels of involvement with voluntary organizations. We turn next to a measure of "spontaneous sociability"—how many other parents do our respondents engage in discussions about schools? The same cluster of variables emerges as important: Ceteris paribus, active choosers talked with four more parents than nonchoosers (see the third column of Table 2). Again, longer term residents, more educated respondents, and frequent churchgoers talk with more parents than do other respondents.

TABLE 2. The Effects of Choice on the Formation of Social Capital in Two New York Districts

	PTA Member	Voluntary Activities	Parents Talked to	Trust Teacher
Active chooser	.128* (.064)	.123* (.064)	4.053* (2.295)	.095* (.049)
Universal choice	−.035 (.066)	.025 (.062)	−.613 (.651)	.096* (.056)
Dissatisfaction	−.042 (.041)	−.003 (.040)	.234 (.404)	−.239*** (.039)
School size	−.000 (.000)	−.000 (.001)	−.000 (.001)	.000 (.000)
Black	.092 (.072)	.048 (.068)	−.401 (1.30)	−.057 (.044)
Hispanic	−.068 (.066)	−.021 (.062)	.419 (1.22)	−.066 (.036)
Asian	.041 (.187)	.149 (.157)	1.61 (2.47)	.059 (.097)
Length of residence	.005* (.003)	.005* (.003)	.085** (.030)	−.002 (.002)
Education	.015** (.005)	.020** (.006)	.148* (.063)	−.009* (.004)
Employed	−.046 (.044)	.031 (.042)	.038 (.427)	.033 (.029)
Female	.277*** (.056)	.110 (.067)	.370 (.708)	−.052 (.036)
Attend church	.041*** (.009)	.023*** (.009)	.242* (.108)	.010 (.006)
Constant	.336** (.129)	.327** (.135)	.739 (2.34)	1.05 (.090)
	$N = 580$, $F = 66$	$N = 580$, $F = 107$	$N = 568$, $F = 4.4$	$N = 578$, $F = 4.3$

Note: Numbers in parentheses are adjusted standard errors. We do not report R-squared statistics because in the adjustment process necessary to correct for the nonrandom assignment problem, this statistic becomes inappropriate (see Aldrich and Nelson 1994, 14–5).
$^{*}p \leq .05$; $^{**}p \leq .01$; $^{***}p \leq .001$

Finally, we examine trust in teachers. As shown in the final column of Table 2, school factors dominate this model. Active choosers are almost 10% more likely to trust teachers all or most of the time and universal choosers are 9% more likely to do so. In contrast, parents who are dissatisfied with their child's school and have considered moving the child to a different school are 24% less likely to trust their child's teachers. Of the demographic factors, only education is related to trust—but this relationship is negative.

Note also that while choosing significantly increases social capital on all four dimensions we measure, school size is not related to any of these measures. Harrington and Cookson (1992) have argued that the introduction of smaller schools in District 4 was the most important innovation accounting for the improvements found in the district. Our results differ—it is choice and not school size that matters.

TAKING ADVANTAGE OF THE QUASI-EXPERIMENTAL DESIGN: REPLICATING THE NEW YORK FINDINGS

Replication is one of the most powerful tools available for validating social scientific findings. In the next stage of our analysis, we take advantage of our quasi-experimental design to replicate the results of our New York study in another pair of school districts. This replication allows us to explore the robustness of our findings by testing their sensitivity to changes in the context of choice. In our next comparison, we explore the effects of community composition on our findings. In our first analysis, we demonstrated that school choice fosters behavior that builds social capital among parents in low-income central city school districts. Given the multitude of problems facing central cities, this is obviously an important finding. The next question is obvious: Does this relationship hold among suburban parents who now make up a larger share of the American population than do those in the central city?

---

[13] While the two-stage results are the technically correct ones, we should also note that these findings are robust with a simpler methodology. Using a one-stage model, the results are essentially the same.

Second, and more important for us, the institutional factors that define the extent of school choice varies across our two sets of communities. In our next "experiment," we compare patterns of activities in a traditional neighborhood school district (where no one can choose a school except by changing their residential location or by opting out of the public sector altogether) with those in a universal choice district (where there are no neighborhood schools). These institutional arrangements represent more extreme points on the policy continuum than do those in District 1 and District 4. Are the results we found in New York replicated under these different community and institutional conditions? Are the magnitude of the effects similar?

SCHOOL CHOICE AND SOCIAL CAPITAL IN SUBURBAN COMMUNITIES

To answer these questions we turn to our second paired set of communities, Montclair and Morristown, New Jersey, two suburban communities within commuting distance of New York City. Given the institutional arrangements governing the schools in these two districts, we can test the effects of universal choice directly, since everyone in Montclair's public schools chooses and no one in Morristown's can.

Montclair and Morristown, New Jersey

In both communities, court-ordered desegregation decisions in the 1970s led to fundamental changes in the school assignment mechanisms; however, very different responses were developed to achieve racial balance. Montclair adopted school choice, with parents given the right to choose schools from kindergarten through the eighth grade (there is only one high school), with choice constrained by racial balancing. In Morristown, residential zones were created for neighborhood schools. These zones are frequently adjusted so that each school in each zone has the same racial balance, but once set the zones are strictly enforced.

School choice has been operating in Montclair for about as long as in District 4. In 1969, the New Jersey Commissioner of Education ordered Montclair to desegregate or lose state funding. A forced busing plan was implemented in 1972, which caused conflict and considerable white flight. A limited choice program was implemented in 1975 to try to encourage voluntary racial balancing by establishing magnet schools. Several changes were made to the choice plan in Montclair, and in 1984 choice was introduced to the whole district by the symbolic act of turning all schools into magnets.

While choice was initially a solution to racial balancing, parents, teachers, and administrators used it to promote competition and better schools (Boyer 1992, 33). Parents in Montclair are provided with considerable information about the schools. In choosing schools, parents request two options and students are placed in their first choice if it matches the racial balancing goals. The schools are nearly uniformly good and about 95% of parents receive their first choice (Strobert 1991, 56–7). Between 60 and 80% of students are bused to their schools, but now such busing is voluntary.

Table 3 shows the demographics of the public school parents in these two New Jersey districts, overall and for our surveyed sample of 400 parents in each community.

Under the universal system of choice in Montclair,

TABLE 3. Montclair and Morristown Population and Sample Demographics

	Montclair		Morristown	
	Population	Sample	Population	Sample
Number of students	5850	356	5080	286
Number of schools	10	10	9	9
Hispanics	4%	3%	9%	7%
Blacks	36%	34%	17%	16%
Whites	56%	57%	70%	70%
Asian	3%	1%	4%	5%
Percentage in poverty	7%	NA	6%	NA
Income <$20,000 per year	16%	8%	21%	14%
Employed	59%	80%	58%	71%
High school degree or more	88%	98%	86%	94%
Single parent	11%	23%	23%	22%
Female	54%	78%	53%	76%

Source: For district information, *School District Data Book Profiles, 1989–90.*

TABLE 4. The Effects of Choice on the Formation of Social Capital in Two New Jersey Districts

	PTA Member (standard error)	% Change	Voluntary Activity (standard error)	% Change	Parents Talked To (standard error)	% Change	Trust Teacher (standard error)	% Change
Universal choice	0.35** (.11)[a]	13%	0.21* (.13)	6%	1.24** (.38)	13%	0.28* (.14)	6%
Black	−0.55** (.13)	−21%	−0.48** (.14)	−14%	−3.38** (.44)	−30%	−0.41** (.15)	−9%
Hispanic	−1.24** (.29)	−45%	−0.96** (.26)	−34%	−2.86** (.91)	−12%	0.34 (.38)	6%
Asian	−0.57 (.33)	−22%	0.15 (.39)	4%	−3.49** (1.17)	−11%	0.49 (.55)	8%
Length of residence	−0.01 (.01)	−0.07%	0.02** (.01)	0.6%	0.07*** (.03)	9%	0.01 (.01)	0.02%
Education	0.09** (.02)	3%	0.06** (.02)	2%	0.31*** (.08)	16%	0.03 (.03)	0.5%
Employed	−0.07 (.14)	−3%	−0.06 (.16)	−1%	−0.78* (.47)	−6%	−0.27 (.18)	−5%
Female	0.40** (.13)	15%	0.52** (.14)	16%	1.22*** (.44)	10%	−0.02 (.16)	−0.5%
Attend church	0.09** (.03)	4%	0.06* (.03)	2%	0.24*** (.08)	11%	−0.01 (.03)	−0.01%
Dissatisfaction	−1.76** (.42)	−8%	−0.01 (.14)	−0.1%	0.51 (.41)	6%	−0.73** (.14)	−18%
Constant	−0.92 (.41)		−0.45 (.44)		1.71 (1.4)		1.04 (.49)	
	$N = 629$ $\chi^2 = 91$ (.00)		$N = 629$ $\chi^2 = 61$ (.00)		$N = 626$ $F = 14$ (.00)		$N = 622$ $\chi^2 = 43$ (.00)	

Note: In the three probit equations the percentage point change figures indicate the effect of a change from 0 to 1 for the dummy variables and represent the effect of a unit change for the nondummy variables. For the regression equation (parents talked to) the percentage changes are calculated from the normalized beta coefficients.

*$p \le .05$; **$p \le .01$; ***$p \le .001$

all parents are required to choose a school for their child. Therefore, it is not necessary to specify the selection process as we did for the analyses of our New York City parents—that is, no assignment equation is needed and the extensive corrections noted in Appendix B are not necessary. Thus, the results reported in Table 4 are the results of straightforward multivariate analyses. For comparability with the linear probabilities reported in our analysis of New York, we report the percentage point change for a unit change in the independent variable (for the dummy variable, this is the effect of having the characteristic [1] versus not having it [0]). Since all Montclair parents *must* choose their children's school and no one in Morristown public schools can choose (except by moving), the coefficient of the dummy variable for Montclair represents the effects of universal choice, ceteris paribus.

The results in Table 4 show patterns consistent with those in our New York analysis. Choosers are significantly more likely to engage all measures of social capital—PTA membership, volunteering for a school activity, talking to people about schools, and trusting teachers—controlling for other important factors.[14]

SCHOOL CHOICE CAN HELP BUILD SOCIAL CAPITAL

At the heart of calls for the introduction of market-like reforms into the public sector lies the belief that giving people choices over public goods will increase efficiency. Research into the effects of reforming the "supply side" of the provision of public goods has established that such competitive mechanisms can in fact pressure the producers of public goods to be more efficient and more responsive (for local public goods, see, e.g., Ostrom 1972, Schneider 1989, Schneider and Teske 1995, Tiebout 1956). Recently, scholars have begun to study the effects of reforms on the demand-side of the market, leading to debates about the level of information held by citizens and the levels necessary for markets for public goods to work (e.g., Lowery, Lyons, and DeHoog 1995, Lyons, Lowry, and DeHoog 1992, Teske et al. 1993, 1995). This debate has focused on only a limited aspect of the behavior of the "citizen/consumer" in the market for public goods, revolving around the question of whether competition can enhance the behavior of citizens as consumers. We broaden the question by asking if government policies that enhance choice over public goods can increase the capacity of the citizen/consumer to act as a responsible, involved citizen. Our results show that in the domain we study, local public education, the answer is yes.

---

[14] We should also note that, for both urban and suburban districts, parents who chose to send their children to private schools are significantly more likely to engage in all of these social capital building activities than public school parents and more so than even active public choosers, with the exception of PTA involvement. This result is not surprising, and has been documented in the literature on private schools.

According to Putnam, societies can evolve two different equilibria as they solve collective action problems. One equilibrium is built on a "virtuous circle" that nurtures healthy norms of reciprocity, cooperation, and mutual trust. The other relies on coercion and creates an environment in which only kin can be trusted. Civic engagement is at the core of Putnam's concept of social capital because it breeds cooperation and facilitates coordination in governing. Public schools constitute a domain in which the virtuous circle is essential for improving the quality of education. Hillary Rodham Clinton (1996) has argued that "it takes a village" to raise a child. It may also take a "village" to educate a child: High quality education is dependent on parental involvement supported by high levels of community involvement. In turn, higher quality education is associated with activities that build social capital—a virtuous circle is created.

Our research shows that the design of the institutions delivering local public goods can influence levels of social capital. No present statistical method can fully correct for problems in estimation introduced by the complex causal linkages that motivate our study. Our two-stage modeling, however, clearly addresses the biases introduced by the nonrandom "assignment" of parents as active choosers in New York. Our research shows that in both an urban and a suburban setting and under different institutional settings of choice, the act of school choice seems to stimulate parents to become more involved in a wide range of school-related activities that build social capital. Our results support arguments linking participation and urban democracy and, within the domain of schools that we studied, are directly congruent with Berry, Portney, and Thomson's (1993, 254) claim that "increased participation does lead to greater sense of community, increased governmental legitimacy, and enhanced status of governmental institutions."

Clearly, many factors affecting the formation of social capital are individual-level characteristics effectively beyond the control of government (e.g., social capital increases with church attendance and with length of residence in a community). This fundamentally limits the role that government can play in nurturing the formation of social capital. Despite this, we believe that governmental policies can and do affect the level of social capital. The careful design of governmental institutions may be able to reverse the ratchet that Fukuyama believes has only driven social capital down.

APPENDIX A: SURVEY METHODOLOGY

We contracted the Polimetrics Research and Survey Laboratory at Ohio State University to carry out the survey. To start, Polimetrics identified the zip codes in each of the four school districts. All listed telephone numbers for each zip code were identified. From this, a list was developed using random generation of the last two digits of the appropriate telephone exchanges, so that unlisted numbers were included as well. All known business telephone numbers were removed as they were not eligible to be interviewed. Then, a random sample was taken of the remaining numbers.

To be eligible to be interviewed, respondents needed to live within the school district, have children between grades K–8, be the adult responsible for decisions affecting that child's education, and identify the school their child attended (which could be either a private school or a district public school).

The actual interviews were conducted from March through June 1995. The interviewers were given extensive training and some interviews were conducted in Spanish. Interviews were monitored randomly and, to ensure validity, 15% of all completed interviews were verified with respondents by the supervisors.

The goal was to obtain 400 completed interviews in each of the four districts. The following table shows the call dispositions in each district.

TABLE A-1. Disposition of Survey Telephone Calls

	District 4	District 1	Montclair	Morristown
Completed	400	401	408	395
Refusals	113	522	109	174
No final disposition	225	1,642	281	343
Nonhousehold	5,237	17,883	5,268	12,913
Ineligible	5,722	13,469	3,935	5,918

APPENDIX B: CORRECTING FOR NONRANDOM ASSIGNMENT

As Achen (1986) demonstrates, ordinary regression fails to produce unbiased estimates of treatment effects in quasi-experiments when the "assignment" to different conditions is not random (see LaLonde and Maynard 1987; Lord 1967, 1969; Heckman 1978; Heckman, Hotz, and Dabos 1987). Consequently, in addition to specifying the behavioral outcome, we must explicitly model the assignment process. To deal with the dichotomous nature of three of our dependent variables, we apply Achen's generalized two-stage least squares estimator (G2SLS). The steps for this estimation procedure, as well as the standard 2SLS we employ to estimate our continuous outcome equation, are summarized below.

The first stage consists of estimating the assignment equation. This can be done in a straightforward manner by applying the linear probability model. Goldberger's (1964) two-step weighted estimator can be employed to correct for the problems of ordinary least squares (OLS) regression with a dichotomous dependent variable. Before calculating the weights, the predicted values outside the 0-1 interval from the OLS regression should be reset to the bounds. It should also be noted that in order for the system of equations to be estimated, at least one variable in the assignment equation must be excluded from the outcome equation. This variable provides the necessary statistical leverage to estimate the system, so its coefficient in the assignment equation must be nonzero. See Table B-1 for the results of the assignment equation.

For the second stage, the forecast values of the treatment variable (the dependent variable from the assignment equation) are inserted into the outcome equation. When the dependent variable in this equation is continuous (as in the case of our "spontaneous sociability" model) ordinary regression can be applied. The resulting coefficients are 2SLS estimates. The only remaining step in the continuous variable case consists of correcting the standard errors of the coefficients. To accomplish this we first denote the variance of the residuals from our OLS regression ω^2. Next we generate a new forecast value for the dependent variable by using the second-stage coefficients and the original variables. We then compute the variance of the new set of residuals, σ^2, by taking the difference between the two equations. The standard errors of the 2SLS coefficients are corrected by multiplying each standard error by the square root of σ^2/ω^2.

If the dependent variable in the outcome equation is dichotomous, as in our three other models, additional steps are necessary. Once again we insert the forecast values of the treatment variable into the outcome equation. After applying OLS to the outcome equation we compute a new forecast value for the dependent variable using the regression coefficients and the original variables. Once again, predicted values outside the 0-1 interval are reset to the bounds. Next we apply Goldberger's two-step weighted estimator to the outcome equation. The coefficients of the final estimation are the 2GSLS estimates, but again, the reported standard errors are wrong. To correct them we first denote the variance of the residuals from the final stage regression as ω^2. We then multiply each standard error by the square root of $1/\omega^2$. We report these corrected coefficients and standard errors in our tables. Note too that once these corrections are implemented the R^2 statistic is no longer meaningful and is not reported for any of our New York models.

TABLE B-1. Assignment (First-Stage) Equation: Active Public School Choosers in New York

	Coefficient	Standard Error
Diversity	.090*	.038
Values	.115**	.037
Length of residence	.005*	.002
Years of schooling	.006	.004
Black	−.252***	.048
Hispanic	−.216***	.045
Asian	−.318**	.116
Employed	.079**	.028
Female	−.003	.043
Attend church	−.003	.006
Constant	.127	.114

*$p \leq .05$; **$p \leq .01$; ***$p \leq .001$

$N = 584$; $F(10,573) = 10.36$; $p = .000$

REFERENCES

Achen, Chris. 1986. *The Statistical Analysis of Quasi-Experiments*. Chicago: University of Chicago Press.

Aldrich, John, and Forrest Nelson. 1994. *Linear Probability, Logit and Probit Models*. Newbury Park, CA: Sage Publications.

Almond, Gabriel, and Sidney Verba. 1963. *The Civic Culture*. Princeton: Princeton University Press.

Alvarez, R. Michael, and John Brehm. 1994. "Two-Stage Estimation of Non-Recursive Choice Models." California Institute of Technology, Social Science Working Paper 905. Typescript.

Anderson, Charles. 1990. *Pragmatic Liberalism*. Chicago: University of Chicago Press.

Anson, Amy, Thomas Cook, Farah Habib, Michael Grady, Norris Haynes, and James Comer. 1991. "The Comer School Development Program: A Theoretical Analysis." *Urban Education* 26 (April):56–82.

Astone, Nan Marie, and Sara McLanahan. 1991. "Family Structure, Parental Practices, and High School Completion." *American Sociological Review* 56 (June):309–20.

Barber, Benjamin. 1984. *Strong Democracy: Participatory Politics for a New Age*. Berkeley: University of California Press.

Berry, Jeffrey, Kent Portney, and Ken Thomson. 1993. *The Rebirth of Urban Democracy*. Washington, DC: Brookings Institution.

Blank, Rolf K. 1990. "Educational Effects of Magnet High Schools." In *Choice and Control in American Education*. Vol 2: *The Practice of Choice, Decentralization and School Restructuring*, ed. William H. Clune and John F. Witte. London: Falmer Press.

Bourdieu, Pierre. 1980. *Questions de Sociologie*. Paris: Minuit.

Boyer, Ernest. 1992. *School Choice*. Princeton, NJ: Carnegie Foundation.

Brehm, John, and Wendy Rahn. N.d. "Individual Level Evidence for the Causes and Consequences of Social Capital." *American Journal of Political Science*. Forthcoming.

Carnoy, Martin. 1993. "School Improvement: Is Privatization the Answer?" In *Decentralization and School Improvement*, ed. Jane Hannaway and Martin Carnoy. San Francisco: Jossey-Bass.

Chubb, John, and Terry Moe. 1990. *Politics, Markets and America's Schools*. Washington, DC: Brookings Institution.

Clinton, Hillary Rodham. 1996. *It Takes a Village: And Other Lessons Children Teach Us*. New York: Simon and Schuster.

Clune, William H., and John F. Witte, eds. 1990. *Choice and Control in American Education*. Vol. 2: *The Practice of Choice, Decentralization and School Restructuring*. London: Falmer Press.

Coleman, James. 1990. *Foundations of Social Theory*. Cambridge, MA: Harvard University Press.

Coleman, James, and Thomas Hoffer. 1987. *Public and Private High Schools Compared*. New York: Basic Books.

Coleman, James, Thomas Hoffer, and Sally Kilgore. 1982. *High School Achievement: Public, Catholic, and Private Schools Compared*. New York: Basic Books.

Coleman, James. 1988. "Social Capital in the Creation of Human Capital." *American Journal of Sociology* 94 (Supplement):S95–120.

Cookson, Peter W., Jr. 1994. *School Choice: The Struggle for the Soul of American Education*. New Haven, CT: Yale University Press.

Coons, John E., and Stephen D. Sugarman. 1978. *Education by Choice: The Case for Family Control*. Berkeley: University of California Press.

Diamond, Larry. 1992. "Economic Development and Democracy Reconsidered" In *Reexamining Democracy*, ed. Gary Marks and Larry Diamond. New York: Sage.

Elmore, Richard F. 1991. "Public School Choice as a Policy Issue." In *Privatization and Its Alternatives*, ed. William T. Gormley, Jr. Madison: University of Wisconsin Press.

Fantini, Mario D. 1973. *Public Schools of Choice*. New York: Simon and Schuster.

Fliegel, Seymour. 1990. "Creative Non-Compliance." In *Choice and Control in American Education*, Vol. 2: *The Practice of Choice, Decentralization and School Restructuring*, ed. William H. Clune and John F. Witte. New York: Falmer Press.

Friedman, Milton. 1955. "The role of government in education." In *Economics and the Public Interest*, ed. R. A. Solo. New Brunswick, NJ: Rutgers University Press.

Friedman, Milton. 1962. *Capitalism and Freedom*. Chicago: The University of Chicago.

Fukuyama, Francis. 1995. *Trust: Social Virtues and the Creation of Prosperity*. New York: Free Press.

Goldberger, Arnold. 1964. *Econometric Theory*. New York: John Wiley.

Granato, Jim, Ronald Inglehart, and David Leblang. 1996a. "The Effect of Cultural Values on Economic Development: Theory, Hypotheses, and Some Empirical Tests." *American Journal of Political Science* 40 (August):607–31.

Granato, Jim, Ronald Inglehart, and David Leblang. 1996b. "Cultural Values, Stable Democracy, and Economic Development: A Reply." *American Journal of Political Science* 40 (August):680–96.

Handler, Joel. 1996. Down From Bureaucracy: *The Ambiguity of Privatization and Empowerment*. Princeton: Princeton University Press.

Harrington, Diane, and Peter Cookson, Jr. 1992. "School Reform in East Harlem: Alternative Schools versus Schools of Choice." In *Empowering Teachers and Parents*, ed. G. Alfred Hess. Westport, CT: Bergin and Garvey.

Heckman, James. 1978. "Dummy Endogenous Variables in a Simultaneous Equation System." *Econometrica* 46 (July):931–59.

Heckman, James J., V. Joseph Hotz, and Marcelo Dabos. 1987. "Do We Need Experimental Data to Evaluate the Impact of Manpower Training on Earnings?" *Evaluation Review* 11 (August):395–427.

Henig, Jeffrey. 1994. *Rethinking School Choice: Limits of the Market Metaphor*. Princeton: Princeton University Press.

Inglehart, Ronald. 1990. *Culture Shift in Advanced Industrial Society*. Princeton: Princeton University Press.

Jackman, Robert, and Ross Miller. 1996a. "A Renaissance of Political Culture?" *American Journal of Political Science* 40 (August):632–57.

Jackman, Robert, and Ross Miller. 1996b. "The Poverty of Political Culture?" *American Journal of Political Science* 40 (August):697–716.

Jencks, Christopher. 1966. "Is the Public School Obsolete?" *The Public Interest* 2 (Winter):18–27.

Kerbow, David, and Annette Bernhardt. 1993. "Parental Intervention in the School: The Context of Minority Involvement." In *Parents, Their Children and School*, ed. Barbara Schneider and James S. Coleman. Boulder, CO: Westview Press.

LaLonde, Robert, and Rebecca Maynard. 1987. "How Precise are Evaluations of Employment and Training Programs: Evidence from a Field Experiment." *Evaluation Review* 11 (August):428–51.

Lee, Dwight R. 1991. "Vouchers—The Key to Meaningful Reform." In *Privatization and Its Alternatives*, ed. William T. Gormley, Jr. Madison: University of Wisconsin Press.

Lee, Seh-Ahn. 1993. "Family Structure Effects on Student Outcomes." In *Parents, Their Children and Schools*, ed. Barbara Schneider and James S. Coleman. Boulder, CO: Westview Press.

Lipset, Seymour Martin. 1995. "Malaise and Resiliency in America." *Journal of Democracy* (January):2–16.

Lord, Frederic M. 1967. "A Paradox in the Interpretation of Group Comparisons." *Psychological Bulletin 68* (November):304–5.

Lord, Frederic M. 1969. "Statistical Adjustments When Comparing Preexisting Groups." *Psychological Bulletin* 72 (November):336–7.

Loury, Glenn. 1977. "A Dynamic Theory of Racial Income Differences." In *Women, Minorities, and Employment Discrimination*, ed. P.A. Wallace and A. LeMund. Lexington, MA: Lexington Books.

Lyons, William, David Lowery, and Ruth Hoogland DeHoog. 1992. *The Politics of Dissatisfaction*. Armonk, NY: Sharpe.

Lowery, David, William Lyons, and Ruth Hoogland DeHoog. 1995. "The Empirical Evidence for Citizen Information and a Local Market for Public Goods." *American Political Science Review* 89 (September):705–9.

Mansbridge, Jane. 1980. *Beyond Adversary Democracy*. New York: Basic Books.

Muller, Chandra, and David Kerbow. 1993. "Parental Involvement in the Home, School and Community." In *Parents, Their Children and Schools*, ed. Barbara Schneider and James Coleman. Boulder, CO: Westview Press.

Ostrom, Elinor. 1972. "Metropolitan Reform: Propositions Derived from Two Traditions." *Social Science Quarterly* 53 (December): 474–93.

Putnam, Robert. 1993. *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton: Princeton University Press.

Putnam, Robert. 1995a. "Bowling Alone: America's Declining Social Capital." *Journal of Democracy* 6 (January):65–78.

Putnam, Robert. 1995b. "Tuning in, Tuning out: The Strange Disappearance of Social Capital in America." *PS: Political Science and Politics*, 28 (December):664–83.

Ravitch, Diane. 1994. "Somebody's Children: Expanding Educational Opportunities for All America's Children." *Brookings Review* (Fall)4–9.

Schneider, Barbara, and James Coleman, eds. 1993. *Parents, Their Children and Schools*. Boulder, CO: Westview Press.

Schneider, Mark. 1989. *The Competitive City*. Pittsburgh: University of Pittsburgh Press.

Schneider, Mark, and Paul Teske. 1995. *Public Entrepreneurs: Agents for Change in American Government*. Princeton: Princeton University Press.

School District Data Book v. 1.0 [CD-ROM]. 1995. U.S. Department of Education, National Center for Education Statistics and The MESA Group.

Smith, Kevin, and Kenneth Meier. 1995. *The Case against School Choice: Politics, Markets, and Fools*. Armonk, NY: M. E. Sharpe.

Stone, Clarence. 1996. "The Politics of Urban School Reform: Civic Capacity, Social Capital, and Intergroup Context." Presented at the annual meeting of the American Political Science Association, San Francisco, August 29–September 1.

Strobert, Barbara. 1991. *Factors Influencing Parental Choice in Selection of a Magnet School in the Montclair, New Jersey Public Schools*. Ed.D. diss. Columbia University Teachers College.

Swank, Duane. 1996. "Culture, Institutions, and Economic Growth: Theory, Recent Evidence, and the Role of Communitarian Polities." *American Journal of Political Science* 40 (August):660–79.

Tarrow, Sidney. 1996. "Making Social Science Work across Space and Time: A Critical Reflection on Robert Putnam's Making Democracy Work." *American Political Science Review* 90 (June): 389–97.

Teske, Paul, Mark Schneider, Michael Mintrom, and Samuel Best. 1993. "Establishing the Micro Foundations of a Macro Theory: Information, Movers, and the Competitive Local Market for Public Goods." *American Political Science Review* 87 (September): 702–13.

Teske, Paul, Mark Schneider, Michael Mintrom, and Samuel Best. 1995. "The Empirical Evidence for Citizen Information and a Local Market for Public Goods." *American Political Science Review* 89 (September):705–9.

Tiebout, Charles. 1956. "A Pure Theory of Local Expenditure." *Journal of Political Economy* 64 (October):416–24.

Verba, Sidney, Kay Lehman Schlozman, and Henry Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*. Cambridge: Harvard University Press.

Wells, Amy Stuart. 1993. *Time to Choose: America at the Crossroads of School Choice Policy*. New York: Hill and Wang.

Wilson, William Julius. 1987. *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*. Chicago: University of Chicago Press.

Witte, John F. 1991. "The Milwaukee Parental Choice Program." In *School Choice: Examining the Evidence*, ed. Edith Rasell and Richard Rothstein. Washington, DC: Economic Policy Institute.

Witte, John F., and Mark E. Rigdon. 1993. "Education Choice Reforms: Will They Change American Schools?" *Publius: The Journal of Federalism* 23 (Summer):95–114.

Wohlstetter, Priscilla, Richard Wenning, and Kerri L. Briggs. 1995. "Charter Schools in the United States: The Question of Autonomy." Working paper, University of Southern California's Center on Educational Governance. Typescript.

Alliances, Preferential Trading Arrangements, and International Trade

EDWARD D. MANSFIELD *Ohio State University*
RACHEL BRONSON *Columbia University*

We analyze the effects of alliances and preferential trading arrangements on bilateral trade flows. Both factors are likely to promote trade among members, but we argue that the interaction between them is central to explaining patterns of commerce. The combination of an alliance, which creates political incentives for participants to engage in trade, and a commercial institution, which liberalizes trade among members, is expected to provide a considerable impetus to commerce among parties to both. The results of our quantitative analyses support these arguments. Both alliances and preferential trading arrangements strongly affected trade from 1960 to 1990, and allies that included a major power conducted considerably more trade than their nonmajor-power counterparts. Moreover, the interaction between alliances and preferential trading arrangements is fundamental to explaining patterns of bilateral commerce: Parties to a common preferential trading arrangement and a common alliance engage in markedly greater trade than do members of either type of institution but not both.

Concerns about the stability of the international trading system have become widespread in recent years. Many observers feel that the relatively liberal system forged after World War II is being undermined by the proliferation of preferential trading arrangements. These concerns have spawned a great deal of empirical research, much of which has found that such arrangements exert a substantial effect on trade flows. Yet, these studies have neglected the role political-military alliances play in shaping patterns of commerce. This omission is important because there is ample reason to expect that alliances guide trade flows, and preferential trading arrangements often are comprised of allies. As a result, extant analyses risk confusing the effects of alliances and preferential trading arrangements on commerce and systematically overstating the effects of the latter.

In this study, we analyze the effects of alliances and preferential trading arrangements on bilateral trade flows. Both factors are likely to promote trade among members, but we argue that the interaction between them is central to explaining patterns of commerce. The combination of an alliance, which creates political incentives for participants to engage in trade, and a commercial institution, which liberalizes trade among members, is expected to provide a considerable impetus to commerce among parties to both.[1]

Our quantitative analysis supports these arguments. We find that both alliances and preferential trading arrangements strongly affected trade from 1960 to 1990, and that allies that included a major power conducted considerably more trade than their nonmajor-power counterparts. Moreover, the interaction between alliances and preferential trading arrangements is fundamental to explaining patterns of bilateral commerce: Parties to a common preferential trading arrangement and a common alliance engage in markedly greater trade than do members of either type of institution but not both.

ALLIANCES AND TRADE

The effects of alliances on commerce emanate from the incentives of both governments and firms. Governments have an incentive to liberalize trade with allies because the efficiency gains from trade yield increases in national income that can be used to augment states' military power. As a result, trade generates security externalities (Gowa 1994).[2] Since the anarchic nature of the international system places pressure on states to attend to the power of others (Waltz 1979), they cannot ignore these security externalities without bearing substantial risks.

Open trade is much more likely to evolve among allies than among adversaries. Trade among allies is likely to enhance the security of all the parties; the gains from trade accrue to states with common security goals and bolster the aggregate political-military power of the alliance. In contrast, open trade among adversaries produces negative security externalities. A state that trades with an adversary augments the national income of its trading partner, thereby threatening to undermine its own security.[3] Especially among states

---

Edward D. Mansfield is Associate Professor of Political Science, Ohio State University, Columbus, OH 43210-1373. Rachel Bronson is a Ph.D. candidate in the Department of Political Science, Columbia University, New York, NY 10027.

Earlier versions of this paper were presented at the annual meeting of the American Political Science Association, September 1–4, 1994, New York, NY; and at seminars at several universities: Chicago (PIPES), Columbia, Cornell, Harvard, Ohio State, Pennsylvania, and Princeton. We are grateful to seminar participants and Jagdish Bhagwati, Xinyuan Dai, Jeffrey Frankel, Joanne Gowa, John Hartman, Arvid Lukauskas, Walter Mattli, Timothy McKeown, Helen Milner, Kenneth Oye, and Robert Shapiro for helpful comments; to Lynn Jacobsen, Salvatore Pittruzello, and Susan Zayak for computer programming assistance; and to Gina Marie Finan and Joe McGarvey for research assistance.

[1] By virtue of our focus on alliances and preferential trading arrangements, our analysis is cast at the systemic level. Although various studies emphasize the domestic determinants of trade (e.g., Cohen 1990, Frieden 1988, Rogowski 1989), it is beyond the scope of this paper to examine these factors.

[2] Security externalities are uncompensated benefits or costs that influence the security of one actor due to the actions of another actor. For a more extensive discussion of this issue, see Gowa 1994.

[3] A state may have an incentive to trade with an adversary if, for example, it anticipates realizing very large gains from trade or

with sufficient market power to influence their terms of trade, governments are expected to discriminate in their foreign economic policies between allies and adversaries (Gowa 1994).

Private traders and investors have incentives to behave in a manner consistent with these government policies. All other things being equal, trade barriers imposed on a product increase its local price, thereby reducing local demand for it by private traders and other consumers. Since governments are more likely to impose trade barriers on the goods of adversaries than on those of allies, local demand for adversaries' goods is likely to be less than that for allies' goods.

In addition, many investments by firms are relation specific, that is, they are made to support a particular transaction with a particular partner (Williamson 1985, 54; Yarbrough and Yarbrough 1992, 25). As Yarbrough and Yarbrough (1992, 28) have pointed out, investments in relation-specific assets "are common in international trade, because the process of specialization according to comparative advantage involves investment in increased productive capacity designed to service export markets." Many observers also argue that such investments have become increasingly pervasive.[4]

Among relation-specific investments related to international trade are those made in facilities designed to transport goods to a specific market, those made in dedicated assets to produce goods for a specific market, and those made to create vertically integrated production facilities across states. For example, a Japanese automobile manufacturer may invest in a plant or technology to meet the specific demands of U.S. consumers. Similarly, a firm located in one country that invests in raw materials may vertically integrate its production facilities by investing in a plant to process these materials that is located in a foreign country where the processing can be conducted most efficiently. In both cases, firms have undertaken investments to support a specific relationship. The costs of shifting the assets in which these firms have invested to their best alternative uses therefore are likely to be high, and their value depends in part on the continuity of the particular relationship for which they are designed (Williamson 1985, 55).

Particularly important for our purposes is the disproportionate susceptibility of relation-specific investments—and the trade flows they generate—to opportunistic behavior by foreign governments.[5] Investors face the risk that a foreign government will take actions, such as imposing new or raising existing trade barriers, which degrade the value of these investments.

Alliances can help regulate opportunism by foreign governments. Because open trade among allies promotes the security of members, governments have less incentive to behave opportunistically toward their allies' firms than toward firms of other states. Thus, private agents reduce the likelihood of opportunism—and enhance the expected profitability of investments—by investing in relation-specific assets to service allies' markets. Similarly, governments have incentives to engage in opportunistic behavior toward their adversaries' firms, since doing so helps to redress the security externalities arising from trade. Firms face heightened risks, including the possibility that a foreign government will raise its trade barriers or even expropriate dedicated assets located within its borders, if they make investments to service adversaries' markets.[6] Furthermore, as the experience of the Coordinating Committee for Multilateral Export Controls (COCOM) illustrates, firms' ability to conduct business abroad can be limited by government actions if they prefer to trade with an adversary (Mastanduno 1992). For these reasons, firms and private traders are expected to conduct more trade with agents located in states that are allies of their home government than with those located in states that are adversaries.

PREFERENTIAL TRADING ARRANGEMENTS, MAJOR POWERS, AND TRADE

Two factors are likely to influence the magnitude of the relationship between alliances and trade. The first concerns whether allies also are members of a common preferential trading arrangement; the second concerns whether at least one of them is a major power.

That preferential trading arrangements are likely to facilitate trade among members is hardly controversial. Such arrangements refer to agreements stipulating that states impose lower levels of protection on members' goods than on the goods of third parties. These agreements may take the form of free trade areas, customs unions, common markets, and economic unions. While differences exist among these commercial arrangements, common to all of them is "the reciprocal nature of the preferential treatment which the participants accord to one another" (Anderson and Blackhurst 1993, 5).

Yarbrough and Yarbrough (1992) argue that preferential trading arrangements provide an institutional means of governing opportunism. Like alliances, preferential trading arrangements are likely to promote relation-specific investments by private agents, which in

---

promoting the competitiveness of domestic industries that are central to its security. Unless both this state and its adversary expect trade to have no influence on the distribution of power between them, however, the party that expects to gain less from trade (and therefore to experience a deterioration in its relative power) will have an incentive to limit commerce.

[4] For example, it has been argued that intrafirm trade is a good measure of trade involving relation-specific investments, and there is considerable evidence that intrafirm trade constitutes a substantial portion of international commerce and has been growing (McKeown 1991, 165–8; Yarbrough and Yarbrough 1992, 32).

[5] Of course, foreign firms also may engage in opportunistic behavior, an issue taken up by Williamson (1985) and Yarbrough and Yarbrough (1992, 34).

[6] This argument is consistent with the view that private traders are likely to "take account not only of price and quality of goods and services but of the place of origin of these products and the political relationship between them" (Pollins 1989a, 738). In addition, Pollins (1989a, 1989b) argues that private agents may prefer to reward actors in states that they perceive to be politically friendly and punish actors located in hostile states. This also suggests that trade will be greater between allies than adversaries.

turn spur commerce among participants. More generally, it seems clear that, all other things being equal, preferential treatment is likely to facilitate the flow of trade among members. Less clear, however, is the extent to which preferential arrangements foster trade and the relative importance of alliances and preferential trading arrangements in this regard.

In addition to the separate effects of alliances and preferential trading arrangements on commerce, there is reason to expect that trade will be especially open between states that are both allies and members of a preferential trading arrangement. To the extent that the latter facilitates the use of strategies of contingent cooperation, and since this type of arrangement often is explicitly designed to reduce trade barriers, the combination of an alliance and a preferential trading arrangement provides multiple and reinforcing incentives for states to liberalize commerce and for firms to make relation-specific investments to serve foreign markets, thereby generating greater trade among members than does either type of institution alone.

Besides preferential trading arrangements, the presence of a major power also is likely to influence the relationship between alliances and trade. As noted earlier, the effects of alliances on trade are expected to be heightened among states with the ability to impose an optimal tariff. States that can degrade the national income of an adversary have little incentive to liberalize trade with it, since doing so only serves to undermine their security. In contrast, such states do have an incentive to liberalize trade with an ally, since an optimal tariff redistributes the gains from trade to them but eliminates these gains altogether for their trading partner, thereby undermining the alliance's strength. Major powers are likely to possess some monopoly power in trade (Gowa 1994, Gowa and Mansfield 1993); hence, the effects on trade flows of alliances involving them are likely to be especially pronounced.

Alliances including major powers also are likely to enhance openness among members because these alliances tend to be more durable and stable than those comprised solely of weaker states. By virtue of major powers' disproportionate strength, alliances involving them often exhibit an especially skewed distribution of power, a characteristic that has been linked to heightened alliance durability and stability (e.g., Morrow 1990).

Consistent with this argument, Gaubatz (1996) found that alliances involving major powers were significantly more durable than other alliances from 1816 to 1965. The results of a preliminary test indicate that major-power alliances also have been more durable than other alliances during the period after World War II, the era on which we focus in this paper. The analyses of alliances and major powers conducted here are based on Small and Singer's (1969, 1982, 44–5) data and updates of them compiled by the Correlates of War (COW) Project. To assess the durability of major-power alliances after World War II, we code each alliance identified by Small and Singer that was formed during this era based on whether it includes at least one major power. We then compare the average duration of alliances that include a major power with the average duration of those that do not.[7] Our results indicate that major-power alliances tend to last about twice as long as other alliances and that this difference is statistically significant ($t = 4.33$, $p < .005$).

While all alliances are expected to promote trade among members, the disproportionate durability of major-power alliances is likely to bolster their effect on trade because members are likely to discount less heavily the future benefits arising from intraalliance trade than do members of other alliances. The relative longevity of major-power alliances and the dearth of such alliances among which members typically can relocate reduce the risk of exit by members. Consequently, these factors limit the likelihood that today's ally will become tomorrow's foe and turn its augmented power, derived from prior intraalliance trade, against its former confederates. These factors also limit the risks faced by investors that the governments of current allies will shift political alignments and threaten their relation-specific assets by engaging in opportunism in the near future. Alliances involving major powers therefore should be advantaged in their ability to promote trade among members.[8]

In sum, we expect that international trade is likely to be greater when trading partners are allies or members of a preferential trading arrangement than when neither of these conditions is met. We expect that commerce will be greater still when both of these conditions obtain and when trading partners are allies and include a major power. We expect that trade will be greatest among states that are allies, members of a preferential trading arrangement, and include a major power.

PREVIOUS EMPIRICAL STUDIES

The existing empirical research bearing on these topics centers primarily on the effects of preferential trading arrangements on bilateral trade flows. Studies of this sort usually conclude that preferential trading arrangements increase the flow of trade among members, although the strength of these effects varies among commercial arrangements (Aitken 1973, Brada and Mendez 1983, Eichengreen and Irwin 1995, Frankel 1993, Linnemann 1966, Pelzman 1977, Pollins 1989a, Tinbergen 1962, Winters and Wang 1994). Yet, these studies rarely emphasize other "noneconomic" determinants of trade, which leads them to ignore the effects of alliances and other political factors. Since preferential trading arrangements often are comprised of allies (Mansfield 1993), these analyses risk confusing the effects of alliances and preferential trading arrangements. In so doing, they are likely to yield biased estimates of preferential trading arrangements' effects on trade flows and, hence, to offer a distorted view of the political economy of international trade.

---

[7] Because many alliances are still in effect, it is not possible to measure their duration. For the purpose of this rough test, alliances still in effect as of 1993 (the last year for which data are available) are coded as ending in this year. Like Morrow's (1990, 921) analysis of alliance durability, "each alliance is treated as a whole and is not decomposed into dyads of allies."

[8] In addition, like the United States after World War II, major powers often have the economic capacity to provide aid and investment that may promote trade with weaker states (Freeland 1985).

Many empirical studies of the international political influences on trade focus on diplomatic cooperation and conflict between states. Polachek (1980, Gasiorowski and Polachek 1982), for example, argues that utility-maximizing actors tend to engage in cooperative relations with trading partners because of the opportunity costs associated with forgoing trade in the event of conflict. In the only studies bearing directly on this topic, Pollins (1989a, 1989b) examined the effects of preferential trading arrangements and diplomatic cooperation and conflict on bilateral trade flows. He found that cooperation promotes trade, but he also observed that his model may not account for variations in trade patterns between allies and adversaries or between major powers and other states (Pollins 1989a, 757). Indeed, since there is evidence that allies frequently engage in political conflicts (e.g., Bueno de Mesquita 1981), it is important to isolate the effects of alliances on trade rather than infer them from studies focusing on the effects of political cooperation and conflict.

A few studies have examined the effects of alliances on bilateral trade flows (Gowa 1994, Gowa and Mansfield 1993, Mansfield and Bronson 1997, Summary 1989), but they focused on trade involving major powers. As we argued above, it is important to determine the extent to which the strength and magnitude of the relationship between alliances and trade depend on whether the commercial partners are major powers. Furthermore, these studies did not examine the interactions among alliances, preferential trading arrangements, and major powers, an issue that is central to our analysis.

THE MODEL

In order to test the hypotheses described earlier, we begin with a gravity model, which has become a standard framework for explaining patterns of bilateral trade (e.g., Aitken 1973; Anderson 1979; Deardorff 1984, 503–4; Frankel 1993; Gowa 1994; Gowa and Mansfield 1993; Leamer and Stern 1970, 145–70; Linnemann 1966; Pelzman 1977; Pollins 1989a; Tinbergen 1962; Winters and Wang 1994). The gravity model describes trade flows between a given pair of states in terms of factors that influence the supply of exports in one country, factors that affect the demand for imports in the other, and factors that impede or facilitate the flow of trade between them. This model usually takes on the following form:

$$X_{ij} = \beta_0 GDP_i^{\beta 1} GDP_j^{\beta 2} POP_i^{\beta 3} POP_j^{\beta 4} DIST_{ij}^{\beta 5} A_{ij}^{\beta 6} e_{ij}, \tag{1}$$

where X_{ij} is the flow of trade between countries i and j; GDP_i and GDP_j are the gross domestic product (GDP) of i and j; POP_i and POP_j are the population (or, equivalently, the per capita GDP) of i and j; $DIST_{ij}$ is the geographical distance between i and j; A_{ij} is a set of additional factors influencing trade between i and j, often including preferential trading arrangements; and e_{ij} is a lognormally distributed error term.

Different variants of this model have been advanced, and various theoretical justifications for it have been offered (Anderson 1979; Bergstrand 1985, 1989; Deardorff 1995; Helpman and Krugman 1985, chapter 8; Leamer and Stern 1970; Linnemann 1966; Tinbergen 1962).[9] But it is widely recognized that a state's capacity to supply exports is directly related to its GDP; so, too, is a state's demand for imports. Population often is included in this model as a proxy for a country's market size. Assuming economies of scale in production, the larger a country's population, the more goods for which it is likely to achieve the minimum scale needed for efficient production without relying on export markets (Aitken 1973, 882; Linnemann 1966, 11–4; Winters and Wang 1994, 12). A more populous country, therefore, is expected to produce less for export relative to its total production than is a less populous country. Similarly, a more populous country is expected to have a greater capacity to satisfy domestic demand with domestically produced goods, and thus a lower demand for imports, than is a less populous country.

Distance usually is included in the gravity model as a proxy for transportation and transaction costs. These costs are expected to rise, and hence bilateral trade flows are expected to decrease, as the distance between trade partners increases. For these and other reasons, the gravity model predicts that bilateral trade flows will be directly related to the GDP of i and j and inversely related to both the population of i and j and the distance between them.

In addition to GDP, population, and distance, we include in the following model variables related to whether both trading partners are members of a common alliance and a common preferential trading arrangement, whether either state is a major power, and the interactions among these factors.

As noted above, we use data on alliances and major powers compiled by the COW Project (Small and Singer 1969, 1982, 44–5) in this study. Furthermore, our analysis centers on the period since World War II, when the major powers have been the United States, the Soviet Union, Great Britain, France, and China. Although this list omits states that are major economic but not military powers—particularly West Germany and Japan—it seems appropriate in light of our emphasis on the relationship between political-military relations and trade. In order to assess the implications of excluding states that are solely economic powers from this list, however, we will analyze the sensitivity of our empirical results to whether West Germany and Japan are considered major powers.

---

[9] A number of studies have attempted to provide a theoretical justification for the gravity model's multiplicative functional form (e.g., Anderson 1979; Bergstrand 1985, 1989; Deardorff 1995; Helpman and Krugman 1985, chapter 8). One reason to use a multiplicative rather than a linear specification of this model is that as the economic size—measured in terms of GDP and population—of either i or j approaches zero, so, too, should trade flows between them (Deardorff 1995, 4).

Also included in the following model are variables that measure whether the trading partners (1) are at war with one another, (2) are parties to the General Agreement on Tariffs and Trade (GATT), (3) had a prior colonial relationship, and (4) have command economies. First, it is important to control for the effects of war because it may influence patterns of both alliances and trade. A wide variety of studies have drawn links between alliances and warfare (e.g., Levy 1989, 228–30, 235–6); and the available evidence indicates that wars tend to dampen trade flows (Gowa 1994; Gowa and Mansfield 1993; Mansfield 1994; Pollins 1989a, 1989b). Second, the effects of the GATT on trade are analyzed since the GATT is widely viewed as having provided the global institutional framework for international trade during the post–World War II era, and there is evidence that it has promoted trade flows among members (e.g., Pollins 1989a). Since distinctions often are drawn between the GATT and preferential trading arrangements in both theoretical and empirical studies of international trade (e.g., Eichengreen and Irwin 1995; Pollins 1989a; Yarbrough and Yarbrough 1992, 5), we consider their effects separately.

Third, prior colonial relations are likely to influence trade flows (Kleiman 1976; Pollins 1989a, 741; Srivastava and Green 1986). Furthermore, omitting this variable from the model risks generating biased estimates of preferential trading arrangements' effects on trade, since a number of such arrangements (including the Lomé Convention and other preferential arrangements involving the European Community/European Union) are comprised of states that had a previous colonial relationship.

Finally, we include a variable pertaining to whether both trading partners have a command economy. Many pairs of countries with command economies also have been allied during the post–World War II era. It is important to ensure that any observed effect of alliances on trade is not due to the unusual ability of states with a command economy to direct trade toward allies and away from other states.

Thus, the model we will estimate is:

$$\begin{aligned}\log X_{ij(t)} = {} & \log \beta_0 + \beta_1 \log GDP_{i(t-1)} + \beta_2 \log GDP_{j(t-1)} \\ & + \beta_3 \log POP_{i(t-1)} + \beta_4 \log POP_{j(t-1)} \\ & + \beta_5 \log DIST_{ij(t-1)} + \beta_6 \log ALLY_{ij(t-1)} \\ & + \beta_7 \log PTA_{ij(t-1)} + \beta_8 \log MP_{ij(t-1)} \\ & + \beta_9 (\log ALLY_{ij(t-1)} \times \log PTA_{ij(t-1)}) \\ & + \beta_{10} (\log ALLY_{ij(t-1)} \times \log MP_{ij(t-1)}) \\ & + \beta_{11} (\log PTA_{ij(t-1)} \times \log MP_{ij(t-1)}) \\ & + \beta_{12} \log GATT_{ij(t-1)} + \beta_{13} \log COL_{ij(t-1)} \\ & + \beta_{14} \log COM_{ij(t-1)} + \beta_{15} \log WAR_{ij(t-1)} + \log e_{ij(t)}. \end{aligned} \quad (2)$$

The dependent variable is the natural logarithm of the real value of exports from state i to state j in year t. The independent variables are defined as follows:

$\log GDP_{i(t-1)}$ and $\log GDP_{j(t-1)}$ are the natural logarithms of the real GDP of i and j, respectively, in year $t - 1$;

$\log POP_{i(t-1)}$ and $\log POP_{j(t-1)}$ are the natural logarithms of the national population of i and j, respectively, in year $t - 1$;

$\log DIST_{ij(t-1)}$ is the natural logarithm of the geographical distance between i and j in year $t - 1$;

$\log ALLY_{ij(t-1)}$ is a dummy variable that equals one if i and j are allied in year $t - 1$, zero otherwise;

$\log PTA_{ij(t-1)}$ is a dummy variable that equals one if i and j are party to a common preferential trading arrangement in year $t - 1$, zero otherwise;

$\log MP_{ij(t-1)}$ is a dummy variable that equals one if either i or j is a major power in year $t - 1$, zero otherwise;

$\log GATT_{ij(t-1)}$ is a dummy variable that equals one if both i and j are party to the GATT in year $t - 1$, zero otherwise;

$\log COL_{ij(t-1)}$ is a dummy variable that equals one if i and j had a colonial relationship ending in or before year $t - 1$ (provided that it concluded after the onset of World War II), zero otherwise;

$\log COM_{ij(t-1)}$ is a dummy variable that equals one if both i and j have a command economy in year $t - 1$, zero otherwise;

$\log WAR_{ij(t-1)}$ is a dummy variable that equals one if i and j are at war with each other in year $t - 1$, zero otherwise; and

$\log e_{ij(t)}$ is a stochastic error term.

The remaining variables are included in order to analyze whether, as we argued earlier, the interactions among alliances, preferential trading arrangements, and major powers help to explain patterns of bilateral trade.[10] A description of the data and coding procedures used in this analysis is provided in the Appendix.

In the following analysis, we include all pairs of countries for which complete data are available in 1960, 1965, 1970, 1975, 1980, 1985, or 1990 (years $t - 1$). The data are pooled across these seven years. The parameters in equation 2 are estimated using ordinary least squares (OLS), after controlling for country-specific and year-specific fixed effects by including a dummy variable for each country and year in the sample (except the United States and 1990, which are the respective reference categories). Because White tests (Greene 1993, 392–3, 450; White 1980) yield evidence of heteroskedasticity in each cross-section, White heteroskedasticity-consistent standard errors are used in this analysis.

Furthermore, because there is reason to expect the disturbances to exhibit autocorrelation, we estimate the parameters in equation 2 after including a lagged endogenous variable in the model. Doing so also is useful since, if alliances, preferential trading arrangements, or other independent variables in equation 2 are influenced by prior trade flows, then omitting a lagged endogenous variable from the model will generate biased estimates of their effects (Eichengreen and Irwin 1995). The logarithm of the lagged value of exports, however, is likely to be correlated with the error term in equation 2, thereby generating inconsistent estimates. We therefore construct an instrument for the lagged endogenous variable, a well-known procedure for ameliorating this problem (Greene 1993, 435–6; Maddala 1988, 392–5).[11] Estimates of the parameters in equation 2 are shown in Table 1. The model estimated without the lagged endogenous variable is referred to as model 1; that estimated with the lagged endogenous variable is referred to as model 1A.

---

[10] It should be noted that in antilogarithmic form, all the dummy variables in equation 2 take on values of e (the base of the natural logarithms) and one; as such, the logarithms of these variables take on values of one and zero.

ESTIMATES OF THE PARAMETERS

The results in Table 1 indicate that the variables in models 1 and 1A explain 55% of the variation in bilateral trade flows. Also, as expected, GDP is directly related to the value of exports, and population and distance are inversely related to the value of exports. The estimates of log $GDP_{i(t-1)}$ and log $GDP_{j(t-1)}$ are positive; the estimates of log $POP_{i(t-1)}$, log $POP_{j(t-1)}$, and log $DIST_{ij(t-1)}$ are negative; and each of them is statistically significant.

In addition, the results based on model 1A demonstrate that the lagged value of log $X_{ij(t)}$ is strongly related to its current value. Surprisingly, however, the estimate of this parameter is negative. Further analysis revealed that this result is due to collinearity between lagged log $X_{ij(t)}$ and the values of both GDP and population in year $t - 1$. To reduce the collinearity among these variables, we replace log $GDP_{i(t-1)}$ and log $GDP_{j(t-1)}$ with log $(GDP_{i(t-1)} \times GDP_{j(t-1)})$ and log $POP_{i(t-1)}$ and log $POP_{j(t-1)}$ with log $(POP_{i(t-1)} \times POP_{j(t-1)})$. This specification is referred to as model 1B in Table 1 and is closely related to that used in a number of previous studies (e.g., Eichengreen and Irwin 1995, Frankel 1993).[12] As expected, the estimate of log $(GDP_{i(t-1)} \times GDP_{j(t-1)})$ is positive, the estimate of log $(POP_{i(t-1)} \times POP_{j(t-1)})$ is negative, and both are statistically significant. Moreover, the estimate of lagged log $X_{ij(t)}$ based on model 1B is positive and significant, which accords with the findings of those studies that have included a lagged endogenous variable in a gravity model (e.g., Eichengreen and Irwin 1995).

It also is important to recognize that, as shown in Table 1, regardless of which model is analyzed, very

TABLE 1. Regression of Exports on GDP, Population, Distance, Alliances, Preferential Trading Arrangements, Major-Power Status, GATT, Prior Colonial Ties, Command Economies, and War, 1960–90

	Model		
Variable	1	1A	1B
log β_0	7.783*	2.462	12.014**
	(3.156)	(3.381)	(3.373)
log GDP_i	.687**	.781**	—
	(.041)	(.047)	
log GDP_j	1.322**	1.608**	—
	(.040)	(.076)	
log POP_i	−.872**	−.808**	—
	(.079)	(.083)	
log POP_j	−.974**	−.959**	—
	(.078)	(.083)	
log $(GDP_i \times GDP_j)$	—	—	.527**
			(.044)
log $(POP_i \times POP_j)$	—	—	−.869**
			(.083)
log $DIST_{ij}$	−.787**	−.790**	−.792**
	(.014)	(.014)	(.014)
log $ALLY_{ij}$	.192**	.214**	.213**
	(.049)	(.050)	(.050)
log PTA_{ij}	.499**	.490**	.466**
	(.038)	(.039)	(.039)
log MP_{ij}	.631**	.652**	.642**
	(.133)	(.124)	(.124)
log $ALLY_{ij}$ × log PTA_{ij}	.589**	.592**	.592**
	(.064)	(.065)	(.065)
log $ALLY_{ij}$ × log MP_{ij}	.082	−.005	−.016
	(.066)	(.063)	(.067)
log PTA_{ij} × log MP_{ij}	−.546**	−.548**	−.527**
	(.065)	(.067)	(.067)
log $GATT_{ij}$	.071	.050	.042
	(.037)	(.040)	(.040)
log COL_{ij}	1.812**	1.800**	1.791**
	(.083)	(.089)	(.089)
log COM_{ij}	.791**	.760**	.841**
	(.102)	(.110)	(.113)
log WAR_{ij}	−6.423**	−6.247**	−6.496**
	(.108)	(.112)	(.109)
lagged log X_{ij}	—	−.293**	.913**
		(.066)	(.014)
$\bar{R}^2$	.55	.55	.54
N	32,156	30,418	30,418

Note: Entries are unstandardized regression coefficients. Two-tailed tests are conducted for each regression coefficient. Figures in parentheses are White heteroskedasticity-consistent standard errors. Regressions include dummy variables for country-specific and year-specific fixed effects.

$^{*}p \le .05$; $^{**}p \le .001$.

---

[11] This instrument is created by regressing the lagged value of log $X_{ij(t)}$ on the lagged values of log $GDP_{i(t-1)}$, log $GDP_{j(t-1)}$, log $POP_{i(t-1)}$, log $POP_{j(t-1)}$, and a dummy variable for each year (except 1990) included in the analysis. Data on exports for 1956 and data on GDP and population for 1955 are used to generate values of the instrumental variable for 1961. The dearth of these data accounts for the decline in N of 1,738 when the lagged endogenous variable is included in the model and (as noted in the Appendix) is one reason we restrict our analysis to the period beginning in 1960.

[12] Frankel (1993, 58; see also Helpman and Krugman 1985) points out that entering the GDP of the importer and exporter in product form is consistent with theories of international trade based on imperfect competition. One implication of this specification is that two states are expected to conduct more trade if both have a medium-sized economy than if one has a large economy and the other has a small economy.

little variation exists in the signs, sizes, or significance levels of the remaining variables in equation 2.

Turning to the remaining variables, there is considerable evidence that wars depress trade and that a former colonial relationship and the existence of a command economy on the part of both trade partners promote commerce. In every case, the estimate of log $WAR_{ij(t-1)}$ is negative, the estimates of log $COL_{ij(t-1)}$ and log $COM_{ij(t-1)}$ are positive, and all of them are statistically significant. In addition, there is some evidence that GATT membership facilitates trade. The coefficient of log $GATT_{ij(t-1)}$ is positive in each case, but it is never statistically significant.

The Effects of Alliances and Preferential Trading Arrangements on Trade

The results in Table 1 also provide strong support for the arguments advanced earlier. They indicate that alliances and preferential trading arrangements each promote trade, that the effects of alliances are heightened if the trade partners include a major power, and that the combination of an alliance and a preferential trading arrangement generates more commerce than does either type of institution alone. The estimates of log $ALLY_{ij(t-1)}$, log $PTA_{ij(t-1)}$, and log $MP_{ij(t-1)}$ are positive and statistically significant based on all three models. Equally important for the purposes of testing the hypotheses described above, the estimates of log $ALLY_{ij(t-1)}$ × log $PTA_{ij(t-1)}$ and log $PTA_{ij(t-1)}$ × log $MP_{ij(t-1)}$ are significant in each instance, although there is no evidence that log $ALLY_{ij(t-1)}$ × log $MP_{ij(t-1)}$ has a statistically significant effect on trade. Moreover, the quantitative effects of alliances, preferential trading arrangements, and major powers on the predicted value of exports tend to be quite large.

In antilogarithmic form and for a given pair of states, *i* and *j*, in a given year, *t*, equation 2 can be expressed as:

$$X_{ij(t)} = C \times [ALLY_{ij(t-1)} \exp(\beta_6 + \beta_9 \log PTA_{ij(t-1)} + \beta_{10} \log MP_{ij(t-1)})] \times [PTA_{ij(t-1)} \exp(\beta_7 + \beta_{11} \log MP_{ij(t-1)})] \times [MP_{ij(t-1)} \exp(\beta_8)], \quad (3)$$

where

$$C = \beta_0 GDP_{i(t-1)}^{\beta_1} GDP_{j(t-1)}^{\beta_2} POP_{i(t-1)}^{\beta_3} \cdot POP_{j(t-1)}^{\beta_4} DIST_{ij(t-1)}^{\beta_5} GATT_{ij(t-1)}^{\beta_{12}} \cdot COL_{ij(t-1)}^{\beta_{13}} COM_{ij(t-1)}^{\beta_{14}} WAR_{ij(t-1)}^{\beta_{15}} e_{ij(t)}.$$

TABLE 2. Effects of Alliances, Preferential Trading Arrangements, and Major-Power Status on the Predicted Value of Exports, Based on Estimates in Table 1

Relationship between Countries *i* and *j*	Model 1	Model 1A	Model 1B
A. Countries *i* and *j* include a major power			
Members of a preferential trading arrangement			
Allies	425	403	394
Not Allies	179	181	179
Not members of a preferential trading arrangement			
Allies	247	237	231
Not Allies	188	192	190
B. Countries *i* and *j* are not major powers			
Members of a preferential trading arrangement			
Allies	360	365	356
Not Allies	165	163	159
Not members of a preferential trading arrangement			
Allies	121	124	124
Not Allies	100	100	100

Note: Entries are the predicted value of exports from country *i* to country *j*, based on equation 3, where *C* is set equal to 100.

As noted earlier, $ALLY_{ij(t-1)}$, $PTA_{ij(t-1)}$, $MP_{ij(t-1)}$, $GATT_{ij(t-1)}$, $COL_{ij(t-1)}$, $COM_{ij(t-1)}$, and $WAR_{ij(t-1)}$ are dummy variables that take on values of *e* and one; their natural logarithms therefore take on values of one and zero. For the purposes of analyzing the effects of alliances, preferential trading arrangements, and major powers on the predicted value of trade between a given pair of states in a given year, *C* can be treated as a constant. The entries in Table 2 are derived by arbitrarily setting *C* equal to 100. The estimates of $\beta_6 - \beta_{11}$ in Table 1 based on models 1, 1A, and 1B then are used to generate predicted values of $X_{ij(t)}$.[13] For each model, comparing the entries in Table 2 allows us to assess the proportional change in the predicted value of trade between a given pair of states in a given year if these states are allied rather than nonallied, are party to a common preferential trading arrangement rather than not, and include a major power rather than being composed solely of nonmajor powers, as well as the extent to which the quantitative effects of each factor are influenced by the other two factors.

---

[13] Rather than setting *C* equal to 100, we could use the observed values of the variables in equation 3, the dummy variables pertaining to each country and year, and the estimates of their regression coefficients to obtain the predicted dollar value of trade for a given pair of countries in a given year. Note, however, that for this given pair of countries and year, doing so would yield the same proportional increases in trade as those derived using the entries in Table 2. Furthermore, doing so often generates predicted values of trade that deviate noticeably from their corresponding observed values. This problem is common in models estimated in double-log form. There is substantial variation about the regression lines estimated using models 1, 1A, and 1B, and this variation is magnified when the value of trade, rather than the logarithm of this value, is analyzed. Consider, for example, the twenty pairs of countries for which the logarithm of exports is closest to the sample mean. On average, the predicted log of exports based on model 1 deviates from the observed log of exports only by about 5%. This figure is only slightly higher for the twenty pairs of countries that conducted the most trade, but it is much higher for those pairs that conducted the least trade. Since the absolute values of these residuals often are relatively large, however, the differences between the predicted and observed values of exports based on this model are much more pronounced than those between the predicted and observed values of the logarithm of exports.

The findings in Table 2 are consistent with the argument that alliances promote trade among members. A change from the absence to the existence of an alliance that does not include a major power increases the predicted value of trade between nonmajor powers by about 20% to 25% if these states are not members of a common preferential trading arrangement. If the trading partners include a major power and are not members of a preferential arrangement, a change from the absence to the existence of an alliance generates about a 20% to 30% increase in the predicted flow of exports. Furthermore, the predicted value of trade between allies that include a major power (and are not members of a common preferential trading arrangement) is roughly twice as large as the corresponding value between nonmajor-power allies.

That the effects of alliances are heightened if the trading partners are members of a common preferential trading arrangement also accords with our argument. Under these conditions, a change from the absence to the existence of an alliance yields an increase in the predicted value of trade of roughly 120% between nonmajor powers and of about 120% to 140% between states that include a major power.

Like alliances, preferential commercial arrangements exert a considerable influence on trade, but our results indicate they have a larger effect than alliances on trade flows between nonmajor powers, whereas the opposite holds for trade flows involving major powers. Instituting a preferential trading arrangement yields an increase of about 60% to 65% in the predicted value of exports between nonmajor powers that are not allied. Surprisingly, however, instituting such an arrangement decreases by about 5% the predicted value of trade between states that include a major power and are not allied.

As expected, the effects of a preferential trading arrangement are heightened if the members are allied. If allies establish a preferential commercial agreement, then the predicted value of exports increases by almost 200% between nonmajor powers and by roughly 70% between countries that include a major power.

These results provide strong support for our argument that the combination of an alliance and a preferential trading agreement generates a greater impetus to trade than does either of these institutions alone. As shown in Table 2, members of an alliance and a preferential trading arrangement conduct considerably more trade than do parties to either one or the other but not both. Moreover, allied preferential trading arrangement partners that include a major power engage in about 10% to 20% more trade than do their nonmajor-power counterparts. Indeed, the predicted value of trade for pairs of states that are allied, belong to a common preferential trading arrangement, and include a major power is the highest in Table 2 based on each model estimated, followed by that for nonmajor powers that are both allies and party to a common preferential trading arrangement, and that for major-power allies that are not members of a common preferential trading arrangement.

Definition of a Major Power

It was noted earlier that our analysis of major powers is based on a definition that emphasizes political-military strength. Economic powers lacking sufficient military capabilities to meet this criterion, however, also may influence the commercial effects of alliances and preferential arrangements on trade. Japan and West Germany are obvious examples of such states, and we therefore estimate the parameters in models 1, 1A, and 1B after including them among the major powers.

The results of this analysis tend to be very similar to those in Table 1, with two notable differences. First, the estimate of log $MP_{ij(t-1)}$ is, on average, about 40% larger when Japan and West Germany are considered major powers than when they are not. This result is hardly surprising, since both countries have been especially active participants in the international trading system during the period examined here.

Second, the estimates of both log $ALLY_{ij(t-1)}$ × log $MP_{ij(t-1)}$ and log $PTA_{ij(t-1)}$ × log $MP_{ij(t-1)}$ are much smaller than their corresponding estimates in Table 1. Thus, for example, instituting an alliance between states that include a major power generates roughly a 15% smaller increase in the predicted value of trade when Japan and West Germany are considered major powers than when they are not. This suggests that political-military major powers augment the openness of alliances and preferential trading arrangements to a greater extent than do other economic powers, which is consistent with the argument advanced above.

THE EFFECTS OF ALLIANCE FORMATION ON CHANGES IN TRADE

Thus far, we have examined the effects of preexisting alliances and preferential trading arrangements, on the level of trade among states. But it is important to ensure that our results are not undermined by simultaneity bias caused by any influence of trade on alliances or preferential arrangements. Various studies have suggested that high levels of trade can generate incentives for states to forge formal institutions, like preferential trading arrangements, that minimize future disruptions of commercial relations (e.g., Eichengreen and Irwin 1995, Yarbrough and Yarbrough 1992). We addressed this issue earlier by lagging the effects of the independent variables one year and by including the lagged value of exports in models 1A and 1B. We now examine whether the formation of alliances (and preferential trading arrangements) increases bilateral trade flows. We then examine the effects of trade flows and changes in them on the formation of alliances. Initially, we estimate the parameters in the following model:

$$\Delta X_{ij(t)} = \alpha_0 + \alpha_1 \Delta GDP_{i(t-1)} + \alpha_2 \Delta GDP_{j(t-1)}$$

TABLE 3. Regression of Change in Exports on Changes in GDP, Population, Alliances, Preferential Trading Arrangements, GATT, Command Economies, and War, 1960–90

Variable	Model 2	Model 2A
α_0[a]	917.833***	780.164***
	(72.440)	(81.574)
ΔGDP_i	−.000521*	−.000361
	(.000208)	(.000271)
ΔGDP_j	.000735	.001613***
	(.000431)	(.000459)
ΔPOP_i	−2.403*	−1.683
	(1.080)	(1.173)
ΔPOP_j	−4.207***	−3.767***
	(1.044)	(1.046)
$\Delta ALLY_{ij}$[a]	106.607	172.183
	(56.109)	(125.577)
ΔPTA_{ij}[a]	−63.376**	−87.851**
	(20.849)	(26.895)
$\Delta(ALLY_{ij} \times PTA_{ij})$[a]	236.174***	430.848***
	(67.700)	(101.215)
$\Delta(ALLY_{ij} \times MP_{ij})$[a]	99.819	104.358
	(115.261)	(227.728)
$\Delta(PTA_{ij} \times MP_{ij})$[a]	174.900*	−96.825
	(89.235)	(71.187)
$\Delta GATT_{ij}$[a]	−16.237	−27.620
	(12.309)	(15.662)
ΔCOM_{ij}[a]	622.404***	301.254***
	(184.746)	(64.213)
ΔWAR_{ij}[a]	−130.052	67.431***
	(76.238)	(17.161)
$\bar{R}^2$	.06	.06
N	23,940	20,892

Note: Entries are unstandardized regression coefficients. Two-tailed tests are conducted for each regression coefficient. Figures in parentheses are White heteroskedasticity-consistent standard errors. Regressions include dummy variables for country-specific and year-specific fixed effects.

[a] Parameter estimate and standard error are divided by 1,000,000.

$^{*}p \le .05$; $^{**}p \le .01$; $^{***}p \le .001$.

$$+ \alpha_3 \Delta POP_{i(t-1)} + \alpha_4 \Delta POP_{j(t-1)} + \alpha_5 \Delta ALLY_{ij(t-1)} + \alpha_6 \Delta PTA_{ij(t-1)} + \alpha_7 \Delta(ALLY_{ij(t-1)} \times PTA_{ij(t-1)}) + \alpha_8 \Delta(ALLY_{ij(t-1)} \times MP_{ij(t-1)}) + \alpha_9 \Delta(PTA_{ij(t-1)} \times MP_{ij(t-1)}) + \alpha_{10} \Delta GATT_{ij(t-1)} + \alpha_{11} \Delta\, COM_{ij(t-1)} + \alpha_{12} \Delta WAR_{ij(t-1)} + \Delta z_{ij(t)}. \quad (4)$$

In this model, referred to as model 2 in Table 3, the change in exports between states i and j is measured from year $t - 5$ to year t, the change in each independent variable is measured from year $t - 6$ to year $t - 1$, and $\Delta z_{ij(t)}$ is an error term. It is clear, however, that this specification may not completely alleviate any simultaneity between trade and the exogenous variables in the model. Changes in trade from year $t - 5$ to year $t - 1$ could, for example, lead to the formation of alliances or preferential trading arrangements during that time. We therefore estimate a second model, referred to as model 2A in Table 3, in which each independent variable is measured from year $t - 11$ to year $t - 6$. In this model, the effects of the independent variables are completely lagged, thereby further reducing any possibility of simultaneity bias.[14]

It should be noted that based on our sample of states and the period considered here, no changes occurred in the distance between states, the states that were major powers, and states that had a prior colonial relationship. These variables, therefore, are not included in equation 4.

As in our earlier analysis, OLS is used to derive estimates of the parameters in models 2 and 2A after pooling the observations over time and including dummy variables for each country and year in the sample (except the United States and 1960, which are the respective reference categories). Since evidence of heteroskedasticity exists in each cross-section, we continue to use White heteroskedasticity-consistent standard errors in our tests of statistical significance.

The results of this analysis are presented in Table 3. Particularly important is the fact that $\Delta ALLY_{ij(t-1)}$, $\Delta(ALLY_{ij(t-1)} \times MP_{ij(t-1)})$, and $\Delta(ALLY_{ij(t-1)} \times PTA_{ij(t-1)})$ are positive in each case.[15] These findings indicate that the formation of an alliance tends to increase trade among members. That this relationship is relatively weak, however, also is clear, since only the estimate of $\Delta(ALLY_{ij(t-1)} \times PTA_{ij(t-1)})$ is statistically significant based on either model.[16] In addition, the results in Table 3 indicate that the effects of alliance formation on trade tend to be larger in the five years after the alliance is formed than in the period during which it is formed. The estimate of $\Delta ALLY_{ij(t-1)}$ is about two-thirds larger and that of $\Delta(ALLY_{ij(t-1)} \times PTA_{ij(t-1)})$ is roughly three-quarters larger based on model 2A than model 2. The estimate of $\Delta(ALLY_{ij(t-1)} \times MP_{ij(t-1)})$, however, is virtually the same size regardless of whether the effects of alliance formation on changes in trade are fully lagged.

---

[14] It should be noted that the functional form of models 2 and 2A is linear rather than multiplicative, like models 1, 1A, and 1B. While a simpler linear model often has obvious advantages, under the present circumstances it could be argued that a multiplicative model is more appropriate. Hence, we also estimated the parameters in models 2 and 2A based on a multiplicative specification, whereby each variable was defined as the change in its natural logarithm. In only three cases—$\Delta GDP_{i(t-1)}$ and $\Delta(PTA_{ij(t-1)} \times MP_{ij(t-1)})$ based on model 2 and $\Delta WAR_{ij(t-1)}$ based on model 2A—did statistically significant estimates based on the linear specification (reported in Table 3) change signs but remain significant based on the multiplicative specification.

In addition, we included in equation 4 the level of exports from i to j in year t. The estimate of this variable was negative but insignificant based both on models 2 and 2A. Moreover, the signs, sizes, and significance levels of the remaining variables in equation 4 were not at all sensitive to the inclusion of this variable.

[15] One limitation of this analysis is that, based on the sample of states for which we have complete data, few pairs of countries changed from nonallies to allies during any of the five-year periods ($t - 6$ to $t - 1$ for model 2 and $t - 11$ to $t - 6$ for model 2A) in which the independent variables are measured. Indeed, most alliances and various preferential trading arrangements that existed during the era we examine were formed prior to its outset. As such, this and the following analysis only can be considered a preliminary first cut at the issues of whether the formation of alliances or preferential trading arrangements gives rise to changes in trade flows and whether trade flows contribute to their formation.

[16] The p-value of the estimate of $\Delta ALLY_{ij(t-1)}$ is .06 based on model 2 and .17 based on model 2A. Thus, the estimate based on model 2 would have been significant at the .05 level had we conducted a one-tailed rather than a two-tailed test.

Whereas the formation of alliances between states that include a major power does not significantly increase trade flows, the formation of preferential trading arrangements between such states does. This increase, however, is realized only in the immediate aftermath of their formation. The estimate of $\Delta(PTA_{ij(t-1)} \times MP_{ij(t-1)})$ is positive and significant based on model 2 but is negative and not significant based on model 2A. Furthermore, the estimate of $\Delta PTA_{ij(t-1)}$ is negative and significant based on both models. In combination with our earlier results, these findings suggest that the trade-promoting effects of preferential arrangements that do not include allies or a major power occur only some time after they are formed.

The effects of GATT membership on trade seem to follow a similar pattern. Whereas our earlier results indicated that pairs of states belonging to the GATT conduct somewhat more trade than do other pairs, changes leading both trade partners to become partners to the GATT actually generate a mild decrease in trade. Indeed, the estimate of $\Delta GATT_{ij(t-1)}$ is negative, but small and insignificant, based on both models. There also is evidence that the onset of war depresses trade but that these effects are short-lived. The estimate of $\Delta WAR_{ij(t-1)}$ is negative and insignificant based on model 2 but positive and significant based on model 2A. In addition, changes leading both trade partners to have a command economy yield increases in trade: Both estimates of $\Delta COM_{ij(t-1)}$ are positive and significant. Finally, increases in the importer's GDP and decreases in the exporter's GDP and in the population of both trade partners give rise to increases in commerce. The estimate of $\Delta GDP_{j(t-1)}$ is positive, those of $\Delta GDP_{i(t-1)}$, $\Delta POP_{i(t-1)}$, and $\Delta POP_{j(t-1)}$ are negative, and five of the eight estimates are significant.

THE EFFECTS OF TRADE ON ALLIANCE FORMATION

The preceding results indicate that allies conduct more trade than do nonallies and that the formation of alliances tends to generate increases in trade. To further ensure that our findings do not reflect the effects of trade flows or changes in trade on the formation of alliances, we conduct two additional analyses.

First, we define a variable that is equal to the log of the odds that states i and j form an alliance during the period from year $t + 1$ to year $t + 4$, where we observe one if an alliance is formed during this period and zero otherwise. Like our earlier analysis, trade flows between i and j are measured in year t (where $t = 1956$, 1961, 1966, 1971, 1976, 1981, and 1986). These data are pooled and a logistic regression is conducted to estimate the effects of trade flows on the probability of alliance formation. The results indicate that the maximum likelihood estimate of trade is positive but small (9.29×10^{-11}) and not statistically significant ($t = .87$).

Second, we analyze whether the change in trade between i and j during the period from year $t - 5$ to year t influences the probability that they form an alliance during the period from year $t + 1$ to year $t + 4$, using the same procedure described above. The maximum likelihood estimate of the change in trade also is positive but small (2.94×10^{-10}) and not statistically significant ($t = 1.16$).

In addition, we introduced into these models other variables included in our earlier analyses and continued to find no evidence that trade flows or changes in them influence the formation of alliances. We also estimated these models using a multiplicative functional form and found no evidence that trade or changes in trade affect alliance formation, controlling for other variables (such as distance) included in our earlier analyses. These findings, therefore, provide no indication that our earlier results are undermined by problems of endogeneity.

DISCUSSION AND CONCLUSION

The results of this study indicate that alliances and preferential trading arrangements both promote the flow of trade and that in combination they generate more commerce than does either alone. These findings are consistent with the argument that firms and private traders have incentives to reduce the risks of opportunism by foreign governments. Investing in relation-specific assets to service the market of an ally or a preferential trading arrangement partner reduces these risks. These risks are further reduced if such a partner is also an ally.

That alliances facilitate trade among members also accords with the argument that the security externalities fostered by commerce create incentives for states to trade more freely with allies than with adversaries. These incentives are especially pronounced for states with the ability to influence their terms of trade. Major powers are likely to have that ability, and this is consistent with our finding that pairs of allied states which include a major power conduct considerably more trade than allied pairs which do not. In addition, major-power alliances tend to be more durable and stable than those comprised solely of nonmajor powers. This tendency further reduces the risks of opportunism faced by investors in dedicated assets to service an ally's market. It also reduces the risk that a government's current allies will become adversaries in the near future and use the augmented political-military capacity derived from intraalliance trade to undermine its security.

Illustrative of the effects of major-power alliances is the case of the United States and South Korea. The United States has long sought to foster South Korean economic growth in order to bolster its security vis-à-vis North Korea and other potential adversaries. To this end, the United States pressured South Korea to adopt an export-led growth strategy and provided a market for a large portion of these exports (Haggard and Moon 1983). Indeed, it is widely acknowledged that these two countries have enjoyed a " 'special' . . . economic and trade relationship based primarily on strategic and other noneconomic factors" (Yang 1986, 34). The United States (and its allies) also has severely limited trade with North Korea in an attempt to undermine its security.

The heightened tendency for major powers to promote trade with allies at the expense of political rivals is not limited to this one example. Cohen argues that:

> from the start of the Cold War, official Washington's chief objective was to reconstruct the war-ravaged economies of Western Europe and Japan, and maintain the vigor of the undamaged Canadian economy, so that these countries could serve as effective barriers to communist expansion. . . . Toward this end, . . . the United States later encouraged an outflow of private investment from the United States, particularly to Canada but also to Europe, and it promoted through GATT a broad program of worldwide liberalization of industrial trade that frequently benefited its allies directly at its own expense (1991, 102; see also Gowa 1994).

In fact, U.S. policymakers seem to have recognized the contribution that free trade can make to the security of participants and the need to discriminate between friends and foes in this regard. As Dean Acheson argued in the late 1940s, "the preservation and development of sound trading relationships with other countries of the free world [that is, U.S. allies] is an essential and important element in the task of trying to build unity and strength in the free world" (quoted in Freeland 1985, 325).

Equally important in this regard was the denial of commercial benefits to adversaries of the United States and its allies. Chief among the efforts to achieve this end was COCOM, which was explicitly designed to stem the flow of commerce that might enhance the security of the Soviet Union and its allies (Mastanduno 1992). Due to its effect, "by the early 1950s, American trade with the East was negligible, while trade between Eastern and Western Europe continued but at much reduced levels" (Pollard and Wells 1984, 355–6).

These illustrations and our statistical results are consistent with the findings of most previous research on the effects of alliances on trade, almost all of which has analyzed commerce between or involving major powers (Gowa 1994, Gowa and Mansfield 1993, Mansfield and Bronson 1997, Summary 1989). But our results indicate that alliances comprised of nonmajor powers also promote trade and that the combination of an alliance and a preferential trading arrangement provides a particularly potent impetus to trade, important issues that have not been addressed to date.

Moreover, our results do not reflect the influence of war, command economies, prior colonial relations, or GATT membership. We controlled for the effects of each of these factors, and, except for the GATT, each has a strong influence on trade flows. That wars depress trade is consistent with various studies which have found that conflict inhibits trade, while cooperation promotes it (Polachek 1980; Pollins 1989a, 1989b). As we mentioned earlier, however, these studies did not analyze the effects of alliances on trade, and Pollins (1989a, 757) noted that these results might not account for variations in trade due to alliance relations. Our findings indicate that, in addition to conflict, alliances are salient influences on commerce.

The observed effects of command economies, prior colonial relations, and GATT membership on bilateral trade flows also accord with the existing literature (Kleiman 1976, Mansfield and Bronson 1997, Pollins 1989a, Srivastava and Green 1986), although the GATT's effect is somewhat weaker than expected. But alliances and preferential trading arrangements have strong and large influences on trade even after controlling for these factors. Thus, our findings pertaining to alliances and preferential trading arrangements are not, for example, due to the tendency for the Soviet Union to direct trade flows within the Warsaw Pact and the Council for Mutual Economic Assistance. Nor are they outgrowths of the tendency for some preferential trading arrangements and alliances to be comprised of states with a former colonial relationship or membership in the GATT.

Much research on the determinants of bilateral trade flows has focused on the effects of preferential trading arrangements. Consistent with the findings of this research, our results indicate that these arrangements had a marked influence on trade after World War II. As we mentioned earlier, however, very few studies have compared the effects of preferential trading arrangements with those of alliances; and no research has examined the extent to which the effects of preferential trading arrangements depend on whether commercial institutions include a major power or are comprised of allies.

Yet, these issues are centrally important. Based on our results, preferential trading arrangements have a larger influence than alliances on trade flows between nonmajor powers, whereas the opposite holds for states that include a major power. Furthermore, absent an alliance, the effects of preferential commercial treatment on trade depend very little on whether the trading partners include a major power. The predicted value of trade conducted by nonallied members of a preferential trading arrangement is about 10% larger if one partner is a major power than if both are not major powers. But regardless of major-power status, the effects of a preferential trading arrangement depend fundamentally on whether its members also are allies. Indeed, allied members of a preferential trading arrangement conduct roughly 120% to 140% more trade than do their nonallied counterparts (refer to Table 2).

Moreover, most studies of preferential trading arrangements omit alliances from the analysis; had we done so, too, substantial bias would have been introduced into our estimates of preferential trading arrangements' effects. For example, estimates of log $PTA_{ij(t-1)}$ based on models 1, 1A, and 1B increase by an average of about 50% when log $ALLY_{ij(t-1)}$, log $ALLY_{ij(t-1)} \times$ log $MP_{ij(t-1)}$, and log $ALLY_{ij(t-1)} \times$ log $PTA_{ij(t-1)}$ are deleted from these models. Since preferential trading arrangements often are comprised of political-military allies (Mansfield 1993), research that focuses solely on discriminatory commercial blocs therefore seems likely to confuse their effects with those of alliances and to systematically overstate the influence of preferential trading arrangements on commerce.

One central purpose of this paper was to analyze whether the combination of an alliance and a preferential trading arrangement provides a greater impetus to commerce than does either type of institution alone. Our results provide strong support for this argument. Nonmajor powers conduct roughly 120% to 200% more trade if they are party to both an alliance and a preferential trading arrangement than if they are either allies or members of such an arrangement (but not both). Allies that belong to a preferential trading arrangement and include a major power conduct about 70% more trade than do allies that include a major power but are not members of a preferential trading arrangement and roughly 125% more trade than nonallied members of such an arrangement that include a major power. Moreover, the predicted value of trade between states that are allies, members of a common preferential trading arrangement, and include a major power is greater than under any other set of conditions examined in this study, followed by the predicted value of trade between nonmajor powers that share both an alliance and a preferential trading arrangement (refer to Table 2).

These findings indicate that the interplay between alliances and preferential trading arrangements is fundamental to explaining patterns of international trade. It is clear that a wide variety of alliances and preferential trading arrangements exist and that different types of each may have different influences on trade. It is equally clear that our analysis is confined to the post–World War II period and that the effects of alliances and preferential commercial treatment during this era may not correspond to their effects during earlier periods. Our results strongly suggest, however, that the pursuit of power conditions the pursuit of plenty. In so doing, this paper provides considerable evidence that an understanding of international politics is central to the study of international trade.

APPENDIX: DATA SOURCES AND CODING

Described here are the data used in this article and some coding issues pertaining to our analyses.

Data on exports are taken from the International Monetary Fund (IMF), *Direction of Trade* (various years). The IMF presents these data in nominal U.S. dollars. Following Eichengreen and Irwin (1995), we use the U.S. wholesale price index and producer price index to create real values of exports. Furthermore, the IMF often does not distinguish between pairs of countries for which so little trade is conducted that the trade flow is reported as zero and pairs of countries for which data are not available. As such, neither type of pair is included in any of our analyses.

Data on GDP and population are taken from the Mark 5.6 version of the Penn World Table (Summers and Heston 1991). In order to maintain consistency with the procedure for deflating the data on exports described above, we generate nominal values of GDP using the data provided in the Penn World Table on per capita GDP expressed in current prices and population. We then deflate these values using the U.S. wholesale price index and producer price index. This data set does not include values of per capita GDP and population for a number of countries in 1990. Where it is possible to do so, we substitute values of these variables in 1989 for missing values in 1990.

Consistent with previous studies using gravity models, we measure distance based on the shortest distance in nautical miles between ports in states *i* and *j*. If at least one trading partner is land-locked, then we rely on land distances (either road or rail) to measure the distance between them. Data on geographical distances are taken from the Defense Mapping Agency, *Distances between Ports* (1985); Fitzpatrick and Modlin (1986); *The Times Atlas of the World* (1992); and *The Times Concise Atlas of the World* (1972).

Data on alliances are taken from Small and Singer (1969) and preliminary updates of these data provided by the COW Project, dated March 25, 1993. We consider all neutrality agreements, ententes, and defense pacts listed by Small and Singer to be alliances. Since they sought to explain wars, Small and Singer did not include wartime alliances in their compilation. They noted, however, that for other purposes it would be important to amend their list correspondingly, and previous studies of the relationship between alliances and trade have done so (Gowa 1994, Gowa and Mansfield 1993). We therefore include as allies all states fighting together in any interstate war listed by Small and Singer. In order to maintain consistency with the earlier studies of alliances and trade, we also include as allies the United States and Japan, even though this pair of countries is not included in Small and Singer's data set. This decision is made because the Japanese–United States Security Treaty (1951–60) and the Treaty of Mutual Cooperation and Security (1960–present) served much the same purpose as an alliance, including the stipulation that the United States come to Japan's assistance in the event it is attacked (Grenville 1974, 270, 286–7). However, the decision to code these states as allies has no bearing on any of our results.

Data on preferential trading arrangements are taken from Belous and Hartley (1990, 141–3), Schott (1989), and Hartland-Thunberg (1980). We also include the Council for Mutual Economic Assistance, the Southern African Development Coordination Conference, and the Eastern and Southern African Preferential Trade Area because various studies have identified them as preferential trading arrangements (Hanlon 1986, Kisanga 1991, Orimalade and Ubogu 1984, Pomfret 1988).

Data on major powers and wars are taken from Small and Singer (1982; Singer and Small 1994). It should be noted that only trade data between India and Pakistan are available for the pairs of states that were at war during the years $(t - 1)$ analyzed here.

Data on GATT membership are taken from various issues of GATT's *Basic Instruments and Selected Documents* (Geneva) and *International Trade* (Geneva). Data on prior colonial relations are taken from Kurian (1992). Data on command economies are taken from Kornai (1992, 6–7) for the period from 1960 to 1985 and from Staar et al. (1991) for 1990.

All the independent variables in equation 2 are measured at year $t - 1$. For each pair of states, *i* and *j*, it should be noted that alliances, preferential trading arrangements, GATT membership, prior colonial relations, command economies, and war are coded based on the end of this year.

All pairs of countries for which complete data are available in 1960, 1965, 1970, 1975, 1980, 1985, or 1990—and that Small and Singer (1969, 1982; Singer and Small 1994) list as members of the interstate system—are included in our sample. Small and Singer only provide data on alliances and interstate wars for members of the interstate system; and, in some of the years mentioned above, their list of members excludes countries for which data on other variables analyzed here are available.

The number of dyads for which complete data exist ranges from 2,647 in 1960 to 5,667 in 1990. These differences arise because the number of members of the interstate system increased during this period, as did the availability of data on GDP and trade flows. Data on trade flows and GDP are quite limited for earlier years, especially for less developed countries and states with command economies. This is the primary reason we do not analyze periods prior to 1960, despite the obvious desirability of doing so if data on a wider range of countries were available.

REFERENCES

Aitken, Norman D. 1973. "The Effect of the EEC and EFTA on European Trade: A Temporal and Cross-Section Analysis." *American Economic Review* 63 (December):881–92.

Anderson, James E. 1979. "A Theoretical Foundation for the Gravity Equation." *American Economic Review* 69 (March):106–16.

Anderson, Kym, and Richard Blackhurst. 1993. "Introduction and Summary." In *Regional Integration and the Global Trading System*, ed. Kym Anderson and Richard Blackhurst. London: Harvester Wheatsheaf.

Belous, Richard S., and Rebecca S. Hartley, eds. 1990. *The Growth of Regional Trading Blocs in the Global Economy*. Washington, DC: National Planning Association.

Bergstrand, Jeffrey H. 1985. "The Gravity Equation in International Trade: Some Microeconomic Foundations and Empirical Evidence." *Review of Economics and Statistics* 67 (August):474–81.

Bergstrand, Jeffrey H. 1989. "The Generalized Gravity Equation, Monopolistic Competition, and the Factor-Proportions Theory in International Trade." *Review of Economics and Statistics* 71 (February):143–53.

Brada, Josef C., and Jose A. Mendez. 1983. "Regional Economic Integration and the Volume of Intra-Regional Trade: A Comparison of Developed and Developing Country Experience." *Kyklos* 36 (4):589–603.

Bueno de Mesquita, Bruce. 1981. *The War Trap*. New Haven: Yale University Press.

Cohen, Benjamin J. 1990. "The Political Economy of International Trade." *International Organization* 44 (Spring):261–81.

Cohen, Benjamin J. 1991. *Crossing Frontiers: Explorations in International Political Economy*. Boulder, CO: Westview.

Deardorff, Alan V. 1984. "Testing Trade Theories and Predicting Trade Flows." In *Handbook of International Economics*, ed. Ronald W. Jones and Peter B. Kenen. Amsterdam: North-Holland.

Deardorff, Alan V. 1995. "Determinants of Bilateral Trade: Does Gravity Work in a Neoclassical World?" Paper presented at the National Bureau of Economic Research conference on Regionalization of the World Economy, Woodstock, Vermont.

Defense Mapping Agency. 1985. *Distances between Ports*. 5th ed. Washington, DC: Defense Mapping Agency.

Eichengreen, Barry, and Douglas A. Irwin. 1995. "The Role of History in Bilateral Trade Flows." Paper presented at the National Bureau of Economic Research conference on Regionalization of the World Economy, Woodstock, Vermont.

Frankel, Jeffrey, A. 1993. "Is Japan Creating a Yen Bloc in East Asia and the Pacific?" In *Regionalism and Rivalry: Japan and the United States in Pacific Asia*, ed. Jeffrey A. Frankel and Miles Kahler. Chicago: University of Chicago Press.

Freeland, Richard M. 1985. *The Truman Doctrine and the Origins of McCarthyism: Foreign Policy, Domestic Politics, and Internal Security 1946–1948*. New York: New York University Press.

Frieden, Jeff. 1988. "Sectoral Conflict and U.S. Foreign Economic Policy." *International Organization* 42 (Winter):59–90.

Fitzpatrick, Gary L., and Marilyn J. Modlin. 1986. *Direct-Line Distances*. International ed. Metuchen, NJ: Scarecrow.

Gasiorowski, Mark, and Solomon W. Polachek. 1982. "Conflict and Interdependence: East-West Trade and Linkages in the Era of Detente." *Journal of Conflict Resolution* 26 (December):709–29.

Gaubatz, Kurt Taylor. 1996. "Democratic States and Commitment in International Relations." *International Organization* 50 (Winter): 109–39.

Gowa, Joanne. 1994. *Allies, Adversaries, and International Trade*. Princeton: Princeton University Press.

Gowa, Joanne, and Edward D. Mansfield. 1993. "Power Politics and International Trade." *American Political Science Review* 87 (June): 408–20.

Greene, William H. 1993. *Econometric Analysis*. 2d ed. New York: Macmillan.

Grenville, J. A. S. 1974. *The Major International Treaties 1914–1973*. London: Methuen.

Haggard, Stephan, and Chung-in Moon. 1983. "The South Korean State in the International Economy: Liberal, Dependent, or Mercantile?" In *The Antinomies of Interdependence: National Welfare and the International Division of Labor*, ed. John Gerard Ruggie. New York: Columbia University Press.

Hanlon, Joseph. 1986. *Beggar Your Neighbor*. London: Villiers Publications.

Hartland-Thunberg, Penelope. 1980. *Trading Blocs, U.S. Exports, and World Trade*. Boulder, CO: Westview.

Helpman, Elhanan, and Paul Krugman. 1985. *Market Structure and Foreign Trade: Increasing Returns, Imperfect Competition and the International Economy*. Cambridge: Massachusetts Institute of Technology Press.

Kisanga, Eliawony J. 1991. *Industrial and Trade Cooperation in Eastern and Southern Africa*. Hong Kong: Avebury.

Kleiman, Ephraim. 1976. "Trade and the Decline of Colonialism." *Economic Journal* 86 (September):459–80.

Kornai, Janos. 1992. *The Socialist System: The Political Economy of Communism*. Princeton: Princeton University Press.

Kurian, George Thomas. 1992. *Encyclopedia of the Third World*, 4th ed. New York: Facts on File. Vols. 1–3.

Leamer, Edward E., and Robert M. Stern. 1970. *Quantitative International Economics*. Boston: Allyn and Bacon.

Levy, Jack S. 1989. "The Causes of War: A Review of Theories and Evidence." In *Behavior, Society, and Nuclear War*, ed. Philip E. Tetlock et al. New York: Oxford University Press.

Linnemann, Hans. 1966. *An Econometric Study of International Trade Flows*. Amsterdam: North-Holland.

Maddala, G. S. 1988. *Introduction to Econometrics*. New York: Macmillan.

Mansfield, Edward D. 1993. "Effects of International Politics on International Trade." In *Regional Integration and the Global Trading System*, ed. Kym Anderson and Richard Blackhurst. London: Harvester Wheatsheaf.

Mansfield, Edward D. 1994. *Power, Trade, and War*. Princeton: Princeton University Press.

Mansfield, Edward D., and Rachel Bronson. 1997. "The Political Economy of Major-Power Trade Flows." In *The Political Economy of Regionalism*, ed. Edward D. Mansfield and Helen V. Milner. New York: Columbia University Press.

Mastanduno, Michael. 1992. *Economic Containment: CoCom and the Politics of East-West Trade*. Ithaca: Cornell University Press.

McKeown, Timothy J. 1991. "A Liberal Trade Order? The Long-Run Pattern of Imports to the Advanced Capitalist States." *International Studies Quarterly* 35 (June):151–72.

Morrow, James D. 1990. "Alliances and Asymmetry: An Alternative to the Capability Aggregation Model of Alliances." *American Journal of Political Science* 35 (November):904–33.

Orimalade, Adeyinka, and R. E. Ubogu, eds. 1984. *Trade and Development in Economic Community of West African States*. Delhi, India: Vikas.

Pelzman, Joseph. 1977. "Trade Creation and Trade Diversion in the Council of Mutual Economic Assistance: 1954–1970." *American Economic Review* 67 (September):713–22.

Polachek, Solomon W. 1980. "Conflict and Trade." *Journal of Conflict Resolution* 24 (March):55–78.

Pollard, Robert A., and Samuel F. Wells, Jr. 1984. "1945–1960: The Era of American Economic Hegemony." In *Economic and World Power: An Assessment of American Diplomacy since 1789*, ed. William H. Becker and Samuel F. Wells, Jr. New York: Columbia University Press.

Pollins, Brian M. 1989a. "Conflict, Cooperation, and Commerce: The Effects of International Political Interactions on Bilateral Trade Flows." *American Journal of Political Science* 33 (August): 737–61.

Pollins, Brian M. 1989b. "Does Trade Still Follow the Flag?" *American Political Science Review* 83 (June):465–80.

Pomfret, Richard. 1988. *Unequal Trade: The Economics of Discriminatory International Trade Policies*. Oxford: Basil Blackwell.

Rogowski, Ronald. 1989. *Commerce and Coalitions: How Trade Affects Domestic Political Alignments*. Princeton: Princeton University Press.

Schott, Jeffrey J., ed. 1989. *Free Trade Areas and U.S. Trade Policy*. Washington, DC: Institute for International Economics.

Singer, J. David, and Melvin Small. 1994. *Correlates of War Project: International and Civil War Data, 1816–1992*. No. 9905. Ann Arbor: Inter-University Consortium for Political and Social Research.

Small, Melvin, and J. David Singer. 1969. "Formal Alliances, 1816–1965: An Extension of the Basic Data." *Journal of Peace Research* 6 (3):257–82.

Small, Melvin, and J. David Singer. 1982. *Resort to Arms: International and Civil Wars, 1816–1980*. Beverly Hills, CA: Sage.

Srivastava, Rajendra K., and Robert T. Green. 1986. "Determinants of Bilateral Trade Flows." *Journal of Business* 59 (October):623–40.

Staar, Richard F., et al., eds. 1991. *Yearbook on International Communist Affairs*. Stanford: Hoover Institution Press.

Summary, Rebecca M. 1989. "A Political-Economic Model of U.S. Bilateral Trade." *Review of Economics and Statistics* 71 (February): 179–82.

Summers, Robert, and Alan Heston. 1991. "The Penn World Table (Mark 5): An Expanded Set of International Comparisons." *Quarterly Journal of Economics* 106 (May):327–68.

Times Atlas of the World. 1992. 9th ed. New York: Times Books.

Times Concise Atlas of the World. 1972. New York: Times Books.

Tinbergen, Jan. 1962. *Shaping the World Economy: Suggestions for an International Economic Policy*. New York: The Twentieth Century Fund.

Waltz, Kenneth. 1979. *Theory of International Politics*. Reading, MA: Addison-Wesley.

White, Halbert. 1980. "A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity." *Econometrica* 48 (May):817–38.

Williamson, Oliver E. 1985. *The Economic Institutions of Capitalism*. New York: Free Press.

Winters, L. Alan, and Zhen Kun Wang. 1994. *Eastern Europe's International Trade*. Manchester: Manchester University Press.

Yang, Jai-Hoon. 1986. "U.S.-Korean Economic and Trade Relations: Building a New 'Special Relationship.'" In *United States–Korea Relations*, ed. Robert A. Scalapino and Han Sung-joo. Research Papers and Policy Studies No. 19. Berkeley: Institute of East Asian Studies, University of California.

Yarbrough, Beth V., and Robert M. Yarbrough. 1992. *Cooperation and Governance in International Trade: The Strategic Organizational Approach*. Princeton: Princeton University Press.

Beyond Quid Pro Quo: What's Wrong with Private Gain from Public Office?

ANDREW STARK *University of Toronto*

The longstanding debate over whether officials should be permitted to profit personally from public office has moved to the center of public discourse in the United States. As the House Ethics Manual *put it recently, it is of concern simply when an officeholder "cash[es] in on official position," regardless of whether his or her official performance was in any way impaired as a result. Although the question of private gain from public office is richly implicative of a number of fundamental issues in political theory—in particular the ongoing controversy over where to draw the borders between private and public as well as the nature of official fiduciary responsibility—it has yet to undergo political-theoretic analysis. In what follows, I examine both the conceptual and normative issues emergent in the debate over private gain from public office.*

In political commentary on official corruption in the United States, it has become a commonplace to observe that instances of bribery, office-buying, and other gross forms of malfeasance have, over time, given way to a variety of more subtle forms of quid pro quo and conflict of interest (Vaughn 1990, 419, 423). Recently, however, public discourse over political ethics has expanded to include yet another realm, lying beyond even the more subtle forms of quid pro quo or conflict of interest, a realm with its very own conceptual and normative perplexities: private gain from public office. It is of concern, as the House *Ethics Manual* states, simply when officeholders "cash in on official position," even if their judgment or performance in office is in no way compromised, impaired, or encumbered as a result (House Committee on Standards 1992, 83). Yet, sustained theoretical analysis of this burgeoning issue in U.S. politics—what is private gain from public office and what may be wrong with it—has yet to commence. According to Dennis Thompson (1995, 49), "what exactly the principle [against private gain from public office] prohibits is not so clear, and its ambiguities are the source of many of the problems in implementing ethics in our time."

Private gain from public office not only plays a growing role in U.S. political discourse but also promises to be of considerable theoretical interest. The daily workings of democratic discourse and institutions continually throw up complex new situations that, cumulatively, can have the effect of fundamentally reframing concepts or issues of theoretical import. In the case of private gain from public office, as I shall show, the surrounding discourse is rich in, and implicative of, central political-theoretic debates over the distinction between public and private as well as the nature of official fiduciary responsibility.

To distinguish private gain from public office from the kind of quid pro quo that preoccupies much scholarly inquiry, consider that in its most generic form a quid pro quo comprises two elements: It involves officials whose judgment is compromised, encumbered, or impaired because (1) they have the official capacity to affect the interests of a private party to whom (2) they are in some way beholden because they have received from that party something of value for which they have provided no private-market consideration in return (so that if the transfer is to be recompensed, it could be recompensed only by an official act; see Rose-Ackerman 1978). Of course, public discourse and scholarly inquiry also display a preoccupation with situations that partake of only one but not the other of quid pro quo's two chief characteristics. Much public debate, for example, centers on cases in which an official's role affects a particular private interest that transfers value to the official, but in which—because the official provides proportional private economic consideration in return—s/he claims not to be beholden. For example, while serving as secretary of the Air Force in 1955, Harold Talbott maintained a consulting practice whose clients included several Air Force contractors. As others have done in similar circumstances, Talbott claimed that because his firm furnished private services in return for the contractors' payments, there was no possibility of his having become beholden to them, obligated to recompense them with an official act (in Talbott's case, the argument failed, and he was forced to resign).

Conversely, another strand of discourse surrounds situations in which an official receives unrecompensed value from a private interest and hence becomes beholden to that interest, even if the official can in no way affect that interest in his or her public capacity. "Unlike other conflict [laws, some statutes] do not require any connection between the payor and the government agency in which the payee work[s], or the specific government work performed by the payee" (Nolan 1992, 89–90). In regulating private subvention for any kind of official activity, these laws are not meant to prohibit quid pro quo per se—many other regulations do that—so much as prevent public decision making from being controlled by any private party, even one whose interests may not remotely be at stake.

Private gain from public office, however, provokes normative concern even when officials are incapable of affecting the fortunes of the paying private interest in question *and* are in no way beholden to it. Private gain

---

Andrew Stark is Associate Professor of Management, University of Toronto, Toronto, Canada M5S 3E6.

The author thanks Stuart Gilman, Robert Howse, John Rohr, and Dennis Thompson for helpful comments and the Woodrow Wilson International Center for Scholars for a fellowship which supported this research.

from public office, in it most generic form, refers to any situation in which officials draw on knowledge, skills, experiences, stature, or prestige derived from their public office to reap some form of private gain—from membership on corporate boards, to fees for professional services, to honoraria for appearances, to expense-paid invitations to charitable events. Private gain from public office can occur contemporaneously with an official's occupancy of role, as when government employees sell manuals for firms seeking to do business with their agency or teach courses on issues related to their official work (U.S. Office of Government Ethics 1989, 485). And private gain from public office can arise subsequent to an official's role occupancy, as when former officials write books and give speeches, or advise clients on how to approach government, assisted by knowledge or stature they derived therein (Baker 1995).

Whatever the case, neither element of quid pro quo need be present. First, the private payor need not be one whose interests the official can in any way affect in his or her public capacity (and when the private payor does happen to be an entity whose interests the official can affect, in which case impaired official judgment may also be an issue, we can identify central strands of discourse concerned only with the element of private gain from public office; see House Committee on the Judiciary 1992, 85; Roberts 1988, 7). Second, since the official provides compensating private-market consideration (board attendance, professional services, speeches, and the like), s/he is not necessarily beholden to the payor. The question, in all cases, is whether the official's capacity to gain privately was enabled, advantaged, or assisted by his or her occupancy of a position of public trust. It is thus "possible to imagine an official who acts impartially, does not play favorites, and is a model of public decorum," but who nevertheless falls afoul of injunctions against "the use of public office for private gain, an independent and specific objective involved in the field of conflicts of interest" (Association of the Bar 1960, 7).[1]

My aim in the following is twofold. First, I explore the principal conceptual ambiguities that arise surrounding the question "what is private gain from public office?" Second, I evaluate the main normative complexities that emerge in connection with the question of what, if anything, is wrong with private gain from public office. That a strenuous conceptual and normative debate exists is well attested in public discourse. On the one side, typically, one finds officials or scholars concerned to diminish the offensiveness of private gain from public office or, at least, contract the category of circumstances to which the term applies (Thompson 1995, 21). On the other, one finds government agencies or political commentators seeking to expand the class of acts covered by the term or attach to it a negative normative charge (Jehl 1993, Johnson 1986). Given the perennial status of discourse, and the pristine nature of scholarly inquiry, I have chosen not to pronounce definitively or prescriptively on the conceptual and normative issues. As Sidney Hook (1980, 11) states, the principal contribution philosophy makes to public policy is to "sharpen the issues at point in public controversy" or, alternatively, where appropriate, to "disclose the absence of genuine issues." I therefore do not impose my own moral "yardstick" on debate (to borrow from Walzer 1987, 20, 22, 26) but, rather, operate in "an interpretive mode" which "struggles to find meaning in the 'text' of public discourse," in "the way in which people explain and justify what they do, the stories they tell," and I suggest the particular kinds of cases under which each side is likelier to be on stronger ground. In doing so I follow Hirschman (1991, 6–7), in focusing "on a few common or typical arguments unfailingly made" by either side, and Smith (1988, 102), in examining a "few major cases that seem representative."

CONCEPTUAL ISSUES

"Private gain" and "public office"—the two concepts at play here—are each ambiguous in meaning. And it is the exploration and exploitation of their ambiguity that, largely, structure debate over the conceptual question "what *is* private gain from public office?" Private gain signifies that culpable officials, in seeking or enjoying the gain in question, are doing so outside their official role. The modifier "private" suggests a kind of gain—a trip on a corporate jet, attending an association meeting at a resort, an expense-paid trip to a charity event—that does not, ought not, or need not redound to the official as part of his or her job. If the official's responsibilities required the official to board the corporate aircraft, or be present at the association meeting, or attend the charity event, then there would be no "private" gain, just the exercise of office. But assume that the activity is emphatically not part of an official's role; that it therefore uncontroversially constitutes private gain. To say that such private gain flows from public office implies that the official enjoys such gain only because s/he happens to occupy that official role. The invitation to the charity event or, more diffusely, the knowledge, skills, and stature which now attract business, would not have evolved had the person never held that office.

Faced with the charge of using public office for private gain, implicated officials will deny either that the gain is private or else that it results from anything public. Thus, on the one hand, implicated officials will insist that boarding the corporate jet, attending the association meeting, or even participating in the charitable event was part of their job—something they had to do in role anyway—and hence constitutes something neither private nor gain. It represents no new net augmentation to their psychic or economic utility; it is in no way an "extra" but simply all in a day's work. On the other hand, conceding that a trip, meal, or invitation does constitute private gain and not official duty, officials—pointing to facets of their out-of-role selves such as a personal association with the charity or innate talents or skills—will then deny that it flows to them because of their public office.

---

[1] Private gain from public office also differs from "appearance cases," in which officials are sanctioned for allowing the appearance of impropriety to arise notwithstanding the fact that they may have committed no real improprieties; that is, in circumstances in which they may have violated no other extant ethics law (a category that includes the rules prohibiting private gain from public office). Just as the two sets of laws cover entirely different categories of offenses (*Sanjour* 1995), so they provoke distinct strands of discourse: Neither of the two major issues typical of "appearance discourse"—the fairness of factually prejudging or of retroactively imposing norms on officials (Stark 1995)—have any role to play in discourse over private gain from public office, which, as I will show, centers on the private-public distinction and the nature of official fiduciary responsibility.

For there to be a coherent concept of private gain from public office, it must take place in a realm outside the borders of official role (otherwise there can be no private gain) but where the role nevertheless casts some sort of penumbra-shadow (otherwise the gain fails to result from public office). Implicated officials, for their part, want either to expand the borders of the role (so that there has been no private gain, only the undertaking of public responsibilities) or else contract the penumbra into nothingness (so that any private gain results from privately, not publicly, rooted capacities). But as I now show, the need simultaneously to expand and contract the realms of public and private, a need which besets argument on both sides if in inverse ways, places each side, whether implicated officials or those accusing them, at cross-purposes with itself.

Private Gain or the Execution of Office?

In 1991 a North Carolina environmental group invited William Sanjour, an employee of the Environmental Protection Agency (EPA), to address its members (expenses paid) on the dangers involved in locating a hazardous waste incinerator in Northampton County. Sanjour, an in-house critic of EPA's willingness to allow such projects, was forced to decline the invitation, citing the rules against private gain from public office. Sanjour and a colleague then launched an action against EPA in D.C. district and subsequently circuit court, seeking to contest the rules covering private gain from public office, at least as applied to his prototypical kind of case. *Sanjour* v. *EPA* and the attendant congressional hearings (*Sanjour* 1992a, 1992b, 1995; House Committee on the Judiciary 1992) emblematize one of the key conceptual debates at play in private gain from public office: Accused officials, citing those qualities of their challenged activities that had a "public" or "official" quality, will claim they were operating in role. Accusing agencies or departments, while denying that the official was operating in role, will cite those same "public" characteristics as evidence that the official's role extended into and enabled the private gain in question.[2]

Agencies and departments, to begin with, have a strong incentive (to the extent that there is any ambiguity or leeway to the issue) to categorize the activity in question—in Sanjour's case, speaking to the North Carolina environmental group—as private or unofficial, not as an exercise of office. For to the extent that the activity *does* fall under the official's in-role responsibilities, its subvention by private sources falls afoul of 18 U.S.C. 209, which prohibits private payments for official acts. To the extent that the acts in question are official, in other words, one is entitled to presume that they already have been paid for, or ought ultimately to be paid for, by the public treasury (House Committee on the Judiciary 1992, 98). Agencies thus have reason to nudge any given privately underwritten act in the direction of the private, to offload it into the unofficial; for if it were official, then the agency itself could be expected to come up with the funds to defray it.[3]

Following this imperative, EPA urged that Sanjour's expense-paid attendance at the North Carolina event could not be classed as an official activity and hence would necessarily constitute private gain. Private, because the agency would not have sent him anyway as part of his job; Sanjour, moreover, would have been advancing his own views, not those of the agency. And the trip would have constituted gain, because the travel and lodging themselves were of some value.[4] There being nothing wrong with private gain in and of itself, the agency further contended it would have resulted from Sanjour's holding public office. Sanjour would not have been invited had he not been an EPA official; his talk would have drawn on knowledge derived from his job and dealt with agency-related subject matter; the group, in inviting Sanjour, may well have been under some illusion that he would be speaking in an official capacity; indeed, the invitation was directed to him at the office. All of this suggested to the agency that while Sanjour's speech would have constituted a private act, it would have accrued to him because of his public office and not (as EPA regulations put it) his "individual knowledge and accomplishments."

---

[2] Sanjour established ripeness by challenging an extant federal regulation, 5 C.F.R. 2635.807, which prohibits certain kinds of private gain from public office. And he established standing as a federal employee governed by it.

[3] A category of activities has emerged which both parties, agency and official, as a consequence of their conflicting imperatives, find it convenient to describe as both public and private, both official and personal (or, more exactly, to which it is convenient for them each to ascribe the least problematic features of both the public and the private). In this realm, acts are sufficiently unofficial so that the agency can disclaim the responsibility to provide them itself, but sufficiently official that the officeholder can deny a charge that they constitute private gain. The U.S. Office of Government Ethics, piecemeal in its rulings, has evolved a category of activities that are not considered part of an agency's "functions" but that nevertheless may lie within its sphere of "interests" (House Committee on the Judiciary 1992, 141). That an activity—say, a particular kind of speaking engagement to a private group—lies outside the agency's functions means the agency is not itself required to underwrite it. But that it falls within the agency's realm of interests means that the official, in having it privately underwritten, is not being personally enriched.

[4] Sanjour, it should be noted, queried how "the Justice Department [could] think that giving up my vacation time for a coach ticket on USAir and a night in a room at a budget motel in the boondocks [could] constitute . . . using public office for private gain" (House Committee on the Judiciary 1992, 25). But the question of whether the travel and lodging were of any subjective value to Sanjour is obviously not cognizable; otherwise, officials could deny private gain in any case (e.g., a junket to Hawaii) simply by denying its subjective utility to them. True, perhaps a de minimis rule could be written into the standards governing private gain from public office, exempting certain kinds of minimal excursions, although doing so still would not touch the main issues at stake. In any case, de minimis exemptions are themselves notoriously controversial.

The very same observations, however, suggested something very different to Sanjour. Specifically, they suggested not that it was by virtue of his public office, and his public office alone, that he would have found himself privately enriched by a trip to North Carolina, rather that his proposed trip resembled far more the exercise of his public office than an instance of private activity or gain. The EPA may not have regarded the activity as "official," but some segment of the public, in addressing the invitation to him at the agency and welcoming him as an EPA spokesman, obviously did—and why should the agency's construction of his official role be controlling? Sanjour further affirmed that the knowledge and information upon which he would have drawn for his speech were indeed derived from his public office—conceding the very point that in other contexts implicated officials are loath to do—but for Sanjour, once again, the public derivation of his activity suggested that it was, in fact, a public activity. The taxpayer, Sanjour urged, had subsidized his acquisition of knowledge and therefore was entitled to its fruits. His environmental insights belonged to the citizen, and it was part of his role to disseminate them. Likewise, the fact that the subject of his speech related to his official duties further buttressed, for Sanjour, the claim that delivering it would have been an in-role not a personal act.

What for the EPA constituted evidence that Sanjour's trip (while private gain) was occasioned by his public role—invitation addressed to the office, drawing on officially derived knowledge, concerned with official subject matter—was for Sanjour evidence that the trip was indeed part of his public role, not supererogatory private gain. In other words, what for the agency were indications that an official role casts a shadow far outside its boundary signified for Sanjour territory well within the boundaries of role.

Sanjour prevailed, but on First Amendment grounds. That is, he argued that any regulation prohibiting such a trip—the purpose of which was to enable him to speak on a public issue—would have interfered with his freedom of speech. In agreeing, the *Sanjour* (1995) court was able to avoid the crucial question of how to determine the borders between public role and private activity, and so left debate over private gain from public office very much open (Potts 1995, 2).[5]

Private Payments, Public Payments: A Clarification

As the term is used in discourse, what makes a gain "private" is not that it comes from a private payor, although it usually does, but that it accrues to a private activity the official undertakes (hence Sanjour's attempt to expand the boundaries of his role and the EPA's attempt to contract them). Private gain can arise, then, when an official bills out-of-role trips or expenses to the public treasury, not just to a private individual or group. During his tenure as George Bush's White House chief of staff, for example, John Sununu sought reimbursement from the public treasury for what arguably were nonofficial acts. Sununu flew on a government plane to New Hampshire, where, while conducting some government business, he attended his son's high school graduation. Sununu also billed the government for travel and hotel costs he incurred in attending a New Hampshire celebrity ski event for the Christa McAuliffe Sabbatical Trust (Wines 1991).

Where do such incidents fit in the spectrum of private gain from public office or in the spectrum of ethical transgressions more generally? In cases of private gain from public office, the official's occupancy of a public role has a gainful effect on his on her private, out-of-role activities. While there is an element of that in the Sununu-type case, the overlaying problem is actually a more traditional form of conflict of interest, in which the official's private interests affect his or her public, in-role activities. Sununu's in-role activities—his deployment of his own time and other official resources—were altered, in part, by his private needs. The test of conflict of interest per se is whether the official may have done something in role in order to advance a private interest that otherwise s/he would not have done (Carson 1994). Sununu's acts met this test; they constituted a form of self-dealing.

The debate surrounding Sununu's ventures, however, illustrates a larger point. Sununu felt compelled to argue, in each case, that the activity concerned *was* official. He claimed he would not have attended his son's graduation had he not been in Boston anyway on official business (thereby choosing, perhaps, to place one facet of his personal virtuousness in question in order to shore up his public rectitude). And if he would not have gone on such a trip anyway, that is, for personal reasons, then, Sununu argued, he must have undertaken it for official reasons, and its being underwritten by the public treasury would not be classifiable as private gain. Sununu likewise designated his attendance at the ski event an official act, since the McAuliffe programs had been classified as an example of President Bush's Points of Light initiative. In fact, Sununu contended that his every act was official, if *official* is defined as what is in the interests of the administration. The White House counsel, however, took a different view, as did most commentators. For example, although the Points of Light initiative was indeed an official program, it was explicitly devoted to recognizing private institutions that had taken over public functions from the state. Sununu's argument contravened the program's spirit, even as it highlighted the ambiguities of the public-private realm (Gray 1991, 3, 4, 5, 7).

---

[5] In this way, *Sanjour* (1995) represents another in a line of cases that have restored to officials, albeit haltingly, their constitutional rights when those rights come into conflict with other goods Congress seeks to promote (see, e.g., the honoraria ban case *United States* v. *National Treasury* 1995, or the Hatch Act cases discussed in Benda and Rosenbloom 1992, 40). But in those cases, the evil the government was seeking to prevent—the financial or partisan corruption of the civil service—is at least conceptually and normatively comprehensible. Here, by contrast, the principal question is to what extent private gain from public office represents an evil in the first place, let alone one whose preclusion would justify any attendant impairment of the constitutional rights of officials. Moreover, political speech constitutes only a confined subset of acts (others include attending events, professional services, etc.) that could constitute private gain from public office. In most cases, officials' First Amendment rights are simply not implicated, which is why *Sanjour* settled little.

From Public Office or Personal Abilities?

Whether the funds underwriting an act such as a speech or a trip come from a private party or the government, the officeholder (if there is any ambiguity involved) has an incentive to classify the act as official; the agency, as private and hence as gain. But this is only half the debate. There is a point sufficiently far along the spectrum at which the official as well as the agency accept that the act in question is uncontroversially private. Here, in seeking to deny that the private gain had anything to do with occupancy of public office, the implicated official will reverse strategy. S/he will insist that the invitation did not in fact go to the office, that his or her benefactors were in no way interested in or even cognizant of his or her official role, and that the knowledge and information on which the official drew were personal and private, based on individual accomplishments—all just the opposite of Sanjour's line of argument. Two cases illustrate the argumentative hazards this strand of discourse imposes on its participants: The first centers on the paradoxes imposed on officials; the second on those imposed on government agencies.

In 1992, the mayor of Washington, D.C., Sharon Pratt Kelly, spoke to a Chicago meeting of the Municipal Bond Investors Assurance Company, which paid her an honorarium and hotel expenses. When the media inquired about the trip, the mayor's spokespeople gave "conflicting information about whether she was representing the District" (Henderson 1992). One of Kelly's staff members, Vada Manager, said the mayor was indeed representing the city; Ellen O'Connor, the deputy mayor for finance, added that the District of Columbia stood "to benefit from the mayor's meeting with the three hundred investors, analysts and bankers" assembled and from her "encouraging them to do business with the city." But the mayor's interim chief of staff, Karen Tramontano, stated that Kelly's comments at the meeting "were not made in her official capacity as mayor, nor did she seek to secure benefits for the District." Instead, Kelly was in attendance as a "nationally recognized figure," and her "conduct was in complete accord with the letter and spirit of the applicable law."

The dichotomy in Kelly's response illustrates the competing imperatives officials face in such situations. On the one hand, they are impelled to classify the act as an official responsibility (Kelly was invited to Chicago in her role as mayor), in which case it does not constitute supererogatory private gain. On the other, officials are impelled to insist that their capacity to perform the act results from purely private, personal accomplishments (Kelly was invited because she is a nationally recognized figure in her own right) and hence does not derive from public office. Officials will either try to argue that they are acting well within the bounds of their public office, in which case there is no private gain, or concede that they are acting out of role and insist that their capacity to do so has absolutely nothing to do with their public office, attributing it exclusively to their own private talents. But the very arguments that suggest the mayor was invited because of her personal stature and not her public office—she did not attend in her capacity as mayor; she did not seek to secure benefits for the district—undermine the contention that her attendance comprised part of her official responsibility and hence in no way constituted private gain.

In the same way, Sununu (as noted) was motivated to insist that his attendance at the McAuliffe Foundation event fell within his official responsibilities; since he would not have made the trip on his personal initiative, his going at taxpayers' expense would have represented no private gain. That argument failed when the White House counsel formally deemed the McAuliffe trip as outside Sununu's official responsibilities. But doing so left open the possibility that if the government could not reimburse Sununu for his travel and lodging, then the foundation itself could—assuming, of course, that Sununu had not been invited because of his official role but because of who he was outside that role. Accordingly, Sununu reversed himself. He pointed out that he had attended the McAuliffe event for a number of years prior to becoming the president's chief of staff, which suggested that he would have been invited anyway, separate and apart from his role. Sununu, in other words, intimated that the invitation came to him not as a result of his public office but because of his personal qualities, traits, and history; his personal links to and support for the charity (Devroy and Babcock 1991; Gray 1991, 16). But in observing that he had regularly attended for many years (and that going this year thus was related to him as a private individual and not his public office), Sununu effectively subverted his earlier claim that he would not have gone anyway (and attending therefore would have related to his public office, hence could not have represented private gain). The same evidence—his habitual attendance patterns—was used to determine both whether the act constituted private gain *and* whether it resulted from public office; it helped define the border between public and private in either case.

The Kelly and Sununu cases display an argumentative tension that is the reverse of Sanjour's situation. Sanjour, recall, courted the danger that whatever constituted for him evidence that his trip was an official act and not private gain (the invitation went to the office; he would have drawn on officially derived knowledge) constituted for the agency evidence that, though the act was unofficial, Sanjour's capacity to undertake it resulted from his public office, from his in-role knowledge and stature. Conversely, for Kelly and Sununu, whatever goes to show that in going to Chicago or New Hampshire they were in no way drawing on public office—on in-role experiences and stature—also undermines any conjoint claim that the activity of going was in fact an official responsibility and hence could not have represented private gain.

Government agencies run into the same set of argumentative constraints, although they face a different kind of internal strategic dilemma. For example, a recent U.S. Office of Government Ethics (OGE) opinion (1990, 122, 123) dealt with an official who, in his private capacity, wanted (expenses privately paid) to lead a delegation of businesspeople to Europe. "On close examination," OGE ruled, "the connection between his proposed activity and his agency responsibility is so fundamental that characterizing it as private would be impossible." Here, the government, not the official, sought to push the expense-paid trip in the direction of the "official," so as to attribute his receipt of it to the public office he held. There was sufficient proximity between the purposes of the trip, on the one hand, and the kind of in-role responsibilities the official possessed, on the other, that in inviting him his hosts may have thought they would benefit from official support and capacities on their trip. Hence, OGE was able to stamp the trip *misuse of office* for private gain.

Yet, recognizing that the very argument it mounted to show the trip redounded to public office (not the employee's personal talents) also could be used to classify it as an official responsibility (hence not private gain), the opinion was careful to state: Even though the employee's "proposed activity might more properly be characterized as official, that can occur only if it is specifically determined by the [agency] to be appropriate involvement in an official capacity" (U.S. Office of Government Ethics 1990, 123). Aware that the exact claims supportive of its case on the one hand weakened it on the other, OGE had to carve out a realm between the official and the unofficial in which such a thing as *private* gain from *public* office is possible. OGE wanted to preempt any argument by the official to the effect that the trip-invitation resulted from purely private, personal aptitudes (as opposed to public office) without giving him the opportunity then to stamp it as an official responsibility (as opposed to private gain).

Role Differences: Legislative and Executive

Thus far, I have not distinguished the kind of private gain that flows from (1) the prestige or stature officials may enjoy from their public role and (2) the knowledge or skills they may derive from that role. Both may be sources of private gain from public office, but in fact important distinctions between them do emerge in discourse, depending on the nature of the official's role (legislative or executive; political appointee or career service).

Consider, to begin with, two principal differences between ethics rules covering private gain from public office in the legislative and executive branches. First, when the chair of the Senate Armed Services Committee addresses, expenses paid, a foreign-policy group on defense issues, doing so is not considered private gain from public office. When the secretary of defense does the same thing, it is. Second, when a mid-level Securities and Exchange Commission official addresses, expenses paid, an entomological society on her butterfly collection (to take a serviceable example), it is rarely considered private gain from public office. When a legislator or a senior committee aide does the same thing, it often is. Again, I abstract here from the question of whether the group (the expense payor) is one whose interests the legislator or the executive official can affect. The question is: What accounts for the greater leniency accorded legislative officials in the first instance and the greater leniency accorded executive officials in the second?

What is pertinent here is a particular kind of distinction between the knowledge or skills (on the one hand) and the prestige or stature (on the other) derived from official role. First, legislative knowledge and skills differ from the executive variety. There survives a tradition whereby legislators, as distinct from executive officials, are thought (indeed, expected) to derive relatively more of their knowledge and understanding from out-of-role experience than from role-engendered expertise. Legislative facts are deemed more universally accessible, and to arrive through many more personal avenues, than those that undergird certain kinds of executive-branch decisions. When it comes to activities that represent private gain, such as an expense-paid speaking trip on defense issues not strictly required by an official's role responsibilities, the legislative ethics committees are thus more likely to attribute the legislator's capacity to perform them to his or her personal capabilities and qualities, and less to abilities or understandings the legislator derived exclusively within role. Legislators, so the ethics committees presume, glean more of their knowledge and skills out of role as citizens, and less of it in role as professionals (House Committee on the Judiciary 1960, 33).

Second, more of a legislator's in-role prestige and status extend out of role—the legislator is more of a celebrity—than is the case for anonymous executive officials. Consider a legislator's attendance at charity events, which is regarded as a private act. The consideration a legislator offers by attending—namely, the capacity to draw a crowd—is private, above and beyond anything s/he would be required to perform within his or her official role (House Committee on Standards 1992, 38). But here, the legislative ethics committees are likely to attribute the legislator's capacity to perform the act, to provide consideration by drawing a crowd, to the legislator's official role, not personal stature. Hence the ethics committees' historic practice of allowing only members who were celebrities before entering Congress to receive reimbursement to attend charity events. But for most legislators, who outside their role would not have drawn a crowd, there exists a prohibition against reaping whatever material gain is entailed in an expense-paid charity event.

More generally, when it comes to private gain that does *not* flow to anything which could remotely be classed as role-engendered skill or knowledge, the legislative ethics committees are more likely to attribute it to the prestige and the stature of role than would be the case in analogous executive-branch situations. If an invitation to join a board or a firm is extended to legislators prior to their assuming office, the invitation is deemed not to redound to the role. Otherwise, there exists a heavy presumption that the legislator "would not have been invited except for [his or her] official position . . . and the prestige" s/he draws from it (House Committee on Standards 1992, 103, 249). Legislators, as distinct from all but the most senior executive officials, resemble public figures more than private citizens; their official status is ever-present and ever-evident. Executive officials, by contrast, are much more able, in out-of-role situations, to shuck the shadow of role entirely; their role more quickly fades in the view of others. Or, to take the initial example, a mid-level SEC official invited expenses paid to address an entomological society about her butterfly collection is, presumably, not being engaged to do so because of any role-derived prestige.

In sum: In that legislators are citizens whereas executive officials are professionals, a certain kind of private gain—that redounding to knowledge or skills—is more readily attributed to legislators' personal talents, insights, and experience and less readily to their official role. But in that legislators are public persons whereas executive officials are private persons, another kind of private gain—that having to do with prestige or stature—is more readily attributed to the legislative role than to anything developed personally or sui generis.

What of intraexecutive differences, in particular between political appointees and career officials? For many discourse participants, such differences in role imply that what may count as troubling private gain from public office for high-level executive officials should not count for those occupying lower ranks (Clark 1996, 95). First, political appointees and other senior officials are simply more likely than lower-ranking civil servants to have acquired, in role, a kind of prestige or stature on which they can capitalize privately. Hence, some senior officials, unlike those lower ranked, are prophylactically prohibited from earning *any* outside income. Second, there is a sense in which the kind of knowledge or skill that can be attributed to the official's public role in the case of political appointees—especially the so-called in-and-outers—is more appropriately attributed in the case of lower-level officials to the person as a private individual. Lower-ranking officials (say, scientists who for twenty years have worked at the Food and Drug Administration) can make a plausible claim that most of the knowledge or skill they may have acquired from their role has long since ceased to be their "master's property." For an employee in this situation, as Williston (1937, sec. 1646) puts it, such knowledge or skills after a time are simply more sensibly seen as "his own property; they *are* himself." By contrast, political appointees—because their assumption of official role is usually so relatively short-lived—are less likely to be able to claim that whatever knowledge or skill their role has imparted has been incorporated into (or constitutes) their out-of-role self. A lawyer who serves as deputy U.S. Trade Commissioner for two years and then gains privately as a result of it is more likely to confront charges that any knowledge or skill gained during her tenure results from the role occupied, not solely from attributes of the out-of-role official herself. Hence, senior officials are prohibited from earning outside income for activities even loosely related to the subject matter of their agency, while those lower ranked are not so restricted as long as the activity is not directly related to agency programs. In sum, because of the hierarchy of office, the prestige that attaches to role is greater for high-ranking officials. Conversely, because of the longevity of tenure for low-ranking officials, the knowledge that derives from role is more likely to seem their own property. For both reasons, private gain from public office occupies a greater conceptual space for high-ranking executive officials.

NORMATIVE ISSUES

It is one thing to confront the thorny conceptual issue of the extent to which, in any given case, an official acting out of role (itself a contested enough concept) benefits from drawing on aspects of the self—knowledge, skills, expertise, relationships, stature, prestige—developed in role. It is another to address the no less difficult normative question as to what, exactly, is wrong with an out-of-role official drawing remuneratively on in-role talents and prestige, assuming that s/he is neither beholden to nor can affect the interests of those providing the remuneration. Private gain from public office seems to occupy a realm not only conceptually ambiguous but also normatively innocuous.

Broadly speaking, two kinds of answer are available for this question. The first runs along these lines: There may be nothing wrong, per se, with officials gaining privately from their role and especially from skills, stature, and capacities they developed while occupying it. We do, however, want to discourage people from serving in government if (part of) their motivation for doing so is ultimately to profit privately from public office. The second normative objection has it that something *is* wrong when officials profit out of role from what they have become in role, regardless of whether their motivation is or ever was to do so. This second criticism—and it is the one that most fully animates discourse—resolves itself from a complaint against private gain from public office per se into (as I show below) a more focused concern with the "unfair advantage" that having occupied public office may lend officials, or former officials, whenever they find themselves competing with members of the public who are also acting in the private marketplace. But while a person's capacity to turn his or her officially derived skills or stature to private account may be harmful to some part of the public in any given case, it also may be helpful to others. Hence, the government itself, in recruitment advertisements for the U.S. Army, in effect promises precisely to provide skills and stature that the official can use remuneratively out of role. It promises that public office *will* lead to private gain, on the grounds that such a practice will be beneficial not only to the official but also to (parts of) the broader public. In the discussion below, I draw on concepts of the fiduciary to sort out this disagreement over the normative "charge" we should attach to private gain from public office.

First, however, why do we not want serving in office those who (as the first objection would have it) are motivated to draw on their government experience and stature for private gain, regardless of whether they are actually successful in doing so (Brauer 1987, 192; Safire 1985)? One answer seems to dominate: We can readily imagine officials so motivated conducting themselves, in office, in ways they would not otherwise were their motivations more public spirited. Officials determined to use their time in government to acquire marketable skills and capacities may work more preponderantly on certain files, spend more time cultivating certain kinds of contacts, and devote more energy to acquiring a certain kind of reputation, than they would were they less concerned to use their time in office to develop marketable skills and capacities. Yet, that being the case, the motivational problem would seem to resemble closely—perhaps even collapse into—a more traditional conflict-of-interest problem involving altered or impaired in-role judgment and performance, where ill and real consequences could follow, in role, from an official's encumbered state of mind. After all, motivation goes to state of mind. Officials motivated by possibilities for private gain to enter or serve in public office have a conflict of interest (their in-role decisions and actions could be affected by their desire to optimize possibilities for private gain). If our concern is with officials motivated to profit personally from government service (regardless of whether they actually do so), then our problem lies not with private gain from public office per se but with conflict of interest. It lies with the extension of private motivations into the role, not with the extension of role into private activities.[6]

If our concern lies, however, with an official profiting privately from government service regardless of whether s/he was ever motivated to do so, regardless of any conflict of interest or impaired official performance, then we must look elsewhere for normative sustenance. Specifically, any objection to "pure" private gain from public office, that is, gain that occurs in the absence of any possibility of encumbered judgment, must be rooted in some kind of fiduciary notion of office. As Ernest Weinrib (1975, 16) has observed, the fiduciary has two obligations: "His duty must not conflict with his interest, and he must not profit from his position." Fiduciary law explicitly distinguishes profiting from position per se, separate and apart from any conflict of interest or encumbrance on judgment, as an impropriety to be guarded against. It thus seems to afford the most promising conceptual vehicle available for exploring the normative issues of private gain from public office. But if fiduciary law in general (or, more exactly, as applied to the professions of law and business) is fairly well developed (Weinrib 1975, 16), it is less so on the profession of government in particular. This is not to say that notions of trusteeship, stewardship, and custodianship are not evident in scholarship surrounding the obligations of officials (Dobel 1990; Frederickson 1991; Gortner 1991, 58; Kass 1990). But no explicit, dominant theory of official fiduciary obligation has entered and suffused public discourse (Clark 1996, Pierce 1989).

I will not here formulate and then impose my own theory of official fiduciary obligation. Rather, by referring to public debate, I will identify the particular understanding of fiduciary obligation that undergirds the objection to private gain from public office, along with a competing fiduciary notion on which such an objection would be ill-founded. Because fiduciary concepts are better developed in the other major professions—law, business, and medicine—I will draw on them to lend definition and distinctiveness to the notions of official fiduciary obligation implicit in discourse over private gain from public office.

To begin with, consider a fundamental way in which the fiduciary concept, when applied to government, inverts the structure it assumes in most other professions. This inversion is equally manifest in the argumentation mounted by those normatively troubled *and* by those normatively unperturbed by private gain from public office; it constitutes a shared assumption of discourse. In law, business, and medicine, on the most common conceptions, the professional (the lawyer, manager, or doctor) is thought to have "fiduciary" or "role-moral" obligations (I use the terms interchangeably) to pursue and protect certain interests possessed by a defined, identifiable set of principals: clients, shareholders, patients. In addition, on this understanding, the professional is thought to have a sometimes conflicting set of "ordinary-moral" obligations to the interests of a broader public, society at large. Role-moral obligations—those the professional bears toward his or her principal—are heightened duties of singular commitment and devotion. They allow the professional to show a keen partiality toward the interests of clients, patients, or shareholders, subject to certain ordinary-moral obligations. The latter—which the professional owes toward all others, an undifferentiated public or society as a whole—comprise minimalist or baseline duties, duties to be fair and decent of the kind we bear toward anyone, including those with whom we have no special relationship (Goldman 1980).

If the role-moral/ordinary-moral rubric has general applicability, then the situation of government can be seen to reverse that of the other professions in one key respect. For if the government official has any kind of fiduciary or role-moral obligation, it is one that redounds precisely to the undifferentiated public, to society as a whole. It is the public itself—and not, generally, particular agency clients, stakeholders, and so forth—that constitutes the official's principal, the entity to whose interests the official is primarily or singularly committed (Walzer 1972). Accordingly, the kind of obligation the official bears toward those specific agency "client groups" with whom s/he deals on behalf of the public-principal (welfare recipients, business contractors, broadcast licensees) would fall into the ordinary-moral class. The official does not bear these client groups fiduciary obligations (which are reserved for the public), although the official does bear them ordinary-moral obligations of baseline fairness and decency of the sort s/he possesses toward anyone. In government, the imposition of a fiduciary construct thus would direct the official's role-moral obligations toward the *public* and the official's ordinary-moral obligations toward particular *clients or groups*. The reverse is the case for other professions.

---

[6] Some have argued that there is nothing wrong with being motivated to enter public office in order to profit personally. After all, government salaries are below par, and the only way a career in the public sector would be open to those less well-to-do is to allow officials to turn it to account. But below-par government salaries argue more convincingly for a scheme of increased public-sector compensation or, at most, for relaxing barriers to private payments for public acts. That is, in an era of government fiscal constraint we may want to extend to private parties a greater opportunity to help underwrite official salaries, albeit salaries publicly set and explicitly designed to fit officials' actual in-role exertions and activities. Below-par official salaries do not argue for removing bars to private gain from public office. Such gain may or may not bear any relation to the official's in-role exertions and responsibilities, and pursuing it may in fact impair the official in executing those responsibilities (see Brudney and Clark 1981, 1033; Kelman 1993, 250).

This is a simple distinction, and one could quibble with it on any number of dimensions. It glosses over, for example, the question of whether the official's fiduciary obligation is in fact directed toward his or her agency or even to the government in general rather than to the public at large. Analogous questions, however, surface in business (over the much-debated issue of whether the manager's role-moral obligations flow toward the corporation instead of the shareholder) and in law (whether the lawyer's role-moral obligations flow to the court as well as the client). Along another dimension, some may argue that officials have role-moral, fiduciary obligations not just to the public but to particular clients (welfare officers, on this argument, have fiduciary obligations to the particular individuals whose case they handle). Along similar lines, some have urged that managers have "multifiduciary" obligations, fiduciary obligations not just to shareholders but to others—customers, suppliers, and so forth (see the discussion in Goodpaster 1991, 53–74).

Precisely because equivalent disputes appear in other professions, I will abstract from them here. The central point, which survives these complications, is that officials' role-moral obligations direct them *relatively* more toward the public, and their ordinary-moral obligations direct them *relatively* more toward client groups; whereas in other professions, role-moral obligations are generally owed to a client, ordinary-moral obligations to the public. In any case, almost all scholars of public administration who deal with these issues—who analyze the official's varied obligations to agency clients, the agency itself, other government bodies, discrete subpublics, the laws, and the Constitution—nevertheless ultimately situate those obligations within, or else derive them from, their own conceptions of a more fundamental fiduciary obligation to the public (see Burke 1986, 42, 216; Cobb and Elder 1972, 108; Cooper 1990, 84, 107, 111; Denhardt 1988, 78–80; Dewey [1927] 1981–91, 245–6, 255–6; Gortner 1991, 39; Rohr 1989).

What do these distinctions between the fiduciary concept as applied to government and as applied to the other professions imply for the normative assessment of private gain from public office? To answer, we need to explore some further elaborations, the first of which shapes the fiduciary concept as it is deployed by opponents of private gain from public office. Specifically, any particular client or other group to whom an official need show only ordinary-moral regard (a welfare recipient, an agency contractor, a broadcast licensee) also comprises part of the public, that is, part of the very principal to whom the official owes role-moral obligations. Discourse over recent federal procurement reforms shows what this can mean. Companies selling to government expect agencies to observe not only ordinary-moral obligations of private contractual fairness in dealing with them, of the sort that any customer ought accord a supplier, but also fiduciary or role-moral obligations of due process in dealing with them, of the sort the state accords to all citizens. So, for example, in objecting to the proposed injection of market-oriented reforms into the public procurement system, some government contractors refuse to view government as an arm's-length customer, obligated to accord them only their ordinary-moral due while looking after, first and foremost, the interests of its principal, the public. Rather, as constituents of the public, contractors regard government officials as being *fiduciarily* obligated—obligated in role—to take heed, in dealing with them, of their own business interests: their interests in profits, employment, market share, and firm survival.[7]

When officials, acting in role, take insufficient heed of their agency-clients' interests, those clients (regardless of how the public is otherwise being served) may well complain that they are being shown insufficient fiduciary regard. Similarly, when officials acting out of role gain from their public office, it is those whose interests are harmed, namely, their private-sector competitors, who complain that the officials involved are violating their fiduciary commitments. It is not, in other words, the public at large or those speaking in *its* name—that is, it is not the principal itself in toto—that claims to have been harmed by private gain from public office. If officials use skills or stature derived from office to aid them, out of role, in supplying various private-market goods or services, it is competing suppliers who will protest (House Committee on the Judiciary 1992). If former officials use skills or stature derived from office to aid them in devising or implementing corporate strategy for Ford or General Motors, Chrysler may object that its tax dollars have gone to subsidize or underwrite training and intellectual capital made available to the competition.[8] Hence, while the public as a whole *may* claim to have been wronged by private gain from public office, it is particular parts of it that actually *do* claim to have been wronged. But theirs is not an ordinary-moral complaint about overly aggressive competition, one they could launch equally against any private-market competitor, one rooted in a sense of unfairness. Rather, their complaint is that a fiduciary, role-moral obligation has been violated, one they could launch only against an official or former official, one rooted in a sense of betrayal. They react not as agency clients, nor as mere business adversaries whose interests are harmed by the official's competitive behavior, but as taxpayers and citizens—principals—whose trust the official has breached.[9]

---

[7] I am grateful to Steven Kelman for his helpful comments on this point (see also House Committee on Governmental Reform 1995, 11). It is useful to contrast all this with the reverse situation of the business manager. In government, as just noted, every client to whom the official owes ordinary-moral obligations is also part of the public to whom the official owes role-moral obligations. But in business, every shareholder to whom the manager owes role-moral obligations is also a member of the public to whom the manager owes ordinary-moral obligations. Thus, the manager may have a role-moral obligation to a particular shareholder to maximize his or her financial return and at the same time an ordinary-moral obligation not to degrade his or her environment.

So it is that normative criticisms of private gain from public office almost always take the form of accusations of unfair competition. The public-as-principal, as a whole, cannot launch such a complaint; only individual members of the public can sensibly do so. McElwain and Vorenberg (1952) perhaps came closest to articulating the chief grievance raised in cases of private gain from public office when they reframed and sharpened, for the government context, the dichotomy Weinrib discerned within the professional's fiduciary obligation. "The conflict-of-interest statutes reflect two essential principles," McElwain and Vorenberg wrote (1952, 955), "impartiality in government service" and that "no government employee is entitled to an advantage over private persons in the conduct of business . . . by virtue of their position." (See also U.S. Office of Government Ethics 1989, 485–6.) It is not profiting by their position per se—that is too broad—but gaining advantage over private persons by virtue of their position which constitutes the harm done.

Discourse suggests that this is the nub of most normative complaints concerning private gain from public office. But how should we evaluate this kind of claim? Or, put another way, what fiduciary understandings lie behind the contrary case, often made in discourse, that private gain from public office represents *no* normative wrong?

Consider two further ways of subdividing the four professions of law, medicine, business, and government. First, in law and medicine, as distinct from business and government, the professional does not or at least need not in the regular conduct of practice face conflicts of interest between different principals to whom s/he is role-obligated. Of course, lawyers often encounter circumstances in which the interests of different clients *could* come into conflict; but such interprincipal legal conflicts of interest are straightforwardly prohibited and are amenable to a variety of remedies. It is even less likely, because of the nonadversarial nature of the practice of medicine, that an individual physician will be placed in a similar situation, that is, in which the interests of different patients conflict. Certainly, scarce medical resources may increasingly bring the interests of different patients into conflict, but the extent to which the capacity to reconcile such conflicts ought to rest with society or with the physician (who generally does not seek that responsibility) is intensely controversial and only now beginning to be debated.[10]

Conflicts between the interests of different principals are, by contrast, far more integral to and accepted in the regular practice of business and government. Shareholders come into conflict over a range of issues concerning which managers are role-obligated to promote equally interests belonging to each of them; citizen's likewise with respect to officials. This is why majorities and minorities exist among shareholders and citizens but not medical patients and legal clients. The fact that in business and government the interests of different principals may conflict means, often, that there is no univocal course of fiduciary action. Such conflicts between principals allow both the manager and the official a particular kind of fiduciary autonomy—"business judgment" and "official discretion"—that does not exist in law and medicine (Applbaum 1992, Manning 1984).

Now, reconfigure the four professions, and consider a way in which government resembles medicine while remaining distinct from both law and business. In the latter two, as compared with government and medicine, the fiduciary is less likely to have to deal with intraprincipal conflicts of interest. This is because in both law and business the practitioner is fiduciarily obligated to serve only one particular interest of any individual principal: pecuniary wealth in the case of the shareholder, legal victory for the client. This is not the case to nearly the same extent in government or medicine, where the diverse interests of the same principal may come into internal conflict within the domain in which the official or physician is fiduciarily responsible. The possibility that officials' roles may require them to reconcile the manifold different interests that any individual member of the public-principal harbors—in economic growth and consumer safety, for example—is well known. Physicians, too, derive much of their (increasingly controversial) autonomy not from interpatient but intrapatient conflict; for example, a patient's interest in being told the truth conflicts with the patient's interest in retaining peace of mind, or a patient's interest in being cured or surviving conflicts with his or her interest in avoiding suffering, disfigurement, or risky treatment. The fiduciary responsibilities of government and medicine thus extend *relatively* more (if not in entirety) to the total individual and, hence, not just to one but to a range of interests a particular principal may have. As do physicians, officials must participate in—and in practice they often dominate—the reconciliation of the internally conflicting interests of their principals, which is why paternalism is a charge levelled more often against officials and physicians than against lawyers and managers.[11]

---

[8] Of course, if former officials also lobby their former colleagues, some form of impaired official performance also may be at issue, but in this case it is the impaired performance of their former colleagues, not of the privately gaining former officials themselves.

[9] Even when groups claiming to speak for the public as a whole—such as Common Cause—criticize officials for gaining privately from public office, their objection has to do with the consequent disadvantage at which particular, identifiable elements of the public are being placed (see House Committee on the Judiciary 1992, 102–7).

[10] It can be argued that while physicians practicing in managed settings owe fiduciary obligations to their patients as physicians, they also owe fiduciary obligations to their business superiors—their shareholders—not as physicians but in the way any hired manager may. But so described, the situation is a conflict between their professional roles as physicians and managers, not between principals to whom they owe fiduciary obligations as physicians.

The sum and substance of all this is that because in government the principal comprises the entire public—both a large class of persons and many of their interests—the official's role-moral responsibilities are exceptionally complex and diluted. The interests for which the official bears *fiduciary* responsibility come into conflict both between individual principals and within individual principals. The former is not true in law and medicine; the latter is not true in law and business. And this particular understanding of official fiduciary obligation emerges in sharpest relief only in comparative professional context.

What does such an understanding imply for the normative evaluation of private gain from public office? If the principal is so multifarious and riven, the official's fiduciary obligations are by no means univocal. True, there is a sense in which some interests of some part of the principal, some part of the public, may find themselves harmed by any given instance of private gain from public office. But there is also a sense in which another part of the principal may be helped, and indeed those harmed in respect of some of their interests may be helped in respect of others, when officials gain privately from public office; hence, the government's Army recruitment advertisements. Indeed, officials themselves are citizens (for the "bureaucrat as citizen" argument, see Cooper 1990, 40; Goodsell 1994, chapter 5), and their gains and losses, arguably, also ought to enter any calculus involved in weighing instances of private gain from public office.[12]

A fiduciary understanding of public office thus can support both the condemnation and the exoneration of private gain from public office. In many situations, however, it may lend relatively greater support to the one as opposed to the other. Consider an official who, perhaps while in office but more likely after leaving it, writes a memoir of his official experience and then sells it. Most observers would neither prohibit nor condemn such activity. The question (assuming the memoir discloses no commercially confidential or officially classified information) is why? Why are official memoirs not generally criticized for representing private gain from public office (Thompson 1995, 21, 51)? It is true that memoirs *may* assume a form far more personal than official, in the sense that officials in-role experiences may be filtered heavily through their personal subjectivity—their own particular perspective and worldview—and the work as a whole may partake largely of insights and understandings formed outside not inside the role. A manual on how to deal with the official's agency, or a course on a policy issue related to the employee's department, may by contrast seem relatively less personal or out of role, and more official or in role, in origin. Even if true, however, this is a matter of degree. And in fact it is usually not true. In most cases it is precisely the official's role-derived experiences or stature that constitute a memoir's selling point. The official—Henry Kissinger, to take an obvious example—would not be a best-seller had he not occupied his role.

Perhaps the more compelling reason memoirs seem relatively unobjectionable on pure private-gain-from-public-office grounds is this: Memoirs *are* "personal," but not in the sense that they are not subjective, not in the sense that the capacity to produce and sell them results more from the out-of-role person than the office. Rather, memoirs are personal in the sense that they are singular, so that no part of the public-principal is harmed by their production, no matter how much their success owes to the official's public experiences, knowledge, and stature. It is hard to imagine who among the public was competitively disadvantaged, hence fiduciarily betrayed, by Henry Kissinger's publication of his memoirs in the way in which a rival handbook publishing house might be, were officials to produce a manual on how to deal with their agency. An analogous part of the public, a part of the principal that has been harmed, would seem not to exist in the case of memoirists.

By contrast, in criticizing Speaker Newt Gingrich's book deal on the grounds that he was "cashing in from public office," William Safire urged that "public officials should not compete with private citizens as providers of media content" (1994, A23). In Safire's view the media content Gingrich offered was sufficiently generic that it embraced the possibility of competing private suppliers. The advantages Gingrich brought as Speaker in conceiving and promoting his book on American civilization gave him a competitive advantage over other private citizens whose tax dollars and civic exertions enabled the Speaker's effort but who also might want to express remuneratively their own views on U.S. politics. But whereas the Speaker may be offering a generic product others also may provide, the

---

[11] Because managers' fiduciary responsibilities extend only to the shareholder's interest in the corporation, and not otherwise to the entire range of additional interests a shareholder may harbor, managers commit fiduciary violations if they go into competition with their former company but not if they compete with other business interests shareholders may harbor. By contrast, we do not generally condemn officials who go into private competition with a service only their former agency provides, which testifies to our tendency not to regard the government itself as the official's principal. But officials risk criticism whenever, drawing on skills or stature derived in government, they enter into competition with any business interest an individual citizen harbors.

[12] Again, the experiences of managers offer a contrast. Because managers' fiduciary responsibilities do not extend to the entire public, courts hold senior executives who compete with their (former) firm to have violated their fiduciary responsibilities no matter how much such competition may benefit society as a whole, and no matter how compelling their own personal interests may be—much to the frustration of some corporate-law scholars (Chew 1989). Yet, corporate-law jurisprudence is much less strict than government-ethics regulation when we move from profiting from position to conflict of interest. Corporate directors, with certain safeguards, are allowed to self-deal in ways that would be prohibited for government officials (Brudney and Clark 1981, 1028).

memoirist is not.[13] The market for at least certain kinds of memoirs is segmented precisely because the product is so personal, and no part of the official's principal is harmed.[14]

CONCLUSION

Much contemporary discourse surrounding official conduct concerns the use of public office for private gain, a phenomenon that exists separate and apart from quid pro quo. Quid pro quo, however broadly understood, arises whenever (1) individuals or groups whose interests an official has the capacity to affect in role (2) transfer value to the official for which s/he provides no compensating nonofficial acts in consideration and so becomes beholden. Pure private gain from public office, by contrast, takes place in a realm beyond even the twilight zone of quid pro quo, where the official is neither capable of affecting the interests concerned nor beholden to them, and where the official's in-role judgment is thus in no way compromised. Though private gain from public office comes within the colloquial embrace of conflict-of-interest problems, it in fact involves no conflict of interest.

Certainly, contemporary public debate is rich in the ambiguous, paradoxical discourse that surrounds private gain from public office per se, separate and apart from these other problems. Private gain from public office, where it exists, must abide in a realm sufficiently unofficial for the act in question to comprise genuinely private gain—not a part of the official's public responsibilities—but it also must be sufficiently proximate to official terrain for it to redound to public office, not the official's personal qualities or attributes. Yet, as I have suggested through an analysis of some representative strands of discourse, what stands to establish the one claim could undermine the other, an internal tension that fundamentally afflicts the argumentation of both agency and official alike.

If the conceptual issues were not sufficiently multivalent, then note that the normative issues are equally or more so. In analyzing discourse over the nature of the wrong done in instances of private gain from public office, I have tried to frame the debate in conceptions of fiduciary responsibility emergent in or presumed by argumentation on either side. It is, after all, the fiduciary concept that bottoms the concern with profiting from position in other professional contexts. But specifically—and as distinct from these other professional-ethics contexts—when the fiduciary concept is applied to government it is the public as a whole, not the immediate client, that is more aptly thought of as the principal, the entity to which the official owes his or her role-moral or fiduciary obligations. Hence, for officials (as distinct from lawyers, doctors, and managers), both interprincipal *and* intraprincipal interest conflicts unavoidably and pervasively complicate the exercise of fiduciary responsibilities. When the fiduciary concept is applied to government, when it is drawn out of discourse over private gain from public office, it thus shows itself (by comparative professional standards) to be singularly indirect and multivalent in the normative judgments it prompts. Or, more exactly, with any given case of private gain from public office, the interests of the principal—the public itself—may be more or less harmed, more or less helped, depending on the circumstances. What this means is that private gain from public office resolves itself into, or becomes equivalent with, a complaint of unfair advantage, and it is generally only those who are harmed who claim to be betrayed by the official. Not surprisingly with a debate of such longstanding, the deep structural assumptions, once disclosed, do not definitively privilege one side over the other. They simply serve to suggest which kinds of cases are more and which are less likely to raise normative concerns.

On a broader theoretical plane, what democratic discourse over private gain from public office suggests is this: Concepts such as the private-public distinction and the nature of fiduciary responsibilities in government are considerably more complex than—and hence pose a raft of new questions in the realm of conceptual analysis beyond—what their purely theoretical exposition alone would imply. Discourse here richly complicates theory, and thereby suggests further fruitful realms of inquiry, even as theory, here as elsewhere, shows itself to command a considerable capacity for illuminating discourse.

REFERENCES

Applbaum, Arthur Isak. 1992. "Democratic Legitimacy and Official Discretion." *Philosophy and Public Affairs* 21 (Summer):240–74.

Association of the Bar of the City of New York. 1960. *Conflict of Interest and the Federal Service*. Cambridge, MA: Harvard University Press.

---

[13] One may query whether, in Kissinger's case, those seeking to write about diplomacy more broadly would have had to compete with him. I have been unable to locate criticism launched against Kissinger's memoirs of the sort Gingrich experienced. But critics *have* attacked Kissinger's consulting business and his occasional column-writing, both of which deal more broadly with foreign and diplomatic matters, on the grounds that in carrying them out he gains a competitive advantage from having held public office (see the discussion in Nussbaum 1985).

[14] The Gingrich case also involved both of the main elements of quid pro quo, although not contemporaneously. On the one hand, when HarperCollins initially offered Gingrich an above-market $4.5 million advance, it seemed to some as if official, not simply private-authorial, services were being bought. That is, it appeared as if Gingrich would likely be beholden to HarperCollins and its owner, Rupert Murdoch, even though Gingrich's official capacity to affect Murdoch's interests in any "direct and substantial" way had not yet been made evident (the deal was conceived in August 1994, well before Gingrich became Speaker). On the other hand, by the time it was revealed that Murdoch did have a special and keenly advanced set of legislative interests Gingrich could affect as Speaker, and that Gingrich had in fact met with Murdoch, the $4.5 million advance had been replaced by a regular publishing contract, whereby Gingrich would be paid only on the basis of sales, that is, on the basis of private consideration delivered. Not surprisingly, Gingrich used this latter fact to deny that he was in any way beholden to HarperCollins. (Safire, upset only at the private gain from public office element, termed any conflict of interest "remote.") But regardless of the state of quid pro quo involved at any one time, persistent throughout the affair was the complaint that Gingrich was "using his high position to generate book sales" or "exploiting [his] office for personal gain" (House Ethics Committee 1995, Rich 1995, Seelye 1995).

Baker, Russell. 1995. "And Only a Pittance, Too." *New York Times*, January 7, sec. A.

Benda, Peter M., and David H. Rosenbloom. 1992. "The Hatch Act and Contemporary Public Service." In *Agenda for Excellence: Public Service in America*, ed. Patricia W. Ingraham and Donald F. Kettl. Chatham, NJ: Chatham House.

Brauer, Carl. 1987. "Tenure, Turnover and Postgovernment Employment Trends of Government Employees." In *The In-and-Outers: Presidential Appointees and Transient Government in Washington*, ed. G. Calvin Mackenzie. Baltimore, MD: Johns Hopkins University Press.

Brudney, Victor, and Robert Charles Clark. 1981. "A New Look at Corporate Opportunities." *Harvard Law Review* 94 (March):997–1062.

Burke, John P. 1986. *Bureaucratic Responsibility*. Baltimore, MD: Johns Hopkins University Press.

Carson, Thomas. 1994. "Conflicts of Interest." *Journal of Business Ethics* 13 (May):387–404.

Chew, Pat K. 1989. "Competing Interests in the Corporate Opportunity Doctrine." *North Carolina Law Review* 67 (Spring):435–502.

Clark, Kathleen. 1996. "Do We Have Enough Ethics in Government Yet?: An Answer from Fiduciary Theory." *University of Illinois Law Review* 1996 (1):57–102.

Cobb, Roger W., and Charles D. Elder. 1972. *Participation in American Politics: The Dynamics of Agenda-Building*. Boston: Allyn and Bacon.

Cooper, Terry L. 1990. *The Responsible Administrator: An Approach to Ethics for the Administrative Role*. San Francisco: Jossey-Bass.

Denhardt, Kathryn G. 1988. *The Ethics of Public Service: Resolving Moral Dilemmas in Public Organizations*. New York: Greenwood.

Devroy, Ann, and Charles R. Babcock. 1991. "Sununu Travel Rules Tightened." *Washington Post*, June 22, sec. A.

Dewey, John. [1927] 1981–91. *The Public and Its Problems*. In *The Later Works of John Dewey, 1925–1953*. 17 vols. Carbondale: Southern Illinois University Press.

Dobel, J. Patrick. 1990. "Integrity in the Public Service." *Public Administration Review* 50 (May/June):354–66.

Frederickson, H. George. 1991. "Toward a Theory of the Public for Public Administration." *Administration and Society* 22 (February): 395–417.

Goldman, Alan H. 1980. *The Moral Foundations of Professional Ethics*. Totowa, NJ: Rowman and Littlefield.

Goodpaster, Kenneth E. 1991. "Business Ethics and Stakeholder Analysis." *Business Ethics Quarterly* 1 (January):53–74.

Goodsell, Charles T. 1994. *The Case for Bureaucracy*. 3d ed. Chatham, NJ: Chatham House.

Gortner, Harold F. 1991. "How Public Managers View Their Environment: Balancing Organizational Demands, Political Realities, and Personal Values." In *Ethical Frontiers in Public Management: Seeking New Strategies for Resolving Ethical Dilemmas*, ed. James S. Bowman. San Francisco: Jossey Bass. Pp. 34–63.

Gray, C. Boyden. 1991. "Memorandum for Governor Sununu, Chief of Staff to the President, *Results of Our Review of Your Travel on Military Aircraft*," from the Counsel to the President, May 9.

Henderson, Nell. 1992. "Kelly Says She Regrets Taking Fee." *Washington Post*, June 27, sec. B.

Hirschman, Albert O. 1991. *The Rhetoric of Reaction: Perversity, Futility, Jeopardy*. Cambridge, MA: Harvard University Press.

Hook, Sidney. 1980. *Philosophy and Public Policy*. Carbondale: Southern Illinois University Press.

House Committee on Governmental Reform and Oversight, Subcommittee on Management. 1995. *Simplifying and Streamlining the Federal Procurement Process*. February 28. Washington, DC: U.S. Government Printing Office.

House Committee on Standards of Official Conduct. 1992. *House Ethics Manual*. Washington, DC: U.S. Government Printing Office.

House Committee on the Judiciary, Subcommittee on Antitrust. 1960. *Hearings, Conflict of Interest*. February 26. Washington, DC: U.S. Government Printing Office.

House Committee on the Judiciary, Subcommittee on Administrative Law and Governmental Relations. 1992. *Office of Government Ethics Regulations on Private Travel Payments*. Washington, DC: U.S. Government Printing Office.

House Ethics Committee. 1995. *Letter to Speaker Newt Gingrich*. December 7.

Jehl, Douglas. 1993. "Job Plans of Clinton Aides Renew Debate on Lobbying." *New York Times*, December 7, sec. A.

Johnson, Haynes. 1986. "Private Gain, Public Trust." *Washington Post*, May 14. Sec A.

Kass, Henry D. 1990. "Stewardship as a Fundamental Element in Images of Public Administration." In *Images and Identities in Public Administration*, ed. Henry D. Kass and Bayard L. Catron. Newbury Park, CA: Sage.

Kelman, Steven. 1993. "What Is Wrong with the Revolving Door?" In *Public Management: The State of the Art*, ed. Barry Bozeman. San Francisco: Jossey-Bass.

Manning, Bayless. 1984. "The Business Judgment Rule in Overview." *Ohio State Law Journal* 45 (3):615–27.

McElwain, Edwin, and James Vorenberg. 1952. "The Federal Conflict-of-Interest Statutes." *Harvard Law Review* 65 (April):955–83.

Nolan, Beth. 1992. "Public Interest, Private Income: Conflicts and Control Limits on Outside Income of Governmental Officials." *Northwestern University Law Review* 87 (Fall):57–147.

Nussbaum, Bruce. 1985. "The Big Business of Being Henry Kissinger." *Business Week*, December 2. Pp. 76–7.

Pierce, Richard J., Jr. 1989. "The Role of the Judiciary in Implementing an Agency Theory of Government." *New York University Law Review* 64 (December):1226–85.

Potts, Stephen D. 1995. "Memorandum to Designated Agency Ethics Officials from the Director of the U.S. Office of Government Ethics." June 26.

Rich, Frank. 1995. "Newt's Big Buck." *New York Times*, December 23, sec. A.

Roberts, Robert N. 1988. *White House Ethics: A History of the Politics of Conflict of Interest Regulation*. New York: Greenwood.

Rohr, John A. 1989. *Ethics for Bureaucrats: An Essay on Law and Values*. New York: Dekker.

Rose-Ackerman, Susan. 1978. *Corruption: A Study of Political Economy*. New York: Academic Press.

Safire, William. 1985. "Dodging the Graft." *New York Times*, September 28, sec. A.

Safire, William. 1994. "Royalties in Office." *New York Times*, December 23, sec. A.

Sanjour v. *EPA*. 1992a. 86 F. Supp. 1033 (D.D.C., 1992).

Sanjour v. *EPA*. 1992b. 997 F. 2d 1584 (D.C. Cir. 1992).

Sanjour v. *EPA*. 1995. United States Court of Appeals for the District of Columbia Circuit. No. 92-5123.

Seelye, Katharine Q. 1995. "Murdoch, Joined by Lobbyist, Talked of Regulatory Problem at Meeting with Gingrich." *New York Times*, January 15, sec. A.

Smith, Rogers M. 1988. "Political Jurisprudence, the 'New Institutionalism,' and the Future of Public Law." *American Political Science Review* 82 (March):89–108.

Stark, Andrew. 1995. "The Appearance of Official Impropriety and the Concept of Political Crime." *Ethics* 106 (January):326–51.

Thompson, Dennis F. 1995. *Ethics in Congress: From Individual to Institutional Corruption*. Washington, DC: Brookings.

U.S. Office of Government Ethics. 1989. *Informal Advisory Letters and Memoranda and Formal Opinions of the U.S. Office of Government Ethics (1979–1988)*. Washington, DC: U.S. Government Printing Office.

U.S. Office of Government Ethics. 1990. *Informal Advisory Letters and Memoranda and Formal Opinions of the U.S. Office of Government Ethics (1990)*.

United States v. *National Treasury Employees Union* 1995. 115 S. Ct. 1003.

Vaughn, Robert G. 1990. "Ethics in Government and the Vision of Public Service." *George Washington Law Review* 58 (February): 417–50.

Walzer, Michael. 1972. "Political Action: The Problem of Dirty Hands." *Philosophy and Public Affairs* 2 (Winter):160–80.

Walzer, Michael. 1987. *Interpetation and Social Criticism*. Cambridge, MA: Harvard University Press.

Weinrib, Ernest J. 1975. "The Fiduciary Obligation." *University of Toronto Law Journal* 25 (Winter):1–25.

Williston, Samuel. 1937. *A Treatise on the Law of Contracts*, rev. ed. New York: Baker.

Wines, Michael. 1991. "Chief of Staff's Air Travel Practices and Questions about Their Propriety." *New York Times*, June 24, sec. A.

American Political Science Review Vol. 91, No. 1 March 1997

Legal Theory in the Collapse of Weimar: Contemporary Lessons?

DAVID DYZENHAUS *University of Toronto*

The Weimar Republic is frequently invoked in political theory as an example when the issue is the appropriate response of liberal democracies to internal, fundamental challenges. I explore that example through the lens of a 1932 court case that tested the legality of the federal government's "coup" against Prussia. In my analysis of the court's judgment and of the arguments of three political and public law theorists, Carl Schmitt, Hans Kelsen, and Hermann Heller, I argue for Heller's democratic vision of the rule of law. In my conclusion, I compare problems in Kelsen's position with problems in the recently articulated position of John Rawls in order to suggest what lessons Weimar may have for contemporary political theory.

The collapse of the Weimar Republic (1918–33) haunts political debate these days, as disaffected groups with increasing support again challenge the most fundamental values of liberal democracies. It is with this challenge that contemporary political philosophy grapples when it tries to deal with the "fact of pluralism." Any attempt to contest those conceptions of the good life which go against the grain of liberal democratic values invites the charge that liberalism is just one ideology among others, each seeking to enforce its partial idea of the good on the whole.

In responding to the fact of pluralism, liberal philosophers, most notably John Rawls (1993), have begun to defend the liberal state in a self-consciously political fashion. The liberal state will not concede the space of politics to those who want to use that space to destroy it with its neutral stance among different individual conceptions of the good. But Rawls also alleges that the highly controversial task of drawing the limits of legitimate politics does not rely on any partial liberal concept of the good.

Does the failure of the Weimar Republic hold any lessons for contemporary attempts to demarcate legitimate politics? In answering this question, my focus is on the positions of three important political and legal philosophers of late Weimar.[1] One is the communitarian existentialism of Carl Schmitt. The second is the legal positivism of Hans Kelsen. The third is the social democratic view of Hermann Heller, who argues for an ethical conception of law such that law provides a constraint on political power.

These positions were brought into sharp contrast by the political decisions made by Germany's federal government in 1932. On July 20, Field Marshall von Hindenburg, the *Reichspräsident*, or president, issued a decree "concerning the restoration of public safety and order in the area of the *Land* of Prussia."[2] This decree is a crucial moment in the breakdown of Germany's first experiment with democracy.[3]

The decree was issued under the authority granted the president by the emergency powers section of the Weimar Constitution, Article 48. It declared the chancellor of the Reich, Franz von Papen, to be the commissioner for Prussia, the largest and most powerful of the German *Länder*, and gave him authority to take over its political machinery.[4] It was issued at the behest of Papen's cabinet, and it formed an integral part of the strategy of the minister of defense, General von Schleicher. The decree responded to the alleged inability and unwillingness of Prussia's government—a coalition in which the main socialist party, the Social Democratic Party (SPD), dominated—to deal with the state of political unrest and violence within Prussia.

David Dyzenhaus is Associate Professor of Law and Philosophy, Law Faculty, University of Toronto, 78 Queen's Park, Toronto, M5S 2C5, Canada.

I thank Peter (Carl) Caldwell, Cheryl Misak, Hamish Stewart, Mike Taggart and the anonymous reviewers for their comments. John P. McCormick deserves special thanks for persuading me to write this article in the first place.

[1] Finn (1991, 166–77) discusses some of the issues considered in this article and also provides an account of Schmitt and Kelsen. His account, however, suffers from an almost total reliance on secondary English sources. This results, on the one hand, in a poor understanding of Schmitt and, on the other, in a neglect of Kelsen's writings on the events, together with an overestimation of Kelsen's actual influence on the legal actors.

[2] For the full text, see *Preussen* [1933] 1976, 481. For a detailed discussion of the events and the legal issues, see Grund 1976 and Grimm 1992. My evaluation differs sharply from both of theirs.

[3] Any account of ideas that "tests" them in the laboratory of history runs certain risks when the history is based on secondary sources (see Lustick 1996). The most obvious risk is that one will rely only on those secondary sources which confirm the conclusions one wishes history to support. My own choice relies heavily on those who observed directly the events detailed here and who were committed democrats: Brecht [1944] 1968 and Eyck 1967. The two most important more recent works on the political history of Weimar (Bracher 1984, Mommsen 1996) agree with these observer accounts on the two points crucial to my own argument in the text. They agree that Weimar was destroyed by the exploitation of its structural problems by those opposed to parliamentary democracy rather than by the structural problems. And they agree that right up until Hitler's seizure of power, displays of democratic commitment by important institutional actors, including the courts, could have made a difference to the course of events. One has to accept both of these points before one can start to make a case for the characterization of the events of July 20 as a coup. Both points are controversial in the historical literature on Weimar, though the preponderance of opinion is with Bracher and Mommsen. For an even-handed discussion of the secondary sources, see Kolb 1988, especially chapters 5 and 6.

[4] One grave structural weakness in the federal system was that it perpetuated a prewar problem by leaving Prussia intact as a political entity. For Prussia contained almost two-thirds of the German population and occupied almost two-thirds of German territory. In addition, the seat of federal government continued to be the Prussian capital, Berlin. This created a severe imbalance in intergovernmental relations, which the framers of the Weimar Constitution left to politics to work out.

This coalition was the most important base of institutional resistance to the Nazi march to power, and it was removed at the stroke of a pen.

Schleicher and the others in Papen's cabinet of right-wing aristocrats intended the coup as the first move in a plan to rid politics of the SPD. They would then be able to crush the communists and simultaneously tame Hitler by drawing him within the control of an increasingly authoritarian cabinet. The strategy would be complete once Hitler was neutralized and the cabinet, having eliminated all internal opposition and obstacles (including the *Reichstag*), ruled Germany by decree.

At the time of the coup, the Prussian government considered armed resistance. But both because it seemed that such action would end in defeat and because, as social democrats, they were committed to legality, they chose to challenge the constitutional validity of the decree before the *Staatsgerichtshof*, the court set up by the Weimar Constitution to resolve constitutional disputes between the federal government and the *Länder*.[5] The court effectively upheld the decree in late October, by which time the SPD was no longer an effective force.[6]

The legal and political significance of this case was clear. Some of the most important public law theorists of the day argued before the court, turning the legal argument into a battle of legal philosophies. Among these were Schmitt, by then the foremost legal theorist of the right and Schleicher's chief legal advisor, and Heller, who argued on behalf of the parliamentary delegation of the Prussian socialists.

Kelsen, Dean of the Law Faculty, University of Cologne, did not participate in the proceedings before the court but felt compelled to write a detailed analysis of the judgment before it was officially published (Kelsen 1932–33). For many years he had been a chief target of Schmitt (as well as Heller), and his analysis of the judgment followed hard on the heels of his own scathing polemic against Schmitt's assertion that the president was the true guardian of the Weimar Constitution (Kelsen 1930–31).

Schmitt won the professorial battle in that the court's judgment effectively upheld his position. Nine months later, after Hitler came to power, he joined the Nazi Party and offered it his legal services. Although he could not teach after the war because of his involvement with the Nazis, Schmitt is still influential in Germany, and in some quarters his ideas are fashionable in Europe and North America. In a time when the idea of a constitutional or legal order which can accommodate the claims of a plurality of groups seems increasingly imperilled, Schmitt, who argued against the possibility of such an order, is likely to gain an even larger audience.

In contrast, Kelsen, a Jewish democrat, was forced out of his post at Cologne in 1933.[7] He made his way via Geneva and Prague to the United States, where, despite many hardships, he continued to work and publish until his death in 1973. But his positivist philosophy of law is studied for the most part by a few specialists.[8]

Heller died in 1933 in Madrid. He was only 42 years old and was trying to complete a full statement of his legal philosophy in his *Staatslehre*, or theory of the state.[9] He was an important partipant in debates about public law and legal theory in Germany, especially in late Weimar, but in comparison with Schmitt or Kelsen he receives little attention in Germany and, until quite recently, was almost unknown in the English-speaking world.[10]

In what follows, I shall show how the legal events around the coup provide a fruitful lens for viewing the debate among Schmitt, Kelsen, and Heller. Under the pressure of making practical arguments on a fundamentally important political issue, the three theorists expose the vulnerabilities as well as the strengths of their theories.[11] I shall also argue that the lens refracts onto the present. In particular, it highlights the most problematic aspects of the contemporary liberal response to pluralism, exemplified in the recent work of John Rawls.

THE WEIMAR CONSTITUTION AND THE JULY 20 COUP

By the time of the coup, parliamentary democracy was severely weakened in Germany. The collapse of the grand coalition government in 1929 ushered in an era of increasingly authoritarian rule by presidential decree. This was made possible by the peculiar place of the president in the Constitution. Writing in 1944, Arnold Brecht, one of the leading constitutional and legal figures of Weimar, states that it was perhaps inevitable that a realistic constitution for postwar Germany should provide, as it did, for a president with broad emergency powers under parliamentary control (Brecht [1944] 1968, 143–4).[12] The Weimar Constitution was drafted in a time of civil unrest which was accurately expected to persist beyond the founding of the new republic. The exercise of these powers, given to the president by Article 48, required the countersignature of the cabinet; and the cabinet, while appointed by the president, had to enjoy the confidence of the *Reichstag*. But the president also had the power to dissolve that body, a power limited only by the vague requirement that he could do this "only once on the same ground."[13] The power to dissolve the *Reichstag* combined with the power to appoint the cabinet meant that the president could ensure a cabinet which would give him the requisite countersignature and which did not have the confidence of the *Reichstag* simply because there was for the time being no parliament in existence. Thus, as Brecht ([1944] 1968, 48–9) says, the problem was not so much the broad framing of the emergency powers but that they could potentially be exercised free of control.

---

[5] In terms of Article 19 of the Weimar Constitution, the *Staatsgerichtshof* was given the jurisdiction to decide on constitutional disputes within a *Land*, between *Länder*, and between a *Land* and the central government. The president was charged with executing its judgments.

[6] For the full text of the judgment as well as of the argument before the court, see *Preussen* [1933] 1976.

[7] Schmitt, it should be noted, had joined the faculty earlier in that year, and as a matter of course he had needed and had sought Kelsen's support for such a move. He returned the favor by participating in the machinations that led to Kelsen's dismissal. See Rüthers 1989, 138–9, quoting from the memoirs of Hans Meyer, a student of Kelsen's at that time.

[8] Kelsen's legal philosophy has had an immense indirect effect on Anglo-American legal philosophy, however, through the work of H. L. A. Hart (1961) and Joseph Raz (1983).

[9] Gerhart Niemeyer, Heller's assistant at that time, worked the manuscript into a publishable state and it was issued in 1934 in Holland.

[10] See the excellent discussion of Heller in Scheuerman 1994. See also Niemeyer 1941 and Kennedy 1984. The best German study of Heller remains Schluchter 1983.

[11] It has been suggested to me that such pressures arguably required the protagonists, especially the two who appeared in court, to twist their arguments to suit the results they wanted. Yet, this suggestion underestimates the extent to which, in the legal culture of Weimar Germany, the legal issues at stake in 1932 were viewed as an occasion for decision by different constitutional theories, rather than by the court; see Schlink 1994, 218. In addition, I would argue that practical contexts in any case provide the best lens for an appreciation of the merits of conflicting theories.

Until an election in April 1932, an SPD-dominated coalition could count on majority support in the Prussian parliament. In anticipation of a situation in which the Nazis would be the strongest minority party, the coalition changed the electoral regime by replacing the requirement that a prime minister could be elected by a relative majority if, on the first ballot, the incumbent had been voted out with the requirement that the new prime minister be chosen by an absolute majority. In that election, the Nazis increased their representation in the Prussian parliament to the point at which they and the German Nationalists controlled about 200 votes, in contrast to the 160 of the SPD and the Center Party. Since the Communist Party had 57 votes and would oppose a government dominated by either the Nazis or the SPD, majority support could not be found for a new government. The old coalition government then resigned, but under Prussian electoral law it continued to operate as a caretaker until a new prime minister had been chosen and had named his cabinet.

On June 14, 1932, Schleicher set in motion his intricate plan by which political disorder would provide the excuse for dealing, first, with the left and, then, with the extreme right. The federal government lifted a ban on Hitler's paramilitary organization—the SA—while a similar ban on the communist Red Front remained in force. The decree unbanning the SA was followed by one of June 28 that prohibited *Land* governments from imposing their own bans on the Nazi organizations. In that month, street battles became the order of the day, as the triumphant return of the Nazi paramilitary to the streets brought a furious and bitter response from the communists.[14]

On July 14, Papen secured the emergency decree from Hindenburg that permitted him to usurp the Prussian government's powers. This was on the wholly contrived pretext that the Prussian government, in particular the SPD element, intended to conspire with the communists to act against the Nazis. Papen kept this decree "in his pocket" until the outbreak of violence gave him the excuse he wanted. On July 20, the Prussian government was forcibly removed from office, and Papen became commissioner for Prussia. The Prussian government sought an injunction from the *Staatsgerichtshof* to prevent Papen from taking office, but this was refused on July 25, on the ground that the injunction would anticipate the final decision of the case.

That final decision was given on October 25 and upheld Prussia's right to participate as an independent political entity at the federal level, but it effectively gave the Reich government the free hand it wanted in Prussia's internal affairs. In any case, the time that had lapsed was more than ample for the destruction of Prussia's republican institutions. The Nazis were only months away from power and given to increasing public indications of their contempt for law and order, which gave the lie to Papen's pretext for intervention in Prussia.[15]

In short, even if the court's decision had gone against the cabinet, its direct effect might not have been large. But if the court had declared the coup unconstitutional, then a major crack would have opened in the veneer of legality that Papen and Schleicher sought to paint over their attempts to establish government by decree in Germany. While both were willing to use the authority of the president's office and person to break free of the parliamentary system entrenched in the Constitution, they (and Hindenburg himself) were anxious to be seen to be keeping within the law.[16] In turn, the legality of Hitler's seizure of power would have appeared even more dubious (Bracher 1984, 525).

---

[12] Brecht, a former judge and senior civil servant, was a counsellor in the first quasi-democratic cabinet, which oversaw the transition from monarchy to parliamentary democracy. He held high office in the federal government until 1927 and was deeply involved in the attempt to preserve and strengthen democracy at the federal level until he was dismissed on demand of the right. He was then appointed by the Prussian government as one of its chief delegates to the *Reichsrat*, and he appeared before the court to defend the Constitution in 1932. (The *Reichsrat* was a kind of senate but did not function as a full upper house of parliament.) He was removed from office by the Nazis in May 1933, in his words "literally the last official in the service of democratic institutions," and left Germany in that year to join the faculty of the New School for Social Research in New York; Brecht [1944] 1968, xii–xvi, at xv.

[13] Article 25.

[14] There were 99 killed and more than 1,000 wounded in skirmishes, most dramatically on Sunday, July 17, in Altona, a communist-dominated suburb of Hamburg, where the battle in the streets left 17 dead.

[15] On July 31 in Königsberg, the Nazis murdered political opponents on the right as well as the left. An emergency decree of August 25, which provided the death sentence for acts of political terrorism, was answered by a particularly brutal Nazi murder of a communist. When those directly responsible were sentenced to death, Hitler publicly applauded them and dedicated himself and the Nazis to securing their release. Erich Eyck (1967, 420–1) aptly describes Papen's reaction: "And so the same national cabinet that had established a national commissioner of Prussia, ostensibly to preserve public peace and order, broke down in the face of the Nazis' threats and, on September 2, commuted the . . . assassins sentences to life imprisonment, thus assuring them their freedom as soon as Hitler came to power. Never before had a German government bowed so openly to political terror."

[16] As Brecht ([1944] 1968, 66–7) put it, by "using only the constitutional remedy of an appeal to the court, the Prussian ministers were led by the desire to preserve the constitutional basis of governmental

Was the coup unconstitutional? An account of the judgment rendered by the court prepares the ground for answering this question.

THE JUDGMENT OF THE COURT

The two crucial paragraphs of Article 48 read as follows:

> If a *Land* does not fulfil the duties imposed on it by the Constitution of the Reich or by a law of the Reich, the President can ensure that these duties are performed with the help of armed force.
>
> If the public safety and order of the German Reich is seriously disturbed or endangered, the President may take the measures necessary for the restoration of public safety and order, and may intervene if necessary with the help of armed force. To this end he may temporarily revoke in whole or in part the fundamental rights contained in [a list of basic rights].[17]

The decree authorizing the coup claimed to be based on both paragraphs one and two, but in fact Papen's own justification for the coup, and much of the argument put by his representatives before the court, relied heavily on the first paragraph.[18] Papen's case for action in terms of paragraph one rested on the allegation that the Prussian government was, in collaboration with the communists, bringing about a state of disorder by acting unfairly against the Nazis. Any further case for action under paragraph two had thus also to be based on this allegation. And, as we have seen, Papen and Schleicher had sent the Nazis a message of qualified support by unbanning their paramilitary organizations while leaving the ban on the Red Front in place and then decreeing that *Land* governments could not use their own emergency procedures to ban the SA.

The court held in respect of paragraph one that it had to be shown that the Prussian government was not fulfilling its duty in terms of the Constitution and federal law to deal to the best of its ability with the breakdown in law and order. And the court rejected Papen's allegations in this regard (*Preussen* [1933] 1976, 511–3).

In respect of paragraph two, the court held that it did not have to broach the question of whether it was entitled to determine if a breakdown in public safety existed in order to evaluate whether the president was entitled to act. According to the court, the July 20 decree had been issued at a time of manifest breakdown in public order, indeed, when Germany was on the verge of a civil war that threatened the constitutional order itself. It reasoned that the president was entitled to take the measures he had taken—seize control of not only the Prussian police but also the entire machinery of state in Prussia. And, it commented, even if the Prussian claim were true that the situation had at least in part been brought about by the Reich government, the court's reasoning on this score was not affected (*Preussen* [1933] 1976, 513–4).

This left the issues of whether the president had abused his discretion and overstepped its limits. In regard to abuse, the Prussian authorities had relied on allegations, hotly disputed by the Reich government's representatives, that the intervention had come about as a result of negotiations between the Reich government and the Nazis with a view to deposing the Prussian government. The court considered these allegations unproved. But it held that, even if true, the charges failed to show that the decree was not aimed at restoring public safety and order. At most, the allegations offered some insight into why the Reich government became convinced that an intervention based on paragraph two was necessary. In addition, the court dismissed the Prussian argument that the Reich government's intervention in Prussia alone, when there was a general breakdown of law and order, was sufficient proof of an abuse of discretion (*Preussen* [1933] 1976, 514).

The court then rejected the Prussian contention that the measures taken overstepped the limits of discretion since they included all the Prussian ministers in their scope, not just the prime minister and the minister of the interior, that is, the officials directly responsible for maintaining law and order. The court held that the scope of the discretion was for the president and not the court to determine. As long as the chancellor kept within the limits of that discretion, he was answerable only to the president (*Preussen* [1933] 1976, 514–5).

The court did hold that the decree could not contradict the Constitution, and this left some remnants of the Prussian government in place.[19] But in effect this gave Prussia nothing, which was starkly pointed out by one of the court's concluding observations. The court noted that its decision to leave the Prussian government intact for the very limited purposes it considered constitutionally required was a recipe for friction with the commissioner. But, it said, if the friction was caused by an act of the Prussian government that amounted to a violation of its duty to the Reich, then the president could always rely on paragraph one to bring Prussia into line (*Preussen* [1933] 1976, 516–7).[20]

---

powers and to force Hindenburg back within constitutional bounds." And Hans Mommsen (1996, 452) comments that the "decision blocked the presidential cabinet's road to an open coup d'état."

[17] The basic rights were articles 114, inviolability of personal liberty; 115, inviolability of the home; 117, privacy of mail, telegraph, and telephone; 118, freedom of opinion and press; 123, freedom of assembly; 124, freedom of association; and 153, inviolability of private property.

[18] For the full text of the decree, see *Preussen* [1933] 1976, 481, and for an English translation, Brecht [1944] 1968, 145–6.

[19] In particular, the decree of the federal government could not infringe on Article 17, which provided that each *Land* should have its own constitution and government that enjoyed the confidence of the people's assembly; nor could it infringe on Article 63, which guaranteed its representation in the *Reichsrat*. Thus, the court held the decree invalid only to the extent that it purported to deprive the Prussian government of its right to represent Prussia in the Prussian parliament, in the *Reichsrat*, and in its relations with other *Länder*. It also held that the decree could not infringe on Prussia's right in terms of Article 33, which gave each *Land* the right to be represented in and to speak out in debates before the *Reichstag* and its committees (*Preussen* [1933] 1976, 515–6).

[20] As Kelsen (1932–33, 81) commented, this observation put a Damoclean sword over what remained of the Prussian government.

We have still to deal with the question of whether the decision had merit. There are three possible answers, each given by the respective legal philosophy of Schmitt, Kelsen, and Heller.

SCHMITT'S ARGUMENT BEFORE THE COURT

Schmitt appeared with four others for the Reich.[21] He was soon in the thick of things in argument about the correct characterization of the events prior to and on the day of the coup, for Brecht invoked in Prussia's opening address to the court a passage from a text Schmitt had just published.

A main claim of this text—*Legalität und Legitimität* [Legality and legitimacy]—is that a parliamentary democracy is committed to safeguarding above all the principle of "equal chance." According to this principle, those who happen to hold political power must be committed to keeping open for their opponents the path to power, whatever the political ideology of their opponents. Schmitt finds it absurd that a parliamentary democracy should offer an equal chance to all parties. He asserts in the passage quoted by Brecht that it is suicidal for a parliamentary democracy if the party in power holds to this principle when the opponent is ideologically committed to the destruction of parliamentary democracy (Schmitt [1932] 1988, 37; *Preussen* [1933] 1976, 12). On the face of it, this passage fit snugly into the Prussian argument, for Brecht contended that Prussia had to take all the steps the law allowed to keep the Nazis from power since they were intent on retaining power forever, once they obtained it (*Preussen* [1933] 1976, 11–28).

Schmitt responded, first, that Brecht failed to see that his question was who should decide which party to a conflict should be dealt with as illegal because it is an enemy of the state, the people, or the nation. That decision cannot, he said, be made by one of the parties to the conflict, who would decide in a purely self-interested way. The decision must be made by an independent government (*Preussen* [1933] 1976, 39). Second, at issue was not merely a political conflict that threatened to explode into civil war. Rather, two states confronted each other, one of which was "occupied" by one of the conflicting parties; both demanded the right to make politics and "then politics in the most far-reaching and intensive sense." He denied that he was questioning the claim that a *Land* in general had the right to a political policy which diverged from that of the Reich. Instead, the issue was Prussia's bid to decide the crucial question of politics—who was an enemy of the state and the Constitution (*Preussen* [1933] 1976, 40–1). Schmitt accused the SPD of a "subterfuge" in its bid to preserve parliamentary democracy in Prussia. In his view, Prussia itself, or more accurately the SPD's role in Prussia, was the threat to which the Reich government reacted, not the breakdown or threatened breakdown in public safety and order within Prussia (p. 469).[22]

Thus, Schmitt considers it irrelevant that it was in a bid to preserve democracy in Prussia and in Germany as a whole that the Prussian SPD had engineered a role for a caretaker government in the event the republican parties could not command a parliamentary majority. The issue for Schmitt had nothing to do with the Prussian government's inability to command majority support. As Heller pointed out in argument before the court, the Reich government itself had no mandate from the *Reichstag* (*Preussen* [1933] 1976, 250). For Schmitt, then, the Papen government's mandate—its legitimacy—stemmed from its independence from parliamentary politics. This independence rendered it capable of dealing with its enemies, who included those wishing to save parliamentary democracy as well as the Communist Party. Indeed, for Schmitt it is political parties in general that pose a threat to state sovereignty. A decision to intervene to preserve parliamentary democracy would perpetuate the struggle between political parties that, according to Schmitt, "poisons" Germany (p. 39). A government that enjoys a parliamentary mandate is part of the problem since such a government is under the control of one or more political parties.[23]

Schmitt went on to claim that the Constitution was silent as to whether a *Land* was guilty of failing to fulfill its constitutional responsibilities when it adopted a different policy from the federal government. The question was one of constitutional interpretation that had to be settled by reference to the meaning of the Constitution as a whole and to its historical origins. Schmitt, however, in turning here to an idea of the totality of the Constitution, is not advocating an approach to constitutional interpretation which seeks to achieve a coherent understanding of the whole document. For, as we shall now see, what Schmitt means by totality is not at all the relevant sections of the Constitution, but those elements that provide a basis for sovereign decision.

Schmitt argued that paragraphs one and two of Article 48 had to be understood as part of one structure devised by the framers of the Constitution to protect the unity of the Reich. That meant protecting against the threat of civil war when political parties felt no obligation of loyalty to the internal policies of the

---

[21] Six parties claimed standing as applicants to the court: Prussia, represented by the ministers who had been removed from office; the ministers themselves; the Prussian SPD; the Prussian Center Party; the *Land* Bavaria; and the *Land* Baden. Both of these *Länder* had caretaker governments and so shared an interest with Prussia in the outcome of the case. I will avoid altogether the issue of their argument as to their standing before the court and the court's response.

[22] For strikingly ambiguous claims in this regard, see Schmitt in *Preussen* [1933] 1976, 288–91, at 290–1; compare Heller's response at pp. 292–4. Schmitt also indignantly rejected the "insulting equation" of the Nazis with the Communist Party (and thus the Prussian attempt to discredit Papen). He maintained that when the Reich government had decided to change its practice regarding the Nazis, this was simply in "objective and rightful" recognition of the simple fact that millions of Germans supported the movement (p. 39).

[23] As Schmitt put things in the concluding paragraph of his essay published on behalf of the Reich government, the issue was not one of Reich and state against Prussia, but of Reich and state against party and political faction (Schmitt 1932, 958).

Reich government. He asserted that, in such a situation, a *Land* was obligated to follow government policies and could be compelled to do so by the government acting in terms of paragraph one, while the Reich government simultaneously took the steps it deemed fit under paragraph two to restore law and order (*Preussen* [1933] 1976, 130–5, 175–81).

In his concluding remarks, Schmitt did not shy away from the radical implications of his argument. He addressed the procedural presuppositions of the case and in effect questioned the value of legal process itself. First, Prussia's standing to argue before the court was at issue since the decree purported to replace the Prussian government with the commissioner. Second, he dealt with the jurisdiction of the court to pronounce on the validity of the decree itself as well as the action taken in its terms. These were, Schmitt maintained, not only legal but also deeply political matters. In regard to standing, he argued that while Prussia had standing, the Prussian government had none, since it had been constitutionally replaced by the commissioner. Of course, this crudely begs the question of constitutionality, but Schmitt's argument is that the court cannot pronounce on that issue since it is a political one.

Schmitt did not contest the claim that the court was the guardian of the Constitution. Rather, this role of the court was confined by its character as a court of law, and thus it was guardian only insofar as an issue was appropriate for a legal and judicial body. Since the issues in this case were deeply political, and the president was the guardian of the Constitution in matters political, the question of constitutionality was for him to decide. For Schmitt, this role for the president was made clear by the powers given him in Article 48 to decide the crucial questions of politics (*Preussen* [1933] 1976, 466–9).

Although Schmitt believed that political power transcends all legal constraints, there is what we might think of as a quasi-normative constraint on power. Schmitt's academic writings argue that the political sovereign is not simply he who commands the resources of power (Schmitt [1932] 1976, [1922] 1988). Such command is a necessary but not sufficient condition for an exercise of sovereign power, for Schmitt holds that, in addition, the sovereign decision has to make the distinction between friend and enemy.

Now we can fully appreciate Schmitt's response to the use of his work by Brecht to buttress Prussia's argument. Schmitt is not claiming in the passage quoted by Brecht that a parliamentary democracy must make the friend/enemy distinction if it is to save itself. Rather, his claim is that a parliamentary democracy is potentially suicidal since it cannot make the distinction it needs to protect itself.

The point of Schmitt's distinction between friend and enemy is the "substantive homogeneity" of the German people. More accurately, the decision as to who is friend and who is enemy is an existential one in the sense that it makes substantive homogeneity possible. It simultaneously brings "the people" into existence and ensures their preservation. Once substantive homogeneity is in place, the individuals who compose the group have a worth that far transcends anything a liberal democratic society can offer ([1932] 1976, 47–53, [1928] 1989, 209–10).

Thus, the quasi-normative constraint that makes a decision a genuine political decision is its friend/enemy distinction. Ruled out is an attempt to perpetuate parliamentary democracy, for such an attempt adopts what Schmitt considers to be the pretense of liberal legalism. That is, it pretends that the distinction between friend and enemy has no place in its politics. It then contradicts itself by permitting to flourish interest groups which are dedicated to the kind of ideology that seeks to eradicate the idea of substantive homogeneity altogether (Schmitt [1932] 1976, 69–79).

The major weakness of Schmitt's legal theory is, I submit, the difficulty of accepting that it is an account of constitutionality and legal order. As Heller pointed out in his argument before the court, the case Schmitt argued on behalf of the Reich seemed to lead inexorably to the conclusion that there was no more to the Weimar Constitution than the emergency powers contained in Article 48 (*Preussen* [1933] 1976, 248–9).[24] To come to this conclusion, to hold that Article 48 is to be interpreted in a normative vacuum, is to hold that it merely provides the occasion for an exercise of power. The idea of emergency powers is then made vacuous, since it presupposes that the powers must be used to restore a state of ex ante legality whose norms continue during a state of emergency to limit executive action. Indeed, the very idea of a state of emergency is made vacuous, since the decision that a state exists is put in the exclusive control of the executive.

The conclusion clarifies an ambiguity in Schmitt's work in the 1920s on emergency powers and on constitutional theory (Schmitt [1928] 1989, [1922] 1989). In those writings he often seems ultimately undecided as to whether law can ever effectively constrain political power. But the conclusion that a genuine political decision is not subject to legal constraints follows directly from the most famous sentence of his corpus, penned in the early 1920s: "Sovereign is he who decides on the exception" (Schmitt [1922] 1988, 5).

This ambiguity reflects, I suggest, the attitude of many Germans to Weimar. But it did not exist merely in the heads of those doubtful about or hostile to the idea of German democracy; it was embedded in the structure of the Weimar Constitution. That fact was highly significant for those who were dubious about or hostile to Weimar. In their eyes, the loss of the kind of legitimacy that disappeared with the monarchy meant the introduction of a permanent state of emergency, since they either doubted or outright rejected the thought that there could be any other kind of legitimacy. In other words, the fact of constitutional ambiguity maintained a permanent opportunity to restore the legitimate regime, as they understood it. But, in the absence of an hereditary monarchy, legitimacy is existentially created and then authenticated through the acclaim—the "Ja"—of the people (Schmitt [1928] 1989, 81–91). Hence, Schmitt's account of politics and law is ultimately committed to welcoming the advent to power of any force that will make the friend/enemy distinction in order to establish the substantive homogeneity of the German people.

---

[24] Heller's comment was directed primarily at another of the Reich's legal representatives, Professor Jacobi, whose argument went even farther, according to Heller, than anything Schmitt had argued. But Heller clearly meant to include Schmitt's argument within the scope of his comment.

The argument Schmitt presented to the court was thus to ask it to rule that it had no role to play in deciding on substantive legal constraints on the federal government, that it had no authority to make any ruling save that it had no authority. The court did not accept this role. As we know, it upheld Prussian claims to its status at the federal level, it asserted its right to scrutinize the legality of executive claims to authorized action under Article 48, and it rejected the federal government's claim to have authority in terms of Article 48.1. But in rejecting any authority to scrutinize the substantive merits of the federal government's case in terms of Article 48.2, the court made an almost empty shell of the legal constraints it did uphold, which gave the federal government most of what it wanted.

In effect, the court seemed to vindicate Schmitt's claim that the limits set by legalism are in substance no limits at all. And I will now show that his claim is supported to the extent that Kelsen seems unable to argue the court should have done better.

LIBERAL LEGALISM

Early in the era of presidential rule by decree, Kelsen saw where Schmitt's line of reasoning would end. In a 1930 essay, Kelsen argued that constitutional review of executive action by judges is merely the most logical way of ensuring that government action stays within the law. Such review ensures conformity to what he calls the principle of legality. Schmitt's claim that the president is the sole guardian of the constitution, unconstrained by law, would culminate, Kelsen predicts, in an "apotheosis" of Article 48; parliamentarianism would be destroyed by a mystical force that regards parliamentary democracy itself as the source of all threats to public safety and order (Kelsen 1930–31, 622–3). And as Kelsen later argued, the court's judgment on the constitutionality of the coup brought about that apotheosis (Kelsen 1932–33).

Kelsen stated that the court was right to uphold its authority to inquire into the presuppositions of Article 48.1 and 48.2. This amounts merely to the court assuming its unrestricted jurisdiction in such matters, a jurisdiction granted by Article 19 of the Constitution (Kelsen 1932–33, 69–71). It follows that the court had the right to decide what measures were appropriate. But he finds that the court's treatment of this particular issue is highly ambiguous: It vacillates between formulations that give a wholly unfettered discretion to the president and formulations that seek, to a greater or lesser degree, to limit that discretion. In particular, he argues that the court's reasoning is fundamentally and irreparably contradictory. It has no principled reason for upholding some legal constraints but not others (pp. 73–7).

According to Kelsen, the court attempted to repair this contradiction by making a distinction between the discretionary powers of the minister of a *Land* with respect to the particular affairs of that *Land*, which may be removed, and all others, which may not. The ground of the distinction offered by the court was simply that the latter pertain to the preservation of the independence of a *Land* and its place within the Reich and so are inviolable. But, Kelsen asks, how can a *Land* exercise these powers when their basis, that is, the entire internal administration, is at the disposal of officials appointed by the president? Not only is the distinction impossible to put into practice, but also there is not the slightest justification for it in the positive law. Henceforth, to the extent that there is any administration, its foundation resides neither in the Weimar nor in the Prussian constitution; it must reside in the norm contained in the decree of the president (Kelsen 1932–33, 77–81).

Kelsen also comments adversely on the court's express wish for cooperation in such circumstances between the governments of the Reich and a *Land*. Such cooperation is legally and politically impossible, especially because of the Damoclean sword hanging over the *Land* by virtue of the court's interpretation of Article 48.1. That is, the court says the president can intervene should the government of the *Land* fail to fulfill its duty in its remaining sphere of operation. And, Kelsen notes, it hardly seems that anything could be done in violation of the government's duty, since government has in effect been removed (Kelsen 1932–33, 81–3).

Up until this stage of his analysis, Kelsen generates in the reader the expectation that the court had the power and even the duty to invalidate the decree. In particular, it could have declared that the presuppositions of action under Article 48.2 had not been fulfilled or that, while they had been fulfilled, the measures in fact adopted went beyond the powers of the president because they overstepped absolute constitutional limits. But the last part of Kelsen's analysis lets the court entirely off the hook.

Kelsen begins this part by drawing attention to what he takes to be a flaw in the court's judgment. The court did not in fact declare which particular aspects of the federal government's decree were invalid or valid, but merely said that action in terms of the decree would be invalid "insofar as" it impinged on the inviolable constitutional provisions. Kelsen's opinion that this is a flaw is driven by the claim that the norm contained in the president's decree is valid until it is in fact nullified, and hence it is only voidable; that is, any presidential decree is valid until expressly invalidated by the court, as long as the decree is within "the limits of his authority" (Kelsen 1932–33, 82–5). In the absence of a declaration of invalidity by the court, the president's decree remains valid.

According to Kelsen, this flaw is compounded by the fact that Article 19 of the Constitution, which established the court, concludes: "The president will execute the judgment of the court." Two interpretations of this article are, he states, possible: Either the judgment is declaratory of invalidity and thus immediately effective, or it is effective from the time of execution by the president. On the first interpretation, the judgment in this case is ineffective because it is altogether unclear what the court invalidated. On the second interpretation, it is for the same reason unclear what the president is required to do, that is, whether he is obliged to issue a new decree or to invalidate the old, and so he is not in fact obliged to do anything (Kelsen 1932–33, 85–9).

Kelsen then ends his critique of the judgment by exonerating the court. The result of a juristic or scientific analysis is, he declares, unsatisfying. The root of this difficulty is not the judgment but the technical inadequacy of the Constitution. It does not provide effective checks for its preservation, in particular no constitutional court properly equipped and aware of its role in exercising control over application of Article 48. This was not an oversight but a consequence of the age-old opposition of German jurisprudence to judicial control of what it takes to be an extralegal "political sphere." Control was given instead to an "executory law," a statute of the *Reichstag* that would, in terms of Article 48.5, "determine the particulars" of valid action; but the *Reichstag* had never gotten round to enacting such a statute.

Kelsen asserts that, as a result, the president had an unfettered discretion. The authors of the Constitution no doubt intended for the president's powers to be strictly limited to the restoration of order and security in the narrowest sense. But this intention, which is not as easy to determine as the content of a statute, and which, even if determinable, is not the exclusive basis for interpretation, was not given an adequate technical expression.

Thus, in the absence of a general norm contained in a statute determining the limits of Article 48, an extensive as well as a restrictive interpretation is permitted. It thus would be self-deceiving to suppose that Article 48 could not be used to change the federal state into a unitary state. The boundaries of Article 48 cannot be discovered in the process of interpretation but only in the process of constitutional lawmaking.

Kelsen concludes that the interpretation of Article 48 entailed by the president's decree was not only as plausible as the court's but also, from a political standpoint, to be preferred. While it was a radically centralizing measure, it at least brought about, technically speaking, an administratively manageable situation. In contrast, the court strove to find a middle ground that actually increased the legal confusion. "The Weimar Constitution is not saved on the golden middle way which the court sought" (Kelsen 1932–33, 89–91).

Of course, this conclusion and the argument leading to it allow for the possibility that the court could have done what Kelsen clearly regards as legally permissible from a scientific point of view. That is, the court could have issued an order which both had the requisite constitutive character, an act of clear judicial legislation, and invalidated the decree in whole or in part. Moreover, Kelsen tends toward advocating invalidation in whole.

But in choosing to focus on the court's failure to give its order this character, Kelsen seems to make clear his view that whatever the court ordered, as long as its order was both constitutive and clear, would be equally valid from the standpoint of legal science. Thus, the court could just as well have ordered that the decree stand as a whole. Kelsen believes such an order to be the effect by default of the judgment and in a way preferable because the situation of radical centralization is at least unconfused. He is, therefore, prepared to go even farther than the court, since he holds that even the formal limits the court upheld, in particular the principle that Prussia's right at the federal level was inviolable, could be abolished by the president's decree.

On the one hand, then, Kelsen's argument tends in the following direction. Constitutional review is the technically appropriate means for bringing to expression the principle of legality, which is identified as a principle by legal science. The issue in the case, although it concerns an actual political conflict, is also a legal question and eminently justiciable, that is, decidable by a court. The appropriate judgment is one which would have upheld the democratic and federal structure of the Constitution. On the other hand, he wants to argue that legal science has little to say on this matter, because from its standpoint a judgment that the decree was constitutional and that it was unconstitutional are equally political acts of judicial legislation. Neither the positive law nor Kelsen's legal theory supply any legal reason to reject Schmitt's argument before the court.

In sum, Kelsen seems at times to be developing a theory of constitutionality that would show how the formal aspects of a legal order impose genuine constraints on political power.[25] This theory appears to be organized around a principle of legality which gives substance to the idea of the *Rechtsstaat*. It is in terms of such a theory that he criticizes both Schmitt and the judgment. As soon as Kelsen comes to the point of saying what such constraints would amount to, however, he either seems to retract entirely or to say that debate about such constraints is a matter for politics, not legal science.[26]

Why does Kelsen reach this conclusion? He holds that the correct or scientific account of legal order is given by his Pure Theory of Law (Kelsen [1934] 1992).

---

[25] The case for this view of Kelsen is made in detail in Dyzenhaus forthcoming.

[26] Indeed, Kelsen's major criticism of Schmitt is not with the substance of Schmitt's legal and political agenda but with dressing up the agenda in a legal argument, whereas in truth Schmitt was advocating a political position. In other words, Schmitt is criticized not because of his political objective to expand the powers of the president but because he presented his objective as a "scientific treatment" (Kelsen 1930–31, 627–8). One should not ignore that there is a certain force to this point. Schmitt usually adopts the guise of the legal scientist, a dispassionate commentator on legal order. Only occasionally does he let this mask drop and allow his passion for the political to peep through. Kelsen shows effectively how Schmitt's conception of the political drives everything else in his theory.

That is, a legal system is a hierarchy of norms, and the validity of each is traceable to a more fundamental norm, until one hits bedrock in the *Grundnorm* or basic norm of the system. Such an order is free of contradictions since any apparent conflict between two norms will be resolved by a more fundamental norm, which gives an official the power to make a binding decision. Similarly, any factual matter to which the law is relevant can be resolved by the law, not in the sense that one can deduce the content of the right decision from a higher-order norm, but in that there will be a higher-order norm which empowers an official to make a binding decision. Since the validity of the basic or constitutional norm cannot be traced to any other norm, Kelsen asserts that its validity therefore must be assumed. The content of the basic norm is contingent on what the founders of the particular legal order in fact willed (pp. 59–60).

Kelsen often claims that any state is a *Rechtsstaat*, no matter the political ideology it serves—communist, fascist, or liberal (Kelsen [1928] 1981, 253). He also states that any dispute as to whether and how the law resolves the facts of a particular case is settled by an act of official discretion, for example, by the judge determining an outcome by ultimately resorting to his own political and ethical views (Kelsen [1934] 1992, 67–8, 84–9).

The major problem for the Pure Theory is that it seems to exemplify exactly the contradiction Schmitt claimed to be endemic to liberal legalism. It seeks to constrain politics by law but ends in a position whereby the political, free of all legal constraints, seems to dictate decisions as to what the law is. The political, that is, provides the criteria for judgment.

In addition, it is difficult to see just what basis Kelsen might offer for criticizing the political ambition of Schmitt's authoritarian theory. At times he comes close to arguing that his understanding of the principle of legality is that one which is required to make sense of a system of parliamentary democracy, itself seen as the fulfillment of a commitment to individual liberty (Kelsen [1929] 1981). In his view, democracy is the political system which creates a coercive order of the kind that screens out any claim that the majority possesses the truth, or that the minority can ever be entirely wrong, by keeping open the opportunity for the minority to become the majority at any time (pp. 101–3).

But, despite the fact that Kelsen seems committed to establishing the *Rechtsstaat* as the legal order of parliamentary democracy, he retreats just before his argument makes a substantive connection between law and political values. His retreat is the result of an avowed relativism about political values, a stance which precludes him from bridging his conceptions of political and legal order by dint of a fully argued commitment to a substantive political theory. Democracy is to be preferred only because it is the expression of a political relativism, itself the product of an epistemological relativism.

Kelsen's position in legal theory, which drives his analysis of the judgment, thus maps neatly onto his position in political theory.[27] The latter perceives no qualitative difference between the legal order of a parliamentary democracy and the legal order established by a dictator.

HELLER'S DEMOCRATIC VISION

Heller was determined to make the case that the pattern of events both before and after the coup showed that, far from being the action of the independent, apolitical government portrayed by Schmitt, the coup was the result of deliberate arrangements between the Nazis and Papen's cabinet. That would, in his view, suffice to show an unconstitutional abuse of discretion (*Preussen* [1933] 1976, 76–7).

The president of the court, Bumke, put to Heller a question which clearly presaged the court's judgment on this issue.[28] Even if such arrangements were relevant to the issue of action under paragraph one, did they have any relevance to action under paragraph two? That is, was Heller denying that public safety and order were disturbed and endangered in Prussia prior to the coup (*Preussen* [1933] 1976, 77)?[29]

Heller's response denied the premise that the disturbance was of the kind contemplated by Article 48. He said that public safety and order had been constantly disturbed in Germany since 1914. The question for the court was whether the disturbance in Prussia was of such a magnitude as to justify intervention and, if so, an intervention that abolished the government when at the same time the Reich government did not act against the other *Länder* in that way.

In Heller's view, the crucial question for the court was whether the Prussian government had been willing and able to deal with the disturbance. Not only was that clearly the case, he maintained, but also, in view of the events following the coup, it had proved itself better able to do so than had the commissioner (*Preussen* [1933] 1976, 77–8).

---

[27] A complicating factor in any evaluation of Kelsen's monograph on democracy is what status he himself would have assigned it: whether he regarded it as an exercise in value-laden political theory by contrast with his work in legal science. My view is that he at the least saw it as a work of political *science*, that is, a work about politics from the same scientific standpoint as his work in legal science.

[28] Heller's appearance before the court on behalf of the Prussian SPD was not a happy one. He lacked Brecht's cool demeanor and, indeed, was often both sarcastic and angry. His opening salvo against Papen and Hitler brought a rebuke from Bumke that set the tone for their interaction throughout the hearing; *Preussen* [1933] 1976, 35–7, at 37. For further such ill-tempered exchanges between Heller and Bumke, see pp. 63–5, 250, 470, and especially 410–1, where Bumke reacts in a way that shows his anger at Heller's challenge to the court. The tone of the exchanges between Schmitt and Heller was also angry; see, for example, pp. 356 and 470. Bumke a respected, highly conservative jurist during Weimar, threw in his lot with the Nazis after 1937 and committed suicide in 1945. For a scathing account of his career, see Müller 1991, 39–45.

[29] Heller's determination in this regard created some tensions between his own argument and that presented by Brecht, who had announced at the outset that the Prussian government did not seek in any way to impugn Hindenburg's integrity, but merely wanted to make sure he had the right information and the right understanding of the Constitution. And Brecht extended this courtesy to the motives of the Reich government (*Preussen* [1933] 1976, 8–9). Bumke did his best to play on this tension (p. 77) and to cut off Brecht's and Heller's attempts to play on tensions in the Reich's arguments; see, for example, pp. 85, 187. Heller ([1932] 1992a, vol. 2) had already sketched this argument in a response to Schmitt.

Heller acknowledged that in practice the question of the validity of action in terms of the different paragraphs could be linked. But in contrast to Schmitt he argued that, in the context of the coup, if action under paragraph one was not justified, then the Prussian government could fulfil its duty of maintaining public peace and order, and thus there could be no basis for intervention under paragraph two. His argument was that the lack of a basis was demonstrated by the kind of intervention that occurred, namely, an obvious political strike against the Prussian government to rid it of SPD influence.[30] In particular, he rejected Schmitt's premise that a court was not competent to assess the validity of action in terms of either paragraph because Heller rejected Schmitt's argument for that premise—to grant such competence to a court would constrain the president's power so as to threaten the destruction of the Reich.[31]

The importance of Schmitt's premise for the Reich's case is clear. The initial proclamations to justify the coup relied more heavily on paragraph one than paragraph two. That is, once the court is conceded the competence to decide on the validity of action in terms of paragraph one, then there is a risk the court will find, as it did, that the action could not be justified, which almost compels a similar conclusion in terms of paragraph two. As we have seen, only the court's finding that any threat to public safety and order was sufficient to justify whatever decree the president saw fit to issue overrode the logic of the argument which invalidated the decree.[32]

Heller, indeed, warned the court in the clearest terms of the consequences of a decision of the kind it eventually reached. If the court were to uphold the validity of the Reich's intervention, it would in effect uphold the contention that the participation of the SPD in government was in itself a threat to public safety and order. The SPD's role in building democracy in Germany would then be at an end, and, Heller declared that the consequences were obvious to all present.

He noted that Brecht had invited the court to speak clearly if it were to uphold the validity of the intervention. Heller, as a social democrat, invited the court to do the same, for then the political situation of Germany would be clear for once and for all. As a jurist and a German, however, he wanted the court to take account of the fact that the route one adopts to reach an end can be crucial and that one cannot build a legal order without being genuinely bound to the law (*Preussen* [1933] 1976, 406–9).

As Heller made clear in an essay published shortly after the coup, his argument depended on the idea that an unlimited jurisdiction or competence is self-contradictory. He argued that the president's jurisdiction under Article 48 had to be limited by the very Constitution which granted it. Those limitations were the ones that accorded with the correct understanding at law of the presuppositions of the Constitution (Heller [1932] 1992a, vol. 2).[33]

In his monographs on legal theory, Heller argues that the standards for correct judgment about the content of law are legal, and he says that these standards must govern legal interpretation, including interpretation by judges. His position differs from Schmitt's in that, with Kelsen, he thinks law provides a constraint on power. But he differs from Kelsen in holding that the constraints can be given substance only by principles of law which, while immanent in legal order, are in their nature political and ethical and fundamental to such order (Heller [1934] 1992, vol. 3, 332–3).[34]

Heller argues that one cannot make sense of the idea of legal order without recognizing the role of such fundamental principles of law in constituting legal order. The rules of positive law, which for Kelsen are what law amounts to, according to Heller are made possible only by the existence of these fundamental principles of law. Moreover, the role of positive law is to give content to the fundamental principles. Thus, an awareness of their existence is essential to the interpretation of positive law.

Heller acknowledges that, even if one assumes the existence of fundamental principles, such as equality before the law, their content is controversial. Furthermore, just as the existence of positive law is made possible by the fundamental principles, so they in turn require the existence of positive law in order to manifest themselves in legal order. They thus require the existence of a political decision unit, a sovereign entity capable of making the decisions that positivize fundamental principles. It might then seem that the content of positive law is arbitrary, dependent as it is on the character of the people who staff the decision unit.

Heller's response to this claim is that it rests on the mistake made by both Schmitt and Kelsen when they suppose that legal meaning is up for grabs in a case of political controversy as to the content of positive law, which cannot be solved by the positive law. It is a mistake for at least two reasons. First, it assumes the truth of the relativist position that controversy over legal interpretation among different groups means there is no rational resolution of those issues. That is, the only resolution is by irrational mechanisms of politics. Second, it fails to see that politics is not merely the irrational play of forces, that political power is in its nature subject to legal constraints (Heller [1929] 1992, vol. 2; [1934] 1992, vol. 3).

---

[30] For further argument along these lines, see Heller in *Preussen* [1933] 1976, 379–80.

[31] *Preussen* [1933] 1976, 135–9, esp. 138–9, referring to Schmitt's argument at 130–4. See also Heller at pp. 167–70, p. 186.

[32] Subject, of course, to the notional constitutional constraints the court upheld.

[33] This argument is anticipated in Heller [1927] 1992, vol. 2, 127. It is important to appreciate that prior to 1930 Heller was hostile to judicial review, although it is not clear to what extent his hostility was based on the conservative, even right-wing, character of the German judiciary or on a democratic theory which confined the legislative role to parliament (Heller [1929] 1992, vol. 2). One interpretation of his advocacy for an activist role for the court in this case is that it was driven purely by the political impotence at this time, of the *Reichstag* which had the authority under paragraph three of Article 48 to revoke emergency measures. My own view is that there is a basis for a theory of democratic judicial review in Heller's work, one which was merely brought to the surface by the events of the 1930s.

[34] On this understanding of Heller, his work would provide a better basis for the theory of value-oriented judicial review in postwar Germany than the conservative and highly influential work by Rudolf Smend, whose constitutional writings from this period are included in Smend 1995.

We are now in a better position to understand the point of Heller's argument before the court. He did more than call for a return to the legal status quo ante of parliamentary democracy. He wanted the court to understand that it had to play a role in restoring the institutional or organizational integrity of democracy. He was asking the court to help keep alive the democratic impulse of the Weimar Constitution because this impulse should inform the judicial understanding of fidelity to law.

His remark that Germany had not enjoyed public safety and order since 1914 goes to the heart of the conflict between pro- and antirepublican forces in Weimar. As we have seen, Article 48 was drafted in anticipation of the need for central government to deal with political crises whose resolution outstripped the ordinary mechanisms of politics and law. Its particular structure, and the failure of the *Reichstag* to take advantage of Article 48.5 to "determine the particulars" of valid action, meant that embedded in the Weimar Constitution was a fatal ambiguity between two conflicting political commitments. On the one hand was the commitment to the legitimacy of parliamentary democracy, on the other was the commitment to the legitimacy of a charismatic leader.

It is worth noting that Friedrich Ebert, the first president of the Weimar Republic, often resorted to Article 48 between 1918 and 1925, not only to put down rebellions from the extreme right and left but also, much more frequently, to deal with economic, fiscal, and social problems. He thus set the stage for the practice by the presidential cabinets of late Weimar, but there is a crucial difference.

While the *Reichstag* accepted Ebert's actions in derogation of its legislative rights,[35] there was no doubt that everything he did sought to bring about, under very difficult economic, social, and political conditions, a period of stability in which democracy could take root. Indeed, he tried to avoid use of Article 48 and preferred to ask the *Reichstag* for an enabling statute that gave him authority for a limited period to legislate by decree on a particular issue, an authority which was subject to parliamentary veto (Rossiter 1948, 44–9).

In other words, Ebert's use of Article 48 presupposed that the normal, constitutional situation was parliamentary democracy, and the particular measures he took were directed by the norms of that situation. It was not political conflict per se that constituted an emergency but political conflict which threatened the maintenance of parliamentary democracy. Such an understanding of a state of emergency meant that both its definition and its resolution were framed by law.

Schleicher and Schmitt, in contrast, considered the perpetuation of parliamentary democracy amid the turmoil of late Weimar to be the state of emergency to which the federal government should react. And that reaction was clearly aimed at establishing a new constitutional and legal order through the back door of Article 48 (Mommsen 1996, chapter 12).

In sum, Heller's view that there was no state of emergency did not deny the extreme nature of the political crisis faced by both the federal and the *Land* governments in 1932. Rather, he perceived a state of emergency as not simply a political crisis but as a constitutional-legal response to such a crisis. No matter the depth of political conflict, for a declaration of a state of emergency to be valid, it must be aimed at the return to the normal, constitutional situation in whose service the relevant legal provisions stand. The fact that the *Reichstag* had never enacted the executory law under Article 48.5 did not mean the legal norms which such a law would have concretized had no application. If the court were to make any sense of its role as an essential component of legal order, it had to apply an understanding of just those norms.

Heller thus thinks that to make sense of the idea of the *Rechtsstaat* one must attempt to link together law, politics, and democracy in one theory about legal order. Kelsen's constant refusal to take the messy step of bridging conceptions of legal and political order collides with his recognition that virtually every legal decision has to take politics and morals into account. By contrast, Heller integrates his theory of the appropriate political organization of democracy and his account of legal order in a way that directly confronts concrete questions of appropriate institutional arrangements.

Heller is in one respect similar to Kelsen: He wants to emphasize that law provides us with a means to positivize values. But he departs from Kelsen in supposing that there must be a point to the process of positivization. The point comes from the realization that we have to make our order of values together—that the final court of appeal does not lie beyond our collective sense of what is right and wrong. But law is then not just an instrument of that collective sense. Rather, law is the way of ascertaining that sense in a process in which those charged with authoritative determinations of the content of the law are kept accountable. Put differently, the legal order is not just an instrument of democracy but an essential part of its realization.

For Heller, the basis of legal order is in part a social one, the citizen's sense of both the actual level of social equality reached and the commitment of the society to social equality. It is in part a political one, the sense of whether politics makes room for citizens to be authors of their own political and social order, so that they are able to influence both legislation and law reform. In the terms Heller uses to describe the citizen, this sense is one of the contingency of the concrete order established by law. It is contingent in that it is the result of politics, but of politics conducted within democratic institutions and thus subject to change. This sense of contingency, that is, requires the institutions of the democratic *Rechtsstaat* to be intrinsic elements of legal order. It is one which makes the following criteria for the validity of law: the democratic process of its production, its implementation and execution through a system of the division of powers, and its openness to reform in the light of citizens' experience of it (Heller [1928] 1992, vol. 2; [1932] 1992b, vol. 2).

---

[35] It only once required cancellation of a decree.

Heller's view is that the point of the democratic institutional structure of the *Rechtsstaat* is to make it possible for the values of social and political order to be positivized in a way that makes the powerful accountable to the subjects of their laws. Morality, in the sense of the values by which the collectivity can legitimately require we live, is just the set of values that are concretized through the positive law. The subjects of the law become its authors through their representatives, who enact legislation, but their authorship does not end there—it continues through an appropriate process of concretization of the legislation.

What makes that process appropriate is that, both institutionally and substantively, the interpreters of the law must regard themselves as participating in a process of legislation which instantiates fundamental ethical principles of law. Most abstractly, these are the principles that promise both freedom and equality to all citizens. The ultimate check on delivery of such promises can be nothing other than the individual legal conscience—the individual citizen's sense of whether the law is living up to its promise (Heller [1934] 1992, vol. 3, 333–9).[36]

Indeed, Heller regards the seeds of modern democracy as sown with the appearance of two ideas: The law binds the rulers to the ruled, and the rulers must find an immanent justification for their rule. Legal process is not then empty form but a process with a substantive point which both shapes and is shaped by the process. To forget this is to legitimate the abuse of legal form by those who want to use it as a cloak for their attempt to seize power.

LESSONS FROM WEIMAR?

Let us return to the question of how, in light of Weimar, political and legal philosophy should respond to the fact of pluralism—the fact of conflict about fundamental values.

Under the direction of John Rawls (1993), liberalism now seems driven by the fear that to claim truth for one's position is to invite a clash of truth claims, which can only breed dissent and conflict. Hence, in seeking to set out the values of the domain of the political, liberalism must claim only that these are the values to which it is reasonable to assent. These "freestanding" values together make up an "overlapping consensus" about the basics of political and legal order (p. 140).

For Rawls, these values stand free of comprehensive positions or individual conceptions of the good life. While such positions perforce claim truth for themselves if they enter the space of public reason or constitutional discourse, the values constituting that space claim only reasonableness. But the claim to reasonableness is far from modest. It operates to exclude the truth claims of comprehensive positions from the public and requires them to contest one another only within the sphere Rawls calls the "social" (p. 220).

Rawls recognizes the effects of relegating comprehensive positions to the social sphere. Illiberal groups will find it hard to maintain themselves since their comprehensive positions are perforce undermined by the public culture of political liberalism (pp. 199–200). And Rawls is clear that if such groups try to gain control over politics they should be "contained" (pp. 37, 54, 60–1). Indeed, in an emergency situation, when it appears containment is not working, political liberalism may have to drop its claim to mere reasonableness and assert its truth in a conflict over political fundamentals (pp. 152–6). Rawls thus seems to vacillate between a curious "epistemic abstinence" about fundamental political values and a deep practical and epistemological commitment to them.[37]

I suggest that there is more than a passing resemblance between this vacillation and Kelsen's refusal to bridge his conceptions of political and legal order by dint of a fully argued commitment to a substantive political theory. The claim to reasonableness of Rawls's political liberalism and the claim to purity of Kelsen's legal theory aspire to a neutrality that will not alienate otherwise fundamentally divided groups. They both wish to preserve democratic politics by not insisting on the rightness of a set of values. But their methods of avoidance create a tension with their commitment to democratic politics.

Indeed, I suggest that Rawls and Kelsen end up gravitating toward opposite poles of the tension within the liberal idea of the rule of law which Schmitt wishes to highlight. This is the tension between a neutrality so neutral that anything goes and a neutrality which is sham because in effect it privileges a partial liberal understanding of the good.

On Kelsen's positivist conception, laws with any content at all can fit the criteria for the validity of law. Unlike Kelsen, Rawls wishes to privilege certain values as the values of politics. He thus proposes criteria of validity which have more (liberal) substance to them. But since, like Kelsen, he wants truth claims to be checked at the door of politics, he remains evasive about their status in a way which invites the charge of sham neutrality.

The tension in the liberal conception of the rule of law can be reduced in two different ways. First, Rawls could give up on the justificatory project altogether.[38] But that would take Rawls along the Kelsenian path, whose danger is not that it is paved exclusively for either saints or sinners, but that it cannot discriminate between the two. That is, the tension is reduced at the theoretical level but in a way which leads to the principled defenselessness of liberalism. The tension is displaced into a free-for-all of politics, where politics is conceived as a kind of normative vacuum, a space contested by groups making distinctions between friend and enemy, on whatever lines they care to define (Schmitt [1932] 1976, 38).

---

[36] In the pages cited in the text, Heller argues that it is a necessary paradox of the democratic *Rechtsstaat* that the ethical right of resistance of the legal conscience is something which has weight but no legal recognition. I explore his complex position on this issue in Dyzenhaus forthcoming.

[37] I owe the term "epistemic abstinence" to Raz (1994).

[38] Richard Rorty (1991) believes Rawls has already taken this option.

Second, Rawls might develop a full justification for the values of the "political." But that justification would have to avoid what Rawls correctly wants to avoid—the privileging of any particular view of the good life.

I suggest that Heller offers the basis for a way out of this tension. In his view, politics is not a normative vacuum but the space one has when those institutions are in place which best allow for the emergence, contestation, and revision of fundamental values in the light of experience. The rule of law is then the institutional mechanism of democracy. Its justification is the same as the justification for democracy itself. And that justification requires both a fully argued commitment to the rightness of democracy and a recognition that democratic politics must be much more open than liberals today commonly suppose.[39]

Of course, as Heller often emphasized, this will make democratic politics into a risky realm. Besides anything else, the very question of the best institutional arrangements must become the subject of political debate and decision. But, in a time of increasing skepticism about the value of liberal democratic institutions, it may well be best, along with Heller, to face up properly to that question.

---

[39] Habermas (1996) can thus be viewed as the standard-bearer of Heller's project.

REFERENCES

Bracher, Karl Dietrich. 1984. *Die Auflösung der Weimarer Republik* [The dissolution of the Weimar Republic]. 5th ed. Dusseldorf: Droste Verlag.

Brecht, Arnold. [1944] 1968. *Prelude to Silence: The End of the German Republic*. New York: Howard Fertig.

Dyzenhaus, David. N.d. *Truth's Revenge: Carl Schmitt, Hans Kelsen, and Hermann Heller in Weimar*. Oxford: Clarendon Press. Forthcoming.

Eyck, Erich. 1967. *A History of the Weimar Republic*, vol. 2. Trans. Harlan P. Hanson and Robert G.L. Waite. New York: John Wiley.

Finn, John E. 1991. *Constitutions in Crisis: Political Violence and the Rule of Law*. New York: Oxford University Press.

Grimm, Dieter. 1992. "Verfassungserfüllung—Verfassungsbewahrung—Verfassungsauflösung: Positionen der Staatsrechtslehre in der Staatskrise der Weimarer Republik" [Constitutional fulfilment—constitutional preservation—constitutional dissolution: The positions of public lawyers in the state crisis of the Weimar Republic]. In *Die Deutsche Staatskrise 1930–33: Handlungsspielräume und Alternativen* [The German state crisis 1930–33: Room for free play and alternatives], ed. Heinrich August Winkler. Munich: R. Oldenbourg.

Grund, Henning. 1976. *"Preussenschlag" und Staatsgerichtshof im Jahre 1932* [The "strike against Prussia" and the court in 1932]. Baden-Baden: Nomos.

Habermas, Jürgen. 1996. *Between Facticity and Norms: Contributions to a Discourse Theory of Law and Democracy*. Trans. William Rehg. Cambridge, MA: MIT Press.

Hart, H. L. A. 1961. *The Concept of Law*. Oxford: Clarendon Press.

Hart, H. L. A. 1983. *Essays in Jurisprudence and Philosophy*. Oxford: Clarendon Press.

Heller, Hermann. 1992. 3 vols. *Gesammelte Schriften* [Collected writings], 2d ed., Christoph Müller. Tübingen: J. C. B. Mohr (Paul Siebeck).

  Vol. 2. *Recht, Staat, Macht* [Law, state, power].

    [1926] 1992. "Die Krisis der Staatslehre" [The crisis of state theory.

    [1927] 1992. *Die Souveränität: Ein Beitrag zur Theorie des Staatsund Völkerrechts* [Sovereignty: A contribution toward the theory of state and international law].

    [1928] 1992. "Politische Demokratie und Soziale Homogenität" [Political democracy and social homogeneity].

    [1929] 1992. "Rechtsstaat oder Diktatur" [Rule of law or dictatorship].

    [1931] 1992. *Europa und der Fascismus* [Europe and fascism].

    [1932] 1992a. "Ist das Reich Verfassungsmässig vorgegangen?" [Did the Reich act in accordance with the constitution?].

    [1932] 1992b. "Bürger und Bourgeois" [Citizen and bourgeois].

  Vol. 3. *Staatslehre als politische Wissenschaft* [Theory of the state as political science].

    [1934] 1992. *Staatslehre* [Theory of the state].

Kelsen, Hans. 1930–31. "Wer soll der Hüter der Verfassung sein?" [Who should be the guardian of the constitution?] *Die Justiz* 6 (11/12):576–628.

Kelsen, Hans. 1932–33. "Das Urteil des Staatsgerichtshof vom 25. Oktober 1932" [The court's judgment of 25 October 1932]. *Die Justiz* 8 (2/3):65–91.

Kelsen, Hans. [1928] 1981. *Der soziologische und der juristische Staatsbegriff* [The sociological and juristic concepts of state], 2d ed. Tübingen: Scientia Verlag Aalen.

Kelsen, Hans. [1929] 1981. *Vom Wesen und Wert der Demokratie* [The nature and value of democracy], 2d ed. Tübingen: Scientia Verlag Aalen.

Kelsen, Hans. [1934] 1992. *Introduction to the Problems of Legal Theory*. Trans. Bonnie Litschewski Paulson and Stanley Paulson. Oxford: Clarendon Press.

Kennedy, Ellen. 1984. "The Politics of Toleration in Late Weimar: Hermann Heller's Analysis of Fascism and Political Culture." *History of Political Thought* 5 (Spring):109–27.

Kolb, Eberhard. 1988. *The Weimar Republic*. Trans. P.S. Falla. London: Unwin Hyman.

Lustick, Ian. 1996. "History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias." *American Political Science Review* 90 (September):605–18.

Mommsen, Hans. 1996. *The Rise and Fall of Weimar Democracy*. Trans. Elborg Forster and Larry Eugene Jones. Chapel Hill: University of North Carolina Press.

Müller, Ingo. 1991. *Hitler's Justice: The Courts of the Third Reich*. Trans. Deborah Lucas Schneider. Cambridge, MA: Harvard University Press.

Niemeyer, Gerhart. 1941. *Law without Force: The Function of Politics in International Law*. Princeton, NJ: Princeton University Press.

Preussen. [1933] 1976. *Preussen contra Reich vor dem Staatsgerichtsthof: Stenogrammbericht der Verhandlungen vor dem Staatsgerichstshof in Leipzig vom 10. bis. 14 und vom 17. Oktober 1932* [Prussia v. Reich before the court: Stenographic report of the proceedings before the court in Leipzig, 10 to 14 and 17 October 1932]. Glashütten im Taunus: Verlag Detlev Auvermann Kg.

Rawls, John. 1993. *Political Liberalism*. New York: Columbia University Press.

Raz, Joseph. 1983. *The Authority of Law: Essays on Law and Morality*. Oxford: Clarendon Press.

Raz, Joseph. 1994. "Facing Diversity: The Case of Epistemic Abstinence." In *Ethics in the Public Domain: Essays in the Morality of Law and Politics*. Oxford: Clarendon Press.

Rorty, Richard. 1991. "The Priority of Democracy to Philosophy." In *Objectivity, Relativism, and Truth: Philosophical Papers Volume 1*. New York: Cambridge University Press.

Rossiter, Clinton L. 1948. *Constitutional Dictatorship: Crisis Government in Modern Democracies*. Princeton: Princeton University Press.

Rüthers, Berndt. 1989. *Entartetes Recht: Rechtslehren und Kronjuristen im Dritten Reich* [Degenerate law: Law professors and state law officers in the Third Reich], 2d ed. Munich: C.H. Beck.

Scheuerman, William E. 1994. *Between the Norm and the Exception: The Frankfurt School and the Rule of Law*. Cambridge, MA: MIT Press.

Schlink, Bernhard. 1994. "German Constitutional Culture in Transition". In *Constitutionalism, Identity, Difference, and Legitimacy: Theoretical Perspectives*, ed. Michel Rosenfeld. Durham: Duke University Press.

Schluchter, Wolfgang. 1983. *Entscheidung für den sozialen Rechtsstaat: Hermann Heller und die staatstheoretische Diskussion in der Weimarer Republik* [Decision for the social rule of law: Hermann Heller and the discussion of state theory in the Weimar Republic], 2d ed. Baden-Baden: Nomos.

Schmitt, Carl. 1932. "Die Verfassungsmässigkeit der Bestellung eines Reichskommissars für das Land Preussen" [The constitutionality of the appointment of a commissioner for the Reich in Prussia]. *Deutsche Juristen-Zeitung* 15 (41):954–8.

Schmitt, Carl. [1932] 1976. *The Concept of the Political.* Trans. George Schwab. New Brunswick, NJ: Rutgers University Press.

Schmitt, Carl. [1922] 1988. *Political Theology*. Trans. George Schwab. Cambridge, MA: MIT Press.

Schmitt, Carl. [1932] 1988. *Legalität und Legitimität* [Legality and legitimacy], 5th ed. Berlin: Duncker & Humblot.

Schmitt, Carl. [1922] 1989. *Die Diktatur* [Dictatorship]. Berlin: Duncker & Humblot.

Schmitt, Carl. [1928] 1989. *Verfassungslehre* [Constitutional theory]. Berlin: Duncker & Humblot.

Smend, Rudolf. 1995. *Staatsrechtliche Abhandlungen und andere Aufsätze* [Essays on public law and other articles], 3d ed. Berlin: Duncker & Humblot.

American Political Science Review Vol. 91, No. 1 March 1997

Stability and Coordination in Duverger's Law: A Formal Model of Preelection Polls and Strategic Voting

MARK FEY *Princeton University*

This paper investigates the dynamics of the "wasted vote" phenomenon and Duverger's Law. I construct a theoretical model in order to consider how preelection polls serve to inform the electorate about the relative chances of the candidates and how that information acts over time to decrease the support of the trailing candidate. The results shed light on how public opinion polls can aggregate information in the electorate and coordinate voters on the viable candidates in the election. Specifically, I show that in a Bayesian game model of strategic voting there exist non-Duvergerian equilibria in which all three candidates receive votes (in the limit). These equilibria require extreme coordination, however, and any variation in beliefs leads voters away from them to one of the Duvergerian equilibria. Thus, non-Duvergerian equilibria are unstable, while two-party equilibria are not. In addition, I describe how preelection polls provide information to voters about the viability of candidates and can thus be used by voters to coordinate on a Duvergerian outcome.

How electoral laws affect the number and strength of political parties has been the focus of a great deal of research in political science. A centerpiece in this effort is the famous work of Duverger (1954) on the effect of election type on political competition. There are two main statements to consider. The first, which Riker (1982b) terms "Duverger's Law," is the proposition that "the simple-majority single-ballot [i.e., plurality] system favors the two-party system" (Duverger 1954, 217). In the next sentence, Duverger states that "of all the hypotheses that have been defined in this book, this approaches the most nearly perhaps to a true sociological law." Indeed, Riker has defended Duverger's Law as an example of the cumulative progress of political science. The second proposition, termed "Duverger's Hypothesis" by Riker, states that "the simple-majority system with second ballot and proportional representation favor multi-partism" (Duverger 1954, 239).

I will focus attention on Duverger's Law, the more widely studied of the two propositions.[1] It has a rich intellectual history dating back to 1881 and an English barrister named Henry Droop, who wrote that "the only explanation which seems to me to account for [two-party systems] is that the two opposing parties into which we find politicians divided in each of these countries [United Kingdom, United States, etc.] have been formed and are kept together by majority voting [what we now call plurality voting]" (quoted in Riker 1982b, 756–7).

Since Droop's time, a broad range of research has touched on Duverger's Law. Several excellent reviews cover both the empirical testing of the law and the theoretical explanations for its widespread applicability (Cox 1991; Duverger 1986; Riker 1982b, 1986). It has been subjected to a good deal of rigorous empirical testing (Lijphart 1994, Rae 1967, Taagepera and Shugart 1989) and has emerged with impressive approbation. From a theoretical perspective, Duverger (1954) offered the initial justification of his law. He reasoned that the field of parties is trimmed to just two by the forces of "fusion" and elimination.[2] Consider a third party (or candidate) receiving a small but significant number of votes. Facing a bleak electoral future, it may choose to ally itself with or even merge or fuse with one of the larger parties in exchange for consideration in other districts or a share of the benefits of office. This is the result of a strategic calculation made by the party leaders. If the third party does not go the route of fusion, it runs the risk that its voters will gradually desert it, and the party will fall victim to elimination because of lack of support. This alternative is the result of strategic calculations by the voters.

Duverger continues his reasoning by noting that "elimination in this sense (the second way in which bipartism is restored) is itself the result of two factors working together: a mechanical and a psychological factor" (1954, 225–6). The mechanical effect refers to the exaggeration of the success of large parties in excess of their electoral margin of victory.[3] The psychological factor is the tendency not to vote for parties without a chance of winning. As Duverger puts it:

> In cases where there are three parties operating under the simple-majority single-ballot system the electors soon realize that their votes are wasted if they continue to give them to the third party: whence their natural tendency to transfer their vote to the less evil of its two adversaries in

---

Mark Fey is Assistant Professor of Politics, Princeton University, Princeton, NJ 08544-1012.

I thank Tom Palfrey, Richard McKelvey, Roger Myerson, Gary Cox, Howard Rosenthal, Tim Feddersen, Rebecca Morton, and participants at the conference on Analysis of Political Institutions, Northwestern University, July 1995, for their time and helpful input.

[1] As Riker (1982b, 754) notes: "It is customary to call the law by Duverger's name, not because he had much to do with developing it but rather he was the first to dare to claim it was a law. The memorial honors, therefore, a trait of character as much as a scientific breakthrough."

[2] Duverger's use of "fusion" to describe mergers between political parties is sensible, but it runs counter to its common usage among U.S. political scientists as the joint sponsorship of a candidate by two or more parties. With this latter meaning, fusion is offered as an *exception* to Duverger's Law in which multiple parties persist in plurality-rule districts. Examples are elections in the Midwest and West during the late-nineteenth century (Argersinger 1980) and contemporary New York (Cox 1987).

[3] In U.S. presidential elections, the Electoral College has this effect.

> order to prevent the success of the greater evil. This "polarization" effect works to the detriment of a new party so long as it is the weakest party but it is turned against the less favoured of its older rivals as soon as the new party outstrips it (p. 226).

This notion that unpopular parties do not receive votes because voters do not want to waste their vote is well known (Downs 1957, Schattschneider 1942). In 1869, Droop explained the concept this way:

> As success depends upon obtaining a majority of the aggregate votes of all the electors, an election is usually reduced to a contest between the two most popular candidates or sets of candidates. Even if other candidates go to the poll, the electors usually find out that their votes will be thrown away, unless given in favour of one or other of the parties between whom the election really lies (quoted in Riker 1982b, 756).

Droop's comments foreshadow the explanation of strategic voting offered by decision theory (Black 1978, Gutowski and Georges 1993, McKelvey and Ordeshook 1972). In this literature, an individual voter in isolation makes a voting decision based on exogenously determined probabilities of certain outcomes. If the voter's favorite candidate has a poor enough chance of winning, the voter may decide to vote for his or her more favored of the two viable candidates, thus avoiding a "wasted" vote. The logical next step is to ask if the exogenous probabilities can be made endogenous: Is strategic voting supported even if voters account for the (possibly strategic) votes of others? Cox (1987) made the first attempt to use this approach to analyze strategic voting in elections. By using the concept of Bayesian equilibrium (Harsanyi 1967–68), Cox was able to "close" the model: "By using an *equilibrium* concept one can take account of the logical connections that exist between preferences, beliefs about others' preferences, voting intentions, and beliefs about others' voting intentions" (1987, 16).[4] Cox's main contribution was to show that strategic voting is the norm rather than the exception and that this phenomenon tends to help a leading candidate and hurt a trailing candidate. Palfrey (1989) extended this approach to prove a much stronger result. Using a Bayesian game much like Cox's, he showed that as the size of an electorate grows without bound, the electoral support of all but the two leading candidates is completely eroded. Palfrey argues that, except for some knife-edge cases, "in equilibrium, the share of the vote for the third party declines to zero as the number of voters increases" (p. 72). Thus, Palfrey provides a strong theoretical argument in support of Duverger's Law.

I will briefly mention several other equilibrium models of Duverger's Law.[5] Riker (1976, 98) adds "disillusioned voting" to a model of sophisticated voting to explain the case of India. Wright and Riker (1989) compare plurality and runoff systems using theoretical models and empirical evidence. Feddersen (1992) proposes a model with several unique features. Unlike Cox and Palfrey, Feddersen examines a complete information game with costly voting, which cost motivates voters to avoid wasting their vote on a hopeless candidate. Thus, in equilibrium, only two parties receive votes. Finally, Cox (1994) extends the Cox-Palfrey model to deal with multimember districts. He offers a formal proof of the "$M + 1$ rule," which states that strategic voting in M-seat districts has the effect that exactly $M + 1$ candidates receive votes in equilibrium.

Myerson and Weber (1993) argue that Palfrey has overstated his case in "proving" Duverger's Law. They construct a quasi-Bayesian "voting equilibrium" in which all three candidates receive votes under plurality rule, in violation of Duverger's Law. They also show that this equilibrium is not just a knife-edge case, as claimed by Palfrey (1989, 82), and conclude that "Duverger's Law cannot be derived exclusively from analyses of voting equilibria" (1993, 106). In addition, Myerson and Weber argue that "any derivation of Duverger's Law would seem to require some additional assumption of dynamic stability or persistence to eliminate equilibria of [this] type" (1993, 106). I respond to this critique by analyzing a dynamic process involving public opinion polls that coordinates voters on two viable candidates and thus eliminates the "non-Duvergerian" equilibria. So, although Palfrey's "knife-edge" claim is technically incorrect, these equilibria require a type of knife-edge coordination of beliefs that, if not exactly satisfied, leads to voters moving to a Duvergerian equilibrium.

The use of public opinion polls as a vehicle for information in the model links this work to the vast literature on public opinion polls, which cannot be surveyed here. Indeed, polls have long been used as a tool to measure public opinion and other variables of interest to political scientists. But the opposite effect, in which poll results change voters' beliefs and thus their decisions, has received less attention in the literature. The possible "bandwagon" and "underdog" effects of election prediction via public opinion polls were first discussed by Simon (1954).[6] McKelvey and Ordeshook (1985) offer a model in which straw polls serve to inform the electorate about the policy positions of candidates. Cukierman (1991) shows that preelection polls can amplify the effects of shifts in ideology in the electorate.

In this paper, I examine how public opinion polls can coordinate voters on particular Duvergerian equilibria and argue that coordination on a three-party equilibrium is impossible. The first section sets up the model and introduces the relevant concepts and notation. The next section shows that equilibria exist in the model that violate Duverger's Law. The third section introduces a dynamic process involving a sequence of polls and shows that the non-Duvergerian equilibrium is unstable. The final section illustrates this instability and discusses how polls can act as a coordination device for voters.

---

[4] Ledyard (1984) first proposed such a "general equilibrium" model of voting, but he did not consider multiple candidates.

[5] Much of this literature is surveyed by Cox (1987, 1991).

[6] Simon may have been the first scholar to analyze the possibility of "bandwagons," but ever since the first polls were taken people have recognized the possibility that their use can affect outcomes. See Smith (1990) and West (1991).

THE MODEL

In this section, I develop the model that will be used to examine the stability of non-Duvergerian equilibria in multicandidate elections. By way of an example, Myerson and Weber (1993) have shown that such equilibria do exist and are immune to small changes in the parameters. The model described here extends this example to the Palfrey model in order to show that the same result holds. We can thus view this framework as a hybrid of the models developed by Palfrey (1989) and Myerson and Weber (1993). The notation will follow that of Palfrey (1989).

There are three candidates, labeled A, B, and C, and n voters. Each voter in the electorate has a preference ordering over the candidates that can be represented by a Von Neumann–Morgenstern utility function. These preferences are referred to as a voter's type. In the general model of Palfrey (1989), there are six groups of similar voter types that occur with positive probability, and each group of types corresponds to one of the six possible (strict) preference orderings.[7] In addition, within each group, the utility of the second-ranked candidate is distributed according to some cumulative distribution function.

In my model, I pick a particular specification of preferences suggested by an example examined by Myerson and Weber (1993, 105–8). Suppose that the electorate is composed of three groups of similar types. Two of these groups involve a uniform distribution over the utility of the second most preferred candidate, while the third is a "degenerate" group involving a single shared type representing one specific preference ranking.[8] So, the electorate is described by three groups of voters and the probability that a randomly selected voter belongs to each group. The first group consists of voters who strictly prefer candidate C to the other two candidates, about whom they are indifferent. The probability a voter belongs to this group is denoted q_C, which, following Myerson and Weber (1993), I assume is equal to .4. The second group consists of voters who (strictly) rank candidate A above candidate B in their orders, and who prefer either A or B to C. Specifically, suppose that the utility values for the three candidates are normalized so that the most preferred candidate, A, has utility 1, the least preferred candidate, C, has utility 0, and voter α's utility for candidate B is v_α. I refer to this group of voters as AB type voters and a voter who assigns utility $v \in (0, 1)$ to B (the second-ranked candidate) as an AB-v type voter. Denote the probability that a randomly selected voter will be an AB type voter as q_{AB}. Assume the utility values for candidate B for voters in this group are distributed uniformly on $(0, 1)$.[9] The last group is made up of BA type voters, with utilities also distributed uniformly on $(0, 1)$. That is, voters in this group have a (strict) preference ordering that ranks B above A above C and have utilities for A distributed uniformly on the unit interval. The probability that a randomly selected voter belongs to this group is denoted q_{BA}. In order to show the general existence of non-Duvergerian equilibria in this setting, suppose that $q_{AB} = .3 + \varepsilon$ for some $\varepsilon > 0$, and thus $q_{BA} = .3 - \varepsilon$. The makeup of the electorate is summarized in Table 1. The set of possible types is denoted by T. Formally, I define

TABLE 1. Voter Types in the Electorate

Voter Type	Utility for candidate A	B	C	Prior Probability
AB	1	$v \sim U(0, 1)$	0	$q_{AB} = .3 + \varepsilon$
BA	$v \sim U(0, 1)$	1	0	$q_{BA} = .3 - \varepsilon$
C	0	0	1	$q_C = .4$

$$T = (\{AB\} \times (0, 1)) \cup (\{BA\} \times (0, 1)) \cup (\{C\}).$$

The probability of a particular type $t \in T$ is given by a probability measure P, which depends on q_{AB}, q_{BA}, and q_C.

One possible interpretation of the particular configuration of voter preferences in the model is as follows. Suppose that a majority (60%) of the electorate is ideologically liberal, while a minority (40%) is conservative, and that candidates A and B are liberals, while C is the lone conservative candidate. Then, the three types of voters consist of the segment of liberals who prefer A to B, with the conservative C as a last choice; the remaining liberals, who prefer B to A but also have C as a last choice; and the conservative voters, who rank C as their favorite and equally dislike the liberals, A and B. Such a situation arose in the 1970 New York senatorial election, in which two liberal candidates, Richard L. Ottinger and Charles E. Goodell, received more than 60% of the votes cast but split this share in such a way that the conservative, James R. Buckley, won the election with just 39% of the vote. Both Ottinger, who ended up with 37% of the vote, and Goodell, with 24% of the vote, were credible candidates—the former was the nominee of the Democratic Party, while the latter was the incumbent, having been appointed to fill the vacancy left by Robert Kennedy's assassination, and was nominated by both the Republican and Liberal parties. The liberal majority in the electorate was unable to coordinate its support behind one of the two liberals in the race, and the result was a conservative victory. Riker (1982a, 85) points to this race as an example of an inherent flaw in plurality rule, namely, the possible selection of a Condorcet loser. A Condorcet loser is a candidate like Buckley who, Riker argues, would lose to either of the liberal candidates in a paired majority decision. Election of such a candidate is thus the *worst* possible outcome in the sense that *any* other candidate would be preferred by a majority to a Condorcet loser. In the configuration of voter types presented in Table 1, candidate C is the plurality winner and also the Condorcet loser.

---

[7] These orderings are *ABC*, *ACB*, *BAC*, *BCA*, *CAB*, and *CBA*.

[8] Thus, we do not impose Palfrey's assumptions that all preference types are possible and that the distribution of preferences has no mass points.

[9] In the notation of Palfrey (1989), I am assuming that $F_{AB}(\cdot)$ is uniform.

I have thus far laid out the distribution (in a probabilistic sense) of voters in the electorate. I now turn to formalizing plurality elections as a game among the voters. Suppose that each voter simultaneously decides for which candidate to vote, taking other voters' actions as given and maximizing expected utility. Voters receive the utility value of the candidate who is elected. In case of a tie, assume that ties are broken alphabetically, that is, A beats B.[10] Implicit in this description of the available actions is the assumption that all voters cast a ballot; voters are not permitted to abstain.

Rather than assume that each voter has complete information about the preferences of other voters, suppose that voters possess incomplete information. They have only (probabilistic) beliefs about the distribution of preferences in the electorate. Specifically, assume that each voter knows his or her own preference and believes that the preference of others is independently and identically distributed according to the true distribution described in the earlier paragraph. This is the standard assumption of common priors in Bayesian games.

In this Bayesian framework, voters are allowed to choose actions that may depend on their type. Therefore, I describe voter α's strategies as (measurable) functions of the form $\sigma^{\alpha}: T \rightarrow \{A, B, C\}$. These strategies represent the possible beliefs of other voters about what voter α will do, depending on α's type. In order to emphasize the anonymity of mass elections, I consider only symmetric Bayesian equilibria. An equilibrium is symmetric if voters of identical type act identically. In other words, voters subscribe to a common function σ in equilibrium. This common, symmetric strategy is, in the language of Bayesian games, common knowledge.

In order to analyze the symmetric Bayesian equilibria in the game, I must characterize the best response of a voter if the other voters, distributed according to P, all use a strategy σ. Given the symmetric nature of the strategy, each voter faces an identical problem posed by the other $n - 1$ voters, in the sense that the distribution of votes (of the other $n - 1$ voters) is a repeated trinomial experiment, which is determined by σ and independent of α. Given a symmetric strategy σ, the type space T can be partitioned into sets D_A, D_B, and D_C, such that D_k represents the types who cast ballots for candidate k. Formally,

$$D_k = \{t \in T: \sigma(t) = k\}.$$

In other words, $D_k = \sigma^{-1}(k)$. The probability that a randomly selected other voter will vote for candidate A, B, or C is denoted by π_A, π_B, or π_C, respectively. These values are defined by

$$\pi_k = \text{Prob.}\,[t \in T \,|\, \sigma(t) = k] = \int_{\sigma^{-1}(k)} dP(t) = \int_{D_k} dP(t).$$

Voters then use these probabilities to calculate the probability of any given distribution of votes. Specifically, the probability that candidate A gets (exactly) a votes, candidate B gets (exactly) b votes, and candidate C gets (exactly) c votes, out of the $n - 1$ other voters, is $f(a, b, c)$, a trinomial probability with formula

$$f(a, b, c) = \frac{(n-1)!}{a!b!c!}\pi_A^a \pi_B^b \pi_C^c, \tag{1}$$

where a, b, and c are nonnegative integers satisfying $a + b + c = n - 1$.

In addition to restricting our attention to symmetric equilibria, I also rule out any equilibria that involve voters using weakly dominated strategies. Voting for a particular candidate is weakly dominated if voting for some other candidate would sometimes be better for the voter and never worse. For example, an AB-v voter should never cast a ballot for C; some of the time a vote for A may make a difference in electing A, but it never will make the voter worse off than choosing C. In fact, the following is easily shown to be true:

LEMMA 1. *In any equilibria that do not involve weakly dominated strategies, all the type* C *voters cast ballots for* C, *and the other two types of voters never vote for candidate* C.

Thus, type C voters always vote sincerely in the model. This lemma implies that in any equilibrium, $\pi_C = q_C = .4$.

Although the C type voters always vote sincerely, the other voters may choose to vote for their second choice in order to avoid "wasting" their vote. The conditions under which such strategic voting will occur are presented in the following lemma, which is due to Palfrey (1989, 75).

LEMMA 2. *In an equilibrium involving probabilities* π_A, π_B, π_C *and* n *voters, an* AB-v *type voter's best response is:*

1. *vote for* A *if* $(1 - v)p_{AB}^n + p_{AC}^n > vp_{CB}^n$;
2. *vote for* B *if* $(1 - v)p_{AB}^n + p_{AC}^n < vp_{CB}^n$; *and*
3. *vote for either* A *or* B *if equality holds.*

A BA-v *type voter's best response is:*

1. *vote for* B *if* $(1 - v)p_{AB}^n + p_{CB}^n > vp_{AC}^n$;
2. *vote for* A *if* $(1 - v)p_{AB}^n + p_{CB}^n < vp_{AC}^n$; *and*
3. *vote for either* B *or* A *if equality holds,*

where

p_{AB}^n = *probability that voting for* A *yields* A, *but voting for* B *yields* B;
p_{AC}^n = *probability that voting for* A *yields* A, *but voting for* B *yields* C; *and*
p_{CB}^n = *probability that voting for* A *yields* C *but voting for* B *yields* B.

The p_{ij}^n probabilities are the likelihood that the voter in question is pivotal between candidates i and j. In other words, p_{ij}^n represents the probability that the ballot cast by a voter determines whether *i* or *j* wins the election. Clearly, these pivot probabilities depend on the likelihood that a randomly sampled other voter will vote for a particular candidate (π_A, π_B, and π_C) and on the size of the electorate, *n*. Note, however, that because of the way ties are broken, the pivot probabilities are not simply the likelihood that two particular candidates tie for the most votes. Instead, these probabilities are slightly more complicated. For example, p_{AC}^n is the probability that *a* voters vote for *A*, *b* voters vote for *B*, and *c* voters vote for *C*, out of the other $n - 1$ voters, where $b < a$ and $a = c - 1$. In the following trinomial formulas, we denote such an event with notation such as $\{b < a = c - 1\}_n$ for the set of nonnegative integers (a, b, c) satisfying $b < a = c - 1$ and $a + b + c = n - 1$. We thus have:

---

[10] The results in this paper do not rely on the particular choice of a tiebreaking rule. The rule with which I work is computationally easiest.

$$p_{AC}^n = \text{probability of } \{b < a = c - 1\}_n$$

$$= \sum_{\{b<a=c-1\}_n} f(a, b, c)$$

$$= \sum_{k=\left[\frac{n-1}{3}\right]}^{\left[\frac{n-1}{2}\right]-1} \frac{(n-1)!}{k!(k+1)!(n-2k-2)!} \cdot (\pi_A)^k(\pi_B)^{n-2k-2}(\pi_C)^{k+1},$$

where $[z]$ is the smallest integer not less than *z*.

$$p_{CB}^n = \text{probability of } \{a < b = c - 1\}_n$$

$$= \sum_{\{a<b=c-1\}_n} f(a, b, c)$$

$$= \sum_{k=\left[\frac{n-1}{3}\right]}^{\left[\frac{n-1}{2}\right]-1} \frac{(n-1)!}{k!(k+1)!(n-2k-2)!} \cdot (\pi_A)^{n-2k-2}(\pi_B)^k(\pi_C)^{k+1}.$$

Note for later use that p_{CB}^n can be expressed as $\sum_{\{b<a=c-1\}_n} f(b, a, c)$. Also,

$$p_{AB}^n = \text{probability of } \{a = b \geq c - 1\}_n + \text{probability of } \{a = b - 1 \geq c - 1\}_n$$

$$= \sum_{\{a=b\geq c-1\}_n} f(a, b, c) + \sum_{\{a=b-1\geq c-1\}_n} f(a, b, c)$$

$$= \sum_{k=\left[\frac{n+1}{3}\right]-1}^{\left[\frac{n}{2}\right]-1} \frac{(n-1)!}{k!k!(n-2k-1)!} \cdot (\pi_A)^k(\pi_B)^k(\pi_C)^{n-2k-1}$$

$$+ \sum_{k=\left[\frac{n}{3}\right]-1}^{\left[\frac{n-1}{2}\right]-1} \frac{(n-1)!}{k!(k+1)!(n-2k-2)!} \cdot (\pi_A)^k(\pi_B)^{k+1}(\pi_C)^{n-2k-2}.$$

Now, if σ is an equilibrium, then it must be the case that all voters are using a best response to σ. This implies that an equilibrium of the model can be characterized by two values, v_{AB}^* and v_{BA}^*, known as cutpoints. These cutpoints represent the (maximum) level of utility that a voter must have in order to vote sincerely. Thus, if a voter has utility $v > v_{ij}^*$ for her second choice, then that voter will vote strategically for her second choice, candidate *j*. Note that if $v_{ij}^* = 1$, then all type *ij* voters will vote sincerely in equilibrium. It is sufficient to characterize equilibria by cutpoints for only type *AB* and *BA* voters, as type *C* voters must use the weakly dominant strategy of voting for *C*, that is, $v_C^* = 1$.

Therefore the following condition must hold in equilibrium:

Condition 1. *In an equilibrium involving probabilities* π_A, π_B, π_C *and* n *voters, type* C *voters vote for* C, *and type* AB *and* BA *voters use the strategy "Vote for* i *if* $v < v_{ij}^*$ *and vote for* j *if* $v > v_{ij}^*$*," where*

$$v_{ij}^* = \min\left\{1, \frac{p_{ij}^n + p_{iC}^n}{p_{ij}^n + p_{Cj}^n}\right\}.$$

This is only a partial description of an equilibrium, however. I add to this a rational expectations condition common to Bayesian equilibria. Specifically, in a given equilibrium, the π's that are taken as given in the first condition are in fact generated by the distribution of voters and the strategies they use in that equilibrium. In this way, voters' beliefs about the actions of other voters (the π's) are self-fulfilling expectations, that is, the beliefs are not contradicted in equilibrium. Given the distributions of voter preferences I have defined, this rational expectations condition simplifies to

Condition 2. *In an equilibrium with* n *voters characterized by cutpoints* v_{AB}^* *and* v_{BA}^*, *the following must hold:*

$$\pi_A = q_{AB}v_{AB}^* + q_{BA}(1 - v_{BA}^*) = (.3 + \varepsilon)v_{AB}^* + (.3 - \varepsilon)(1 - v_{BA}^*),$$

$$\pi_B = q_{BA}v_{BA}^* + q_{AB}(1 - v_{AB}^*) = (.3 - \varepsilon)v_{BA}^* + (.3 + \varepsilon)(1 - v_{AB}^*),$$

and $\pi_C = q_C v_C^* = .4.$

These two conditions must be met in any equilibrium in this model. It is natural to ask what such equilibria look like, if they exist. Palfrey (1989) develops a (partial) answer. First, any single-party equilibrium, in which only one candidate receives votes, must involve some voters employing weakly dominated strategies. Thus, I rule out any such equilibria in the model. Second, Palfrey shows that multiple two-party equilibria exist as the number of voters becomes large. There are two such equilibria in the model, one in which candidates *A* and *C* receive all the votes cast and one in which only candidates *B* and *C* receive votes. I conclude this section by stating Palfrey's theorem dealing with these two-party equilibria. In the next section, I discuss the existence of three-party equilibria in the model. Finally, in a later section I address the problem of coordinating on one of the two possible two-party equilibria.

It turns out that the key element in the voter's decision calculus is the relative likelihood of a tie between the leading and second-place candidate versus a tie between the trailing candidate and one of the other candidates. Specifically, Palfrey proves the following lemma:

LEMMA 3 (PALFREY). *Assume* $\pi_A < \min\{\pi_B, \pi_C\}$. *Then* $\lim_{n\to\infty}(p^n_{AC}/p^n_{CB}) = 0$ *and* $\lim_{n\to\infty}(p^n_{AB}/p^n_{CB}) = 0$.

This lemma is used to prove the following theorem, applicable to my model:

THEOREM 1 (PALFREY). *There does not exist a sequence of equilibria* $\{v^*_n\}^\infty_{n=1}$ *such that* $0 < \pi^*_A < \min\{\pi^*_B, \pi^*_C\}$, *where* $\pi^*_j = \lim_{n\to\infty}\pi_j(v^*_n)$, $j = A, B, C$ *and* $\pi_j(v^*_n)$ *is given by condition 2.*

This theorem states that a trailing candidate's vote share must erode away to nothing as the size of the electorate grows, if all voters vote strategically.

EXISTENCE OF NON-DUVERGERIAN EQUILIBRIA

In the previous section, I developed a model that admitted two equilibria supportive of Duverger's Law. In these equilibria, the "wasted vote" phenomenon led to some voters voting strategically for their second choice in the election. As a result, only two candidates received votes in the limit of these equilibria. In this section, I demonstrate the existence of equilibria that do not support Duverger's Law. I will show that for a range of ε, there exist three-party equilibria, and these equilibria persist as the number of voters grows without bound. The latter statement is important because the two-party equilibria identified in the previous section are truly two-party only in the limit. In any large, finite electorate—that is, $n < \infty$—these equilibria still contain a small number of people voting for the trailing candidate. This follows from the fact that in any finite electorate the probability of a tie between the last-place candidate and the leading candidate is still positive, albeit very small. Thus, voters who prefer the trailing candidate with sufficiently small utility for the second-place candidate will still vote sincerely.

As suggested by Myerson and Weber (1993), I will examine the situation described as an "exceptional case" by Palfrey (1989, 82), namely, the case in which $\pi^*_A = \pi^*_B < \pi^*_C$. In this situation, one dominant candidate emerges, opposed by two smaller parties. As the two smaller parties receive (nearly) equal vote shares, supporters of either are unwilling to abandon their most preferred candidate; there is no single trailing candidate for voters to abandon, à la Duverger's Law. This was apparently the case in the 1970 New York race for the U.S. Senate seat, in which neither Ottinger nor Goodell could muster enough support to emerge as the single viable liberal alternative to the conservative Buckley.

In the non-Duvergerian equilibrium I identify, equality of vote shares, $\pi^*_A = \pi^*_B$, holds only in the limit as n goes to infinity. In a finite electorate, the following happens in equilibrium: The more popular candidate, A, receives a slightly smaller vote share than B. This prompts the small fraction of AB voters with very high utilities for candidate B to vote strategically for B. Thus, even though there is a larger distribution of AB type voters than BA types, the lower vote share for A is self-fulfilling. As the size of the electorate grows, the vote share for candidate A increases, while the vote share for B decreases. In fact, the shares for both candidates approach .3. In the limit, the candidates receive equal vote shares, and thus a three-party equilibrium is sustained. This argument is laid out formally in the rest of this section.

I will make use of a lemma first proved by Cox (1987, 33).

LEMMA 4 (COX). *Suppose* $\pi_A > \pi_B$. *Then*

$$f(a, b, c) > f(b, a, c) \quad \textit{iff} \quad a > b,$$

where $f(a, b, c)$ *is defined by 1.*

Proof: From the definition of f, we have

$$f(a, b, c) > f(b, a, c) \quad \text{iff}$$

$$\pi^a_A\pi^b_B > \pi^b_A\pi^a_B \quad \text{iff} \quad \pi^{(a-b)}_A > \pi^{(a-b)}_B.$$

As $\pi_A > \pi_B$,

$$\pi^{(a-b)}_A > \pi^{(a-b)}_B \quad \text{iff} \quad a - b > 0 \quad \text{iff} \quad a > b.$$

Q.E.D.

In order to show the existence of a non-Duvergerian equilibrium in the model, I must identify values v^*_{AB} and v^*_{BA} that satisfy the conditions for equilibria given in the previous section. One element of the preceding discussion is that these values must vary for different values of n. Luckily, we do not have to consider every combination of v_{AB} and v_{BA} for every possible n. To show this, I establish two lemmas. They both involve a critical value $\tilde{v} = .3/(.3 + \varepsilon)$. If $v_{BA} = 1$, then $\tilde{v}$ is the value of v_{AB} which insures that the probability of a vote for candidate A and candidate B is equal, that is, $\pi_A = \pi_B$.[11] The first lemma deals with the possibility of equilibria involving AB cutpoints greater than $\tilde{v}$.

LEMMA 5. *In equilibrium, it cannot be the case that* $\tilde{v} < v^*_{AB} < 1$, *where* $\tilde{v} = .3/(.3 + \varepsilon)$.

Proof: If $\tilde{v} < v^*_{AB} < 1$, then by condition 2, $\pi_A > .3$. Thus, it must be that $\pi_A > \pi_B$. By lemma 4, if $a > b$, then $f(a, b, c) > f(b, a, c)$. Therefore,

$$\sum_{\{b<a=c-1\}_n} f(a, b, c) > \sum_{\{b<a=c-1\}_n} f(b, a, c),$$

---

[11] This follows immediately from condition 2.

and thus $p^n_{AC} > p^n_{CB}$. By condition 1, however, v^*_{AB} must satisfy

$$v^*_{AB} = \min\left\{1, \frac{p^n_{AB} + p^n_{AC}}{p^n_{AB} + p^n_{CB}}\right\}.$$

Thus, $v^*_{AB} = 1$ in equilibrium, which is a contradiction. *Q.E.D.*

This lemma does not rule out an equilibrium with $v^*_{AB} = 1$. Such an equilibrium would have $\pi_A > \pi_B$ and $\pi_C > \pi_B$ for all n. Thus, this equilibrium is one of the Duvergerian equilibria accounted for in theorem 1. In order to identify a non-Duvergerian equilibrium, we must focus on equilibria with $v^*_{AB} < \tilde{v}$. The following lemma shows that any equilibria in this region must involve *BA* type voters voting sincerely.

LEMMA 6. *If* $v^*_{AB} < \tilde{v}$ *holds in some equilibrium with* n *voters, then* $v^*_{BA} = 1$.

Proof: Follows immediately from condition 1 and the supposition that $v^*_{AB} < 1$. *Q.E.D.*

This lemma allows us to ignore v^*_{BA} in what follows and focus on the cutpoint for *AB* type voters, v_{AB}, which I denote as v. In order to emphasize the dependence of this cutpoint on the size of the electorate, n, I use the notation $v(n)$ or v_n in what follows. I denote an equilibrium cutpoint for *AB* type voters in an electorate of size n as v^*_n. As an additional convenience, I define a "best response" function $h_n(v)$ that summarizes the two conditions for equilibria with n voters. Specifically, if a value $v_{AB} = v < \tilde{v}$ is an equilibrium, condition 2 requires that $\pi_A(v) = (.3 + \varepsilon)v$, as $v^*_{BA} = 1$. It follows that $\pi_B(v) = .6 - \pi_A(v)$, as $\pi_C = .4$. Thus, by condition 1, I define the best response to v as

$$h_n(v) = \min\left\{1, \frac{p^n_{AB}(\pi_A(v)) + p^n_{AC}(\pi_A(v))}{p^n_{AB}(\pi_A(v)) + p^n_{CB}(\pi_A(v))}\right\}.$$

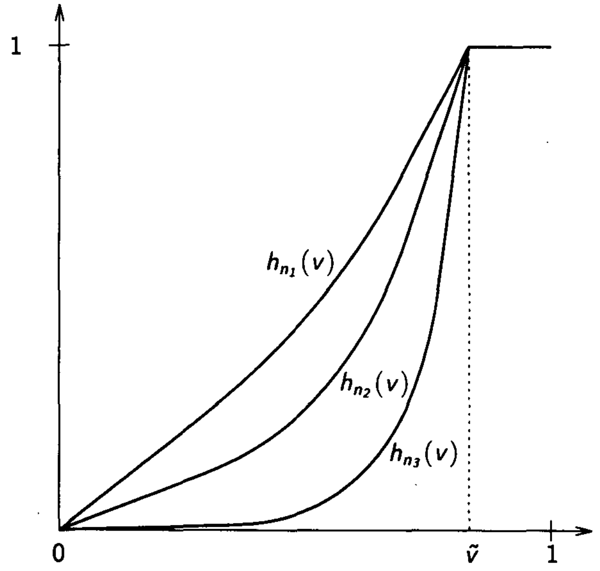
It is immediately apparent that $h_n(v)$ is continuous, $h_n(v) = 1$ for $v > \tilde{v}$, and $h_n(v)$ is differentiable everywhere except $\tilde{v}$. It follows from the definition that v is an equilibrium if and only if v is a fixed point of $h_n(v)$, that is, $h_n(v) = v$. For any n, $v = 0$ is a fixed point of $h_n(v)$. This corresponds to the two-party equilibria in which all voters abandon candidate A. I now proceed to show that there is also a three-party equilibrium in the model. I shall use the following lemma:

LEMMA 7. *For any fixed* $v < \tilde{v}$, $\lim_{n\to\infty} h_n(v) = 0$.

Proof: The definition of $h_n(v)$ can be rewritten as

$$h_n(v) = \min\left\{1, \frac{\dfrac{p^n_{AB}(\pi_A(v))}{p^n_{CB}(\pi_A(v))} + \dfrac{p^n_{AC}(\pi_A(v))}{p^n_{CB}(\pi_A(v))}}{\dfrac{p^n_{AB}(\pi_A(v))}{p^n_{CB}(\pi_A(v))} + 1}\right\}.$$

FIGURE 1. The Best Response Function $h_n(v)$ as the Size of the Electorate Grows



From lemma 3 it follows that $\lim_{n\to\infty} h_n(v) = 0$. *Q.E.D.*

I illustrate the implication of these facts about $h_n(v)$ in Figure 1, which graphs $h_n(v)$ for three different values of n, $n_1 < n_2 < n_3$. As illustrated, the function moves closer to the horizontal axis and the vertical dotted line at $\tilde{v}$ as n grows larger.

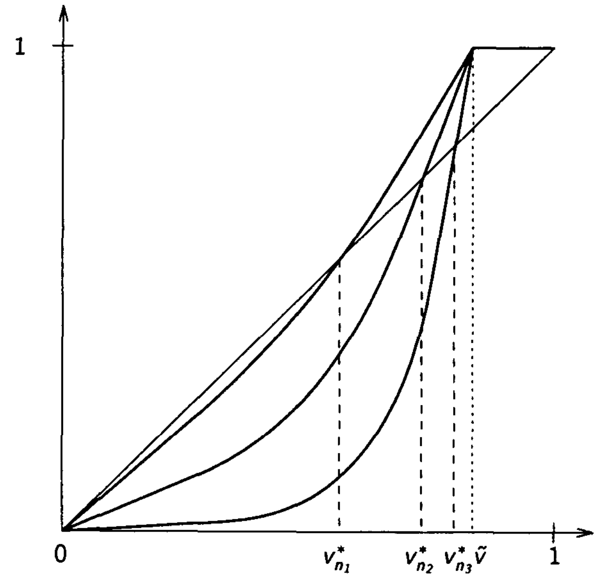
I next prove the main result of this section on the existence of a three-party equilibrium in the model. This theorem establishes that non-Duvergerian equilibria are possible under a range of preference distributions. Thus, the equilibria in the model are not generically Duvergerian.

THEOREM 2. *There exists a sequence of equilibria* $\{v^*_n\}^\infty_{n=1}$ *such that* $0 < \pi^*_A = \pi^*_B < \pi^*_C$, *where* $\pi^*_j = \lim_{n\to\infty} \pi_j(v^*_n)$, $j = A, B, C$ *and* $\pi_j(v^*_n)$ *is given by condition 2.*

Proof: For each n, let F_n be the set of fixed points of $h_n(v)$. As $0 \in F_n$ for all n, F_n is nonempty. Fix $v < \tilde{v}$. Lemma 7 is equivalent to the statement that for any $\delta > 0$, there exists an integer N_δ such that for all $n \geq N_\delta$, $h_n(v) < \delta$. As h_n is continuous and $h_n(v) = 1 > v$ for $1 > v > \tilde{v}$, this implies that for sufficiently large n, each F_n contains a fixed point between v and $\tilde{v}$. More specifically, for any $v < \tilde{v}$, there exists an integer N_v such that $\forall n \geq N_v$, $(v, \tilde{v}) \cap F_n$ is nonempty. Thus, it is possible to choose a sequence of fixed points that converge to $\tilde{v}$. Denote this convergent sequence as $\{v^*_n\}^\infty_{n=1}$. This sequence fulfills the requirements of the theorem. *Q.E.D.*

This theorem is illustrated in Figure 2. For each n, there is an equilibrium v^*_n satisfying $v^*_n = h_n(v^*_n)$. Three such values are shown in the figure, which also illustrates that as n grows, the shape of the h_n function forces v^*_n to converge to $\tilde{v}$.

FIGURE 2. Sequence of Equilibria Converging to $\tilde{v}$



STABILITY AND DYNAMICS OF NON-DUVERGERIAN EQUILIBRIA

The existence of a non-Duvergerian, three-party equilibrium in the model was demonstrated in the last section. This equilibrium is susceptible to two types of criticism. The first is that it requires widespread, accurate information unlikely to be present in a real electorate. The share of *AB* voters who vote strategically for candidate *B* in equilibrium depends critically on the size of the electorate, *n*, and the exact proportion of *AB* voters in the population, $0.3 + \varepsilon$. The larger is *n*, the more *AB* type voters who are "allowed" by the equilibrium to vote sincerely. While I have assumed that these values are common knowledge among the voters, it is highly doubtful that such an assumption would be justified in a real electorate. This feature is particularly striking as the other, two-party equilibria of the model do not depend critically on these values. Even with some disagreement or uncertainty among the electorate about the parameters, either two-party equilibrium could be sustained. These equilibria require only that voters agree about which candidate is trailing. In the non-Duvergerian equilibrium, however, uncertainty could cause some voters to vote "incorrectly" (not as prescribed by the equilibrium). Will the political process restore the delicate balance required by the equilibrium? This question raises the issue of stability of an equilibrium, which is a major focus of this section.

The other criticism of the three-party equilibrium in the model also applies to the Duvergerian equilibria. As Cox (1991, 12) points out, all the equilibria identified thus far require voters to overcome a very serious coordination problem. The "fulfilled expectations" assumption acts to sidestep this issue. For example, the non-Duvergerian equilibrium requires that just the right number of *AB* types vote for their second choice, even though they outnumber the *BA* types; a number that ensures candidate *A* trails *B* by just enough to justify the strategic voting. Likewise, the Duvergerian equilibria, in which either candidate *A* or *B* gets no votes, require that voters all know, without communication, which is the viable candidate who will receive all the votes. Cox likens this issue to the coordination problem posed by the Battle of the Sexes game and comments that "these models should both be interpreted as showing that, *if coordination problems can be overcome*, then all equilibria in multicandidate plurality elections entail that only two candidates will receive any support" (1991, 13, emphasis in original). While this qualifier is certainly necessary, my analysis will have a great deal to say about overcoming the problem of coordination in multicandidate elections.

The root cause of both weaknesses lies in the use of a static model to describe a dynamic process. As Palfrey puts it, "the results in this essay have been interpreted as implying that if three parties are competing in a single-ballot, winner-take-all system, *eventually* (i.e., in an equilibrium) one of the parties will be weeded out. However, the model is static, even though the interpretation has dynamic overtones" (1989, 85, emphasis added). Indeed, the reasoning Duverger offers for his law is a dynamic story in which voters, over time, gradually abandon an unpopular party in larger and larger numbers until no support remains. The static "one-shot" noncooperative game we have used to model the main elements of this story cannot fully describe the dynamics involved.

Both of these objections arise with the model because voters acquire information, either about the parameters of the electorate or the viability of candidates, without an explanation for how this (self-fulfilling) information occurs in the population. Both Palfrey and Myerson and Weber cite public opinion polls, past election outcomes, and media reports, among others, as sources of the equilibrium information of voters. These sources may depend on earlier polls, which may depend on even earlier polls, and so forth. With this interpretation, an equilibrium occurs when the most recent poll provides the electorate with enough information to act in a way that justifies the predictions of the poll. Yet, neither paper explicitly models the dynamic process of preelection polling or considers the stability of the resulting equilibrium. It is this task to which I now turn.

In the analysis, I will use the function $h_n(v)$ defined in the previous section to describe an equilibrium in terms of fixed points. Here, think of $h_n(v)$ as describing an adjustment process that gives a best response for an individual *AB* type voter to the shared belief about the current cutpoint used by the *AB* types as a group.[12] This description is based on the following process. Let $v_0 < \tilde{v}$ be some initial cutpoint used by *AB* type voters. Suppose all begin by using v_0 in making their vote decision. Next, suppose that a public opinion poll is taken to measure voting intention, and the results are publicly announced. Then, given the cutpoints in use, the results should be, on average, that candidate A receives a fraction $q_{AB}v_0$ of the vote, candidate B gets $q_{BA} + q_{AB}(1 - v_0)$ of the vote, and C receives support from $q_C = .4$ of the voters. Suppose voters interpret these results as the probabilities that a randomly selected other voter will vote for a particular candidate, which I have called π_A, π_B, and π_C. Then, the best response under these expectations is to use a cutpoint v_1 satisfying

---

[12] As was shown in the previous section, as long as $v < \tilde{v}$, the best response for *BA* types and *C* types is to vote sincerely. Thus, I once again focus attention on *AB* types and suppose that the other types all vote sincerely.

$$v_1 = h_n(v_0) = \min\left\{1, \frac{p^n_{AB}(\pi_A(v_0)) + p^n_{AC}(\pi_A(v_0))}{p^n_{AB}(\pi_A(v_0)) + p^n_{CB}(\pi_A(v_0))}\right\}.$$

Suppose that there is a second opinion poll. Using the cutpoint v_1, voters' responses once again are announced, and voters use these results to update their beliefs about the probabilities of votes from other voters. This gives rise to a new cutpoint, $v_2 = h_n(v_1)$. If this process continues with a large number of polls, the convergence properties of the cutpoints used by the voters can be evaluated, where these cutpoints are given by the sequence $v_{t+1} = h_n(v_t)$.

This approach also can be used to determine the stability of the various equilibria in the model. Suppose that voters initially use cutpoints that differ slightly from those prescribed by the equilibrium. If the polling process just described leads voters to adjust their behavior so as to be more in accordance with the equilibrium, the equilibrium can be termed stable. If the polling process causes voters to use cutpoints that differ even more widely from the equilibrium cutpoints, the equilibrium can be termed unstable.

This concept of equilibrium stability is captured in a definition developed by Palfrey and Rosenthal (1991) for a similar problem. Their definition, in the context of my model, is the following:

DEFINITION 1. *An equilibrium* v^* *is:*

- *an expectationally stable equilibrium (ESE) if there exists an open interval* $I(v^*)$ *containing* v^* *such that, for all* $v \in I(v^*)$, $[v - h_n(v)](v - v^*) > 0$ *if* $v \neq v^*$;
- *a globally expectationally stable equilibrium (GESE) if it is an ESE relative to the open interval* $(0, \tilde{v})$.
- *an expectationally unstable equilibrium (EUE) if there exists an open interval* $I(v^*)$ *containing* v^* *such that, for all* $v \in I(v^*)$, $[v - h_n(v)](v - v^*) < 0$ *if* $v \neq v^*$.
- *a globally expectationally unstable equilibrium (GEUE) if it is an EUE relative to the open interval* $(0, \tilde{v})$.

Note that this definition of an EUE is a stronger statement about the dynamic process than that v^* is *not* an ESE. An EUE involves a dynamic process which moves cutpoints farther away from the equilibrium cutpoints, as opposed to a process which does not move the cutpoints closer.

In order to evaluate the stability of my equilibrium, I need the following lemma:

LEMMA 8. *For the sequence of equilibria* v^*_n *described by Theorem 2,* $\lim_{n\to\infty} d/dv\, h_n(v^*_n) = \infty$.

Proof: Differentiating h_n yields

$$\frac{d}{dv}h_n(v)$$

$$= \frac{(p^n_{AB} + p^n_{CB})\frac{d}{dv}(p^n_{AB} + p^n_{AC}) - (p^n_{AB} + p^n_{AC})\frac{d}{dv}(p^n_{AB} + p^n_{CB})}{(p^n_{AB} + p^n_{CB})^2}.$$

Since $h_n(v^*_n) = v^*_n$ at an equilibrium v^*_n, this expression can be rewritten as

$$\frac{d}{dv}h_n(v^*_n)$$

$$= \frac{\frac{d}{dv}(p^n_{AB} + p^n_{AC})(v^*_n) - v^*_n\frac{d}{dv}(p^n_{AB} + p^n_{CB})(v^*_n)}{p^n_{AB}(v^*_n) + p^n_{CB}(v^*_n)}.$$

Thus, the derivative of each p^n_{ij} is needed.

$$\frac{d}{dv}p^n_{AC} = \frac{d}{dv}\pi_A(v)\frac{d}{d\pi_A}p^n_{AC}$$

$$= (q_{AB})\left(\sum_{k=\left[\frac{n-1}{3}\right]}^{\left[\frac{n-1}{2}\right]-1} f(k, n-2k-2, k+1)\right.$$

$$\left.\cdot\left[\frac{k}{\pi_A} - \frac{n-2k-2}{\pi_B}\right]\right)$$

$$= q_{AB}p^n_{AC}\sum_{k=\left[\frac{n-1}{3}\right]}^{\left[\frac{n-1}{2}\right]-1}\frac{f(k, n-2k-2, k+1)}{p^n_{AC}}$$

$$\cdot\left[k\left(\frac{1}{\pi_A} + \frac{2}{\pi_B}\right) - \frac{(n-2)}{\pi_B}\right]$$

$$= q_{AB}p^n_{AC}\left[\left(\frac{1}{\pi_A} + \frac{2}{\pi_B}\right)\sum_{k=\left[\frac{n-1}{3}\right]}^{\left[\frac{n-1}{2}\right]-1}\right.$$

$$\cdot k\frac{f(k, n-2k-2, k+1)}{p^n_{AC}}$$

$$\left.-\frac{(n-2)}{\pi_B}\cdot\frac{\sum_{k=\left[\frac{n-1}{3}\right]}^{\left[\frac{n-1}{2}\right]-1} f(k, n-2k-2, k+1)}{p^n_{AC}}\right]$$

$$= q_{AB}p^n_{AC}\left[\left(\frac{1}{\pi_A} + \frac{2}{\pi_B}\right)(nE^n_{AC}) - \frac{(n-2)}{\pi_B}(1)\right],$$

where

$$E^n_{AC} = \frac{1}{n}\sum_{k=\left[\frac{n-1}{3}\right]}^{\left[\frac{n-1}{2}\right]-1} k\frac{f(k, n-2k-2, k+1)}{p^n_{AC}}.$$

Likewise, it can be shown that

$$\frac{d}{dv} p^n_{CB} = q_{AB} p^n_{CB}\left[\frac{(n-2)}{\pi_A} - \left(\frac{1}{\pi_B} + \frac{2}{\pi_A}\right)(nE^n_{BC})\right],$$

where

$$E^n_{BC} = \frac{1}{n} \sum_{k=\left[\frac{n-1}{3}\right]}^{\left[\frac{n-1}{2}\right]-1} k \frac{f(n-2k-2, k, k+1)}{p^n_{CB}},$$

and

$$\frac{d}{dv} p^n_{AB} = q_{AB}\left[p^n_{AB}\left(\frac{1}{\pi_A} - \frac{1}{\pi_B}\right)(nE^n_{AB}) - \frac{\sum_{k=\left[\frac{n}{3}\right]-1}^{\left[\frac{n-1}{2}\right]-1} f(k, k+1, n-2k-2)}{\pi_B}\right],$$

where

$$E^n_{AB} = \frac{1}{n}\left(\sum_{k=\left[\frac{n+1}{3}\right]-1}^{\left[\frac{n}{2}\right]-1} k \frac{f(k, k, n-2k-1)}{p^n_{AB}} + \left(\sum_{k=\left[\frac{n}{3}\right]-1}^{\left[\frac{n-1}{2}\right]-1} k \frac{f(k, k+1, n-2k-2)}{p^n_{AB}}\right)\right).$$

The E^n_{ij} terms represent the expected winning shares of the two candidates who receive the most votes, conditional on there being a tie for first place between candidates i and j,[13] and Palfrey (1989, 87–9) has shown that these terms have the following limits:

$$\lim_{n\to\infty} E^n_{AC} = \max\left\{\frac{1}{3}, \frac{1}{2 + \frac{\pi_B}{\sqrt{\pi_A \pi_C}}}\right\}$$

$$\lim_{n\to\infty} E^n_{CB} = \frac{1}{2 + \frac{\pi_A}{\sqrt{\pi_B \pi_C}}}$$

$$\lim_{n\to\infty} E^n_{AB} = \frac{1}{3}.$$

For sufficiently large n, v^*_n is close to $\bar{v}$, and thus $\lim_{n\to\infty} E^n_{AC} = 1/(2 + \pi_B/\sqrt{\pi_A \pi_C})$.

Returning to $h_n(v)$ and substituting the derivatives gives:

$$\frac{d}{dv} h_n(v^*_n)$$

$$= q_{AB} n \frac{p^n_{AB}\left(\frac{1}{\pi_A} - \frac{1}{\pi_B}\right)E^n_{AB} + p^n_{AC}\left[\left(\frac{1}{\pi_A} + \frac{2}{\pi_B}\right)E^n_{AC} - \frac{1}{\pi_B}\right]}{p^n_{AB}(v^*_n) + p^n_{CB}(v^*_n)}$$

$$- v^*_n q_{AB} n \frac{p^n_{AB}\left(\frac{1}{\pi_A} - \frac{1}{\pi_B}\right)E^n_{AB} + p^n_{CB}\left[\frac{1}{\pi_A} - \left(\frac{1}{\pi_B} + \frac{2}{\pi_A}\right)E^n_{CB}\right]}{p^n_{AB}(v^*_n) + p^n_{CB}(v^*_n)}$$

$$+ q_{AB} \frac{(1 - v^*_n) \frac{\sum_{k=\left[\frac{n}{3}\right]-1}^{\left[\frac{n-1}{2}\right]-1} f(k, k+1, n-2k-2)}{\pi_B} + \frac{2p^n_{AC}}{\pi_B} + \frac{2p^n_{CB}}{\pi_A}}{p^n_{AB}(v^*_n) + p^n_{CB}(v^*_n)}$$

$$= q_{AB} n \frac{\frac{p^n_{AB}}{p^n_{CB}}\left(\frac{1}{\pi_A} - \frac{1}{\pi_B}\right)E^n_{AB} + \frac{p^n_{AC}}{p^n_{CB}}\left[\left(\frac{1}{\pi_A} + \frac{2}{\pi_B}\right)E^n_{AC} - \frac{1}{\pi_B}\right]}{(p^n_{AB}(v^*_n)/p^n_{CB}(v^*_n)) + 1}$$

$$- v^*_n q_{AB} n \frac{\frac{p^n_{AB}}{p^n_{CB}}\left(\frac{1}{\pi_A} - \frac{1}{\pi_B}\right)E^n_{AB} + (1)\left[\frac{1}{\pi_A} - \left(\frac{1}{\pi_B} + \frac{2}{\pi_A}\right)E^n_{CB}\right]}{(p^n_{AB}(v^*_n)/p^n_{CB}(v^*_n)) + 1}$$

$$+ \frac{O(1)}{(p^n_{AB}(v^*_n)/p^n_{CB}(v^*_n)) + 1},$$

where I use the notation $O(1)$ to stand for terms that are bounded as n grows without bound.

Applying lemma 3 and condition 2 and using the fact that all the E^n_{ij} terms have constant limits yields

$$\frac{d}{dv} h_n(v^*_n) = q_{AB} n \frac{(o(1) \cdot O(1)) + (o(1) \cdot O(1))}{o(1) + 1}$$

$$- \pi_A(v^*_n) n \frac{(o(1) \cdot O(1)) + \left[\frac{1}{\pi_A} - \left(\frac{1}{\pi_B} + \frac{2}{\pi_A}\right)E^n_{CB}\right]}{o(1) + 1}$$

$$+ \frac{O(1)}{o(1) + 1}$$

$$= \frac{n\left(o(1) - \left[1 - \left(\frac{\pi_A}{\pi_B} + 2\right)E^n_{CB}\right]\right) + O(1)}{o(1) + 1},$$

where $o(1)$ stands for terms that go to zero as n grows without bound.

For sufficiently large n,

$$\frac{d}{dv} h_n(v^*_n)$$

$$\sim \frac{n\left[\left(2 + \frac{\pi_A}{\pi_B}\right)\left(\frac{1}{2 + \frac{\pi_A}{\sqrt{\pi_B \pi_C}}}\right) - 1 + o(1)\right] + O(1)}{o(1) + 1}.$$

As $2 + \pi_A/\pi_B > 2 + \pi_A/\sqrt{\pi_B \pi_C}$ when $\pi_B < \pi_C$ (true for sufficiently large n), we conclude that $\lim_{n\to\infty} d/dv\ h_n(v^*_n) = \infty$. *Q.E.D.*

---

[13] Actually, because of the tie-breaking rule, this is not exactly true. For example, E^n_{AC} is the expected vote share of A conditional on B coming in last and A receiving exactly one less vote than C.

This lemma is actually a stronger result than is necessary for the main result of this section. While all that is needed for the theorem is that $h_n'(v) > 1$, it should be noted that I actually proved $h_n'(v)$ is of order n. This should not be surprising, as we know that for any $v < \tilde{v}$, $h_n(v)$ is near zero for sufficiently large n, while $h_n(v) = 1$ for $v > \tilde{v}$.

The main result of this section is that the non-Duvergerian equilibrium in the model is expectationally unstable.

THEOREM 3. *For sufficiently large* n, *the equilibria* v_n^* *described by theorem 2 are expectationally unstable.*

Proof: I must show that there exists an integer N such that for all $n > N$, v_n^* is an EUE. By lemma 8, an N exists such that $d/dv\ h_n(v_n^*) > 1$. This implies that there exists an interval $I(v_n^*)$ containing v^* such that, for all $v \in I(v_n^*)$,

$$\frac{h_n(v) - h_n(v_n^*)}{v - v_n^*} > 1.$$

As v_n^* is an equilibrium, $h_n(v_n^*) = v_n^*$. Thus,

$$\frac{h_n(v) - v_n^*}{v - v_n^*} = \frac{h_n(v) - v + v - v_n^*}{v - v_n^*} > 1$$

$$\text{implies} \quad \frac{h_n(v) - v}{v - v_n^*} > 0.$$

I conclude that

$$\frac{v - h_n(v)}{v - v_n^*} < 0.$$

Thus, if $v \neq v_n^*$, $v - h_n(v)$ and $v - v_n^*$ have the opposite sign. So, for all $n > N$, $(v - h_n(v))(v - v_n^*) > 0$ for all $v \in I(v_n^*)$, such that $v \neq v_n^*$. Thus, v_n^* is an EUE. *Q.E.D.*

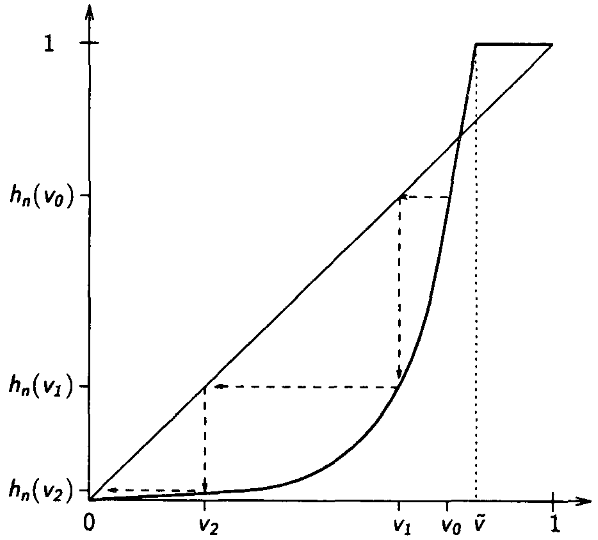
Therefore, if voters are not perfectly informed as to the "correct" equilibrium cutpoint, even by a minute amount, the polling process will move the cutpoints used by the voters away from the three-candidate equilibrium. Since real elections lack precise coordinated action among voters, it is implausible to believe that this non-Duvergerian equilibrium can be maintained in the presence of repeated public opinion polls.

In contrast, it is easy to see that the Duvergerian equilibria in the model are expectationally stable—even if some voters are voting "incorrectly," the process of responding to repeated polls eventually will coordinate all the voters on the "correct" viable candidate. I thus conclude that real-world elections with available public opinion polls are much more likely to correspond to Duvergerian outcomes than to non-Duvergerian outcomes.

GLOBAL STABILITY AND POLLS AS A COORDINATION DEVICE

In the last section, I showed that the non-Duvergerian equilibrium in the model is (locally) expectationally unstable. In this section, I show that the equilibrium is globally unstable, and the dynamic polling process must converge to one of the two Duvergerian equilibria, depending on the initial beliefs of voters. Thus, the polling process acts as a coordination device for the voters.

FIGURE 3. The Adjustment Path of Opinion Polls



These conclusions are justified by the following theorem:

THEOREM 4. *For sufficiently large* n, *the equilibria* v_n^* *described by theorem 2 are globally expectationally unstable.*

Proof: Note that it is sufficient to prove that for sufficiently large n, $h_n(v) < v$ for $0 < v < v_n^*$, and $h_n(v) > v$ for $v > v_n^*$. The second half of this claim is trivial. For the first half, suppose the result does not hold. Then, for some sequence $\{v_n\}_{n=N}^{\infty}$, $h_n(v_n) > v_n$ for $0 < v_n < v_n^*$. As $h_n(v)$ is continuous, this implies that for all $n > N$, there exists a fixed point of $h_n(v)$ at $0 < v < v_n^*$. Assume this sequence of fixed points converges; otherwise consider a convergent subsequence. If this sequence converges to $\tilde{v}$, this contradicts theorem 3, which applies to *any* sequence of fixed points of $h_n(v)$ converging to $\tilde{v}$. If it does not, it contradicts theorem 1, as no such three-party equilibrium can persist. *Q.E.D.*

This theorem implies that the "best response" function $h_n(v)$ has a form like that of Figure 3. Moreover, as the non-Duvergerian equilibrium is globally unstable, the dynamic polling process defined in the previous section must converge to one of the two Duvergerian equilibria. This is also illustrated in Figure 3. Suppose voters hold an initial belief, v_0, that is less than the equilibrium cutpoint v_n^*. Then, the theorem implies that the adjusted cutpoint $v_1 = h_n(v_0)$ will be less than v_0. Likewise, v_2 will be less than v_1, and, as the figure illustrates, eventually v_t must converge to zero. A similar argument shows that if v_0 is above the equilibrium cutpoint, v_t converges to one. Thus, the repeated cycles of opinion poll and adjustment lead voters to one of the Duvergerian equilibria.

In fact, this dynamic process can shed some light on overcoming the coordination problem faced by voters. Polls can guide voters to coordinate on a particular equilibrium, depending on their initial beliefs. In the previous section, the voters shared an initial belief, v_0, about the cutpoint used by the AB types, which was then adjusted to v_1 after the first poll, and so on. This is a strong assumption about the initial conditions of the polling process. Instead, I relax this assumption and suppose that each voter α has some initial belief, v_0^α, about v_{AB}.[14] When the first poll is taken, suppose each voter votes according to the initial belief, v_0^α. This poll will result in vote shares s_A, s_B, and s_C for the candidates, where these outcomes depend on the distribution of beliefs. After this first poll, suppose that voters adjust their beliefs that these shares represent the probabilities of a randomly selected other voter voting for a given candidate. Then, the polling process serves to aggregate the intentions of the voters and transmit this information about the viability of the candidates back to the electorate. If the polling reveals that candidate B is the serious challenger to C, then a bandwagon effect develops as more and more AB types switch their vote to B. Likewise, if A has the strongest initial support, a bandwagon develops for A at the expense of B. The global stability result in this section guarantees that, for *almost any* distribution of initial beliefs, this process will converge to one of the two Duvergerian outcomes.

In this way, polls can help voters coordinate on the viable candidates in the race, to ensure that their vote is not wasted. This coordination can be fully effective, however, only after an infinite number of polls. Thus, in actual elections we should not expect to see such dramatic results. In fact, if polls are not frequent enough or are ignored by voters, this process may have little effect on the actual outcome. But as polling becomes an even larger part of media coverage of elections, one should expect strategic voting and coordination effects to have an ever greater importance in politics.

CONCLUSION

This paper has focused on the "wasted vote" phenomenon and Duverger's Law. I have shown that in a Bayesian game model of strategic voting, there do exist non-Duvergerian equilibria in which all three candidates receive votes (in the limit). These equilibria require extreme coordination, however, and any variation in beliefs leads voters away from them to one of the Duvergerian equilibria. Thus, non-Duvergerian equilibria are unstable, while two-party equilibria are not.

In addition, I described how preelection polls provide information to voters about the viability of candidates and thus can be used by voters to coordinate on a Duvergerian outcome. While there is significant empirical support for these outcomes in plurality systems, this conclusion about voter behavior has not been extensively explored. Some suggestive results from the study of "momentum" and "winnowing" in presidential primaries (Aldrich 1980, Bartels 1988, Brady 1996) and strategic voting in recent presidential elections (Abramson et al. 1995) point to the importance of polls and early primary results in the behavior of voters. In addition, there has been experimental confirmation of this prediction. Forsythe et al. (1993) describe a set of plurality rule experiments that closely resemble the theoretical model developed here. They show that preelection polls significantly reduce the frequency with which the Condorcet loser is elected. With polls, voters are able to coordinate on one of the Duvergerian equilibria. While these results are promising, additional work remains to be done on this process in the lab, among real electorates, and in actual elections.

Finally, this work is certainly not the final word on the nature of Duverger's Law and preelection polls. Indeed, the limitations of this paper offer several possible avenues for future research, two of which I will mention here. First, I have addressed the question of stability of the non-Duvergerian equilibrium within a model similar to the example analyzed by Myerson and Weber. In particular, the model presented here has the feature that only one type of voter votes strategically in equilibrium. An obvious question to consider is whether my results on stability and coordination hold in a more complex model in which more than one type of voter is strategic in equilibrium. While this paper offers insight into the general case, the status of non-Duvergerian equilibria in the general model of Palfrey is an open question. I conjecture that *all* such non-Duvergerian equilibria are expectationally unstable.

Second, in this paper voters base their strategic voting on information obtained from the sequence of polls preceding the election. In these polls, however, voters do not explicitly consider how their response to the poll will affect the actions of other voters after the results are announced. Thus, voters are strategic in their poll responses only to the extent that their responses are best replies to previous information (and not just sincere reports of their most preferred candidate). They do not attempt to "manipulate" the sequence of polls in order to improve the chances of a favorable outcome in the final vote. Even though it seems unlikely that voters in a real campaign would attempt to manipulate poll results, especially given the severe coordination problem that would need to be overcome,[15] the question of what equilibria would arise in such a model is an interesting one. With no restrictions on the information or strategies of voters, the model would essentially be a model of voting preceded by repeated rounds of cheap talk. Thus, one would expect a wide range of equilibria to exist. Restricting the information of some voters, as in Fey (1995), also may be a productive mode of analysis. The answers to these questions should help us build a deeper understanding of the issues raised here.

---

[14] For completeness, I should add an initial belief about v_{BA}.

[15] I thank an anonymous referee for pointing this out.

REFERENCES

Abramson, Paul R., John H. Aldrich, Phil Paolino, and David W. Rohde. 1995. "Third-Party and Independent Candidates in American Politics: Wallace, Anderson, and Perot." *Political Science Quarterly* 110 (3):349–67.

Aldrich, John H. 1980. *Before the Convention: Strategies and Choices in Presidential Nomination Campaigns*. Chicago: University of Chicago Press.

Argersinger, Peter H. 1980. "'A Place on the Ballot': Fusion Politics and Antifusion Laws." *American Historical Review* 85 (April): 287–306.

Bartels, Larry M. 1988. *Presidential Primaries and the Dynamics of Public Choice*. Princeton: Princeton University Press.

Black, Jerome H. 1978. "The Multicandidate Calculus of Voting: Application to Canadian Federal Elections." *American Journal of Political Science* 22 (August):609–38.

Brady, Henry. 1996. "Knowledge, Strategy, and Momentum in Presidential Primaries." *Political Analysis* 5:1–38.

Cox, Gary W. 1987. "Duverger's Law and Strategic Voting." University of Texas. Typescript.

Cox, Gary W. 1991. "Duverger's Law: An Interpretive Review of Research." University of California, San Diego. Typescript.

Cox, Gary W. 1994. "Strategic Voting Equilibria under the Single Non-Transferable Vote." *American Political Science Review* 88 (September):608–21.

Cukierman, Alex. 1991. "Asymmetric Information and the Electoral Momentum of Public Opinion Polls." *Public Choice* 70 (May): 181–213.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper and Row.

Duverger, Maurice. 1954. *Political Parties: Their Organization and Activity in the Modern State*. Trans. Barbara and Robert North. New York: Wiley.

Duverger, Maurice. 1986. "Duverger's Law: Forty Years Later." In *Electoral Laws and Their Political Consequences*, ed. Bernard Grofman and Arend Lijphart. New York: Agathon Press. Pp. 69–84.

Feddersen, Timothy J. 1992. "A Voting Model Implying Duverger's Law and Positive Turnout." *American Journal of Political Science* 36 (4):938–62.

Fey, Mark. 1995. "Information, Polls, and Campaign Dynamics in Multicandidate Elections." Princeton University. Typescript.

Forsythe, Robert, Roger B. Myerson, Thomas A. Rietz, and Robert J. Weber. 1993. "An Experiment on Coordination in Multicandidate Elections: The Importance of Polls and Election Histories." *Social Choice and Welfare* 10 (July):223–47.

Gutowski, William E., and John P. Georges. 1993. "Optimal Sophisticated Voting Strategies in Single Ballot Elections Involving Three Candidates." *Public Choice* 77 (October):225–47.

Harsanyi, John C. 1967–68. "Games of Incomplete Information Played by Bayesian Players, Parts (I, II, and III)." *Management Science* 14 (November): 159–82, (January): 320–34, (March): 486–502.

Ledyard, John O. 1984. "The Pure Theory of Large Two Candidate Elections." *Public Choice* 44 (1):7–41.

Lijphart, Arend. 1994. *Electoral Systems and Party Systems*. Oxford: Oxford University Press.

McKelvey, Richard D., and Peter C. Ordeshook. 1972. "A General Theory of the Calculus of Voting." In *Mathematical Applications in Political Science*, ed. J. F. Herndon and J. L. Bernd. Vol. 6. Charlottesville: University Press of Virginia.

McKelvey, Richard D., and Peter C. Ordeshook. 1985. "Elections with Limited Information: A Fulfilled Expectations Model Using Contemporaneous Poll and Endorsement Data as Information Sources." *Journal of Economic Theory* 36 (June):55–85.

Myerson, Roger B., and Robert J. Weber. 1993. "A Theory of Voting Equilibria." *American Political Science Review* 87 (March):102–14.

Palfrey, Thomas R. 1989. "A Mathematical Proof of Duverger's Law." In *Models of Strategic Choice in Politics*, ed. Peter C. Ordeshook. Ann Arbor: University of Michigan Press. Pp. 69–92.

Palfrey, Thomas R., and Howard Rosenthal. 1991. "Testing Game-Theoretic Models of Free Riding: New Evidence on Probability Bias and Learning." In *Laboratory Research in Political Economy*, ed. Thomas R. Palfrey. Ann Arbor: University of Michigan Press. Pp. 239–68.

Rae, Douglas. 1967. *The Political Consequences of Electoral Laws*. New Haven: Yale University Press.

Riker, William H. 1976. "The Number of Political Parties: A Reexamination of Duverger's Law." *Comparative Politics* 9 (October):93–106.

Riker, William H. 1982a. *Liberalism against Populism: A Confrontation between the Theory of Democracy and the Theory of Social Choice*. Prospect Heights, IL: Waveland Press.

Riker, William H. 1982b. "The Two-Party System and Duverger's Law: An Essay on the History of Political Science." *American Political Science Review* 76 (December):753–66.

Riker, William H. 1986. "Duverger's Law Revisited." In *Electoral Laws and Their Political Consequences*, ed. Bernard Grofman and Arend Lijphart. New York: Agathon Press. Pp. 19–42.

Schattschneider, E. E. 1942. *Party Government*. New York: Farrar and Rinehart.

Simon, Herbert A. 1954. "Bandwagon and Underdog Effects of Election Predictions." *Public Opinion Quarterly* 18 (Fall):245–53.

Smith, Tom W. 1990. "The First Straw? A Study of the Origins of Election Polls." *Public Opinion Quarterly* 54 (Spring):21–36.

Taagepera, Rein, and Matthew S. Shugart. 1989. *Seats and Votes: The Effects and Determinants of Electoral Systems*. New Haven: Yale University Press.

West, Darrell M. 1991. "Polling Effects in Election Campaigns." *Political Behavior* 13 (2):151–63.

Wright, Stephen G., and William H. Riker. 1989. "Plurality and Runoff Systems and Numbers of Candidates." *Public Choice* 60 (February):155–75.

Trends in the Partisan Composition of State Legislatures: A Response to Fiorina

JEFFREY M. STONECASH *Maxwell School, Syracuse University*
ANNA M. AGATHANGELOU *Oberlin College*

The percentage of state legislative seats won by Democrats in nonsouthern states has increased steadily since World War II. Fiorina (1994) argues that this is because of the professionalization of state legislatures: Legislatures now meet longer and pay higher salaries; legislative positions which require more time are less attractive to Republicans, who can make more money elsewhere; higher salaries attract Democrats, who make less in the private sector. That analysis has several serious flaws. First, nonsouthern states have gradually become more Democratic at all levels as part of a long-term regional political realignment. The rise in Democrats in legislatures outside the South is due more to this realignment than to legislative professionalization. Second, trends in southern states contradict his hypothesis. Professionalization has increased, but state legislatures are becoming more Republican. Finally, Fiorina's analysis is worth careful reconsideration because it suggests that there is little connection between constituencies and partisan outcomes.

Morris Fiorina (1994) proposes an interesting hypothesis about the trend in the partisan composition of state legislatures since 1946, namely, the increased proportion of Democrats in legislatures outside the South. He argues that the increase is due to legislative professionalization: Longer sessions make these positions less attractive to Republicans, and higher salaries make the positions more attractive to Democrats.

While the analysis seems plausible, it has serious flaws. First, Fiorina ignores the regional secular political realignments occurring in the country and thus misses a more plausible explanation of partisan trends in nonsouthern states. Although there is a relationship between salary and partisanship, it is a classic case of covariation without a causal connection. Second, his pooled analysis misses the fact that trends in many nonsouthern states contradict his hypothesis. Third, Fiorina excludes states in which the trend directly contradicts his argument. These flaws in and of themselves are reasons to be skeptical of his argument, but perhaps the more important reason is his implicit renewal of an old argument that there is little connection between constituencies and partisan outcomes. This implication is important in thinking about U.S. politics and will be addressed after reviewing the other issues. The primary goal of our analysis is to question the validity of Fiorina's interpretation of change and suggest the plausibility of another perspective.

FIORINA'S ARGUMENT

Fiorina's goal is to explain why the proportion of Democrats in legislatures outside the South has increased steadily since 1946. He begins with three propositions. First, legislative candidates and legislators, as do any group of people, calculate the costs and benefits of pursuing a position. Second, the costs of holding a legislative seat have risen over time as legislative sessions have become longer. Third, compensation has risen, making the position more attractive to some (1994, 306–7). These conditions and changes interact with the pool of party candidates and their job and income opportunities. On average, Republicans are in professional positions that yield higher income and give them flexibility, while Democrats are in lower income positions that are more likely to pay an hourly wage or have a less flexible schedule (p. 307).

Fiorina argues that the interaction of changes in legislatures with these two pools of party candidates explains why Democrats are becoming more prevalent in nonsouthern state legislatures. When legislative office was a part-time job, Republican candidates were more likely to seek the position because a job with prestige, low demands, and low wages would be attractive to them and feasibly would fit into their schedule. Democrats, in contrast, with their more rigid work schedules, would find it difficult to pursue such positions. As legislative jobs became more demanding, and as the pay increased, Republicans found it more difficult to fit the position into their schedule, whereas Democrats found the position more attractive because the compensation was better than for their existing jobs. This resulted in more competition for seats from Democrats, who gradually replaced Republicans.

Fiorina then provides several case studies from states (Ohio and California) in which session length and compensation have increased along with the proportion of Democrats in the state house. His pooled time-series analysis estimates the average relationship between compensation and session length and the proportion of seats held by Democrats in lower houses outside the South since 1946. He finds that these two indicators of professionalism have a positive relationship with Democratic success and concludes that the evidence supports his proposition.

Jeffrey M. Stonecash is Professor of Political Science, Maxwell School, Syracuse University, Syracuse, NY 13244. Anna M. Agathangelou is Instructor, Women's Studies Program and Department of Politics, Oberlin College, Oberlin, OH 44074.

PROBLEMS WITH THE ANALYSIS

While there may be grounds for questioning Fiorina's theory, the concern here is with the evidence presented to support his argument. There are at least four problems. First, Fiorina provides no evidence about differences in the careers or backgrounds of candidates from the two parties. Previous studies indicate that the class origins of Democratic and Republican elected officials do not differ much (Jewell and Patterson 1977, 62–8; Keefe and Ogul 1986, 109–14). If we are to accept the argument that Democrats and Republicans face different opportunities, then surely we ought to see some systematic evidence (not just anecdotes, as on p. 306).

Second, a more serious problem involves the interpretation of trends in the partisan composition of state legislatures. Fiorina neglects the effect of long-term regional trends shaping the partisan composition of state legislatures and other offices. The North became heavily Republican after the 1896 realignment. Since that time, as a result of a complex set of political interactions involving party positioning and electoral reactions, to be discussed below, the North has drifted toward the Democrats. Fiorina does not discuss this alternative and very plausible interpretation of why partisanship in state legislatures has changed. He falls prey to a classic case of covariation without causal connection.

Third, in examining aggregate relationships, Fiorina fails to examine whether the relationship applies state by state. When examined at that level, the relationships run contrary to his hypothesis in a substantial number of states.

Fourth, by choosing to exclude the South, Fiorina eliminates cases which contradict his argument. That choice was made on the ground that "the Republicans have *never* won control of a southern or border legislative chamber" since 1946 (p. 305). Because Fiorina's study is almost entirely about trends, not party control, there is no basis for excluding the South. Trends in professionalism and partisan composition in that region are just as relevant as in other areas of the country. When the southern trends are considered, the evidence contradicts Fiorina's proposition. The following analysis elaborates on these problems.

HISTORICAL BACKGROUND AND PARTISAN TRENDS SINCE 1946

Occasionally, critical elections create nationwide realignment of partisan attachments, and the 1896 election was such an event. Because of sectional divisions over economic issues, the North became heavily Republican, the South heavily Democratic (Sundquist 1983, 143–69). These new partisan attachments extended to state elective office, and in the early 1900s the nation was polarized. The North, defined as all states that provided some support to the Union, was heavily Republican.[1] The South, the eleven states constituting the Confederacy, was totally dominated by the Democrats. Table 1 presents the average percentage of seats held by Democrats in state houses by region from 1900 to 1910.[2] Democrats completely dominated the South, while the rest of the country aligned with the Republicans.

TABLE 1. Percentage of Seats in State Legislatures Held by Democrats, by Region, 1900–10

State Legislature	Region South $n = 60$	 North $n = 82$	 Other $n = 93$
House seats	91.7%	29.8%	26.9%
Senate seats	93.3%	25.5%	27.3%

Note: N refers to the number of state sessions involved from 1900 to 1910. The percentages are the averages of state proportions.

The regional dominance of the parties was so complete that within each region it assuredly reduced the connection between constituencies and the party that won state legislative seats. The domination was such that party strength could only decrease if other issues intruded and created divisions among constituencies. Republican erosion began in the North during the 1920s. Al Smith appealed to urban immigrants, who faced difficult working and living conditions, and Franklin Roosevelt solidified that appeal with expressions of concern for urban problems. People in northern cities began to move to the Democratic Party, creating divisions between Democrats and Republicans within the North (Lubell 1965, 48–55). The potential for a northern Democratic electoral base among blacks expanded after 1950, as many southern blacks moved to northern cities (Lemann 1991, 6). After the mid-1960s the Democratic Party acquired a national image as liberal on social and race issues, and the Republican Party developed a more conservative image on those issues (Carmines, Renten, and Stimson 1984).

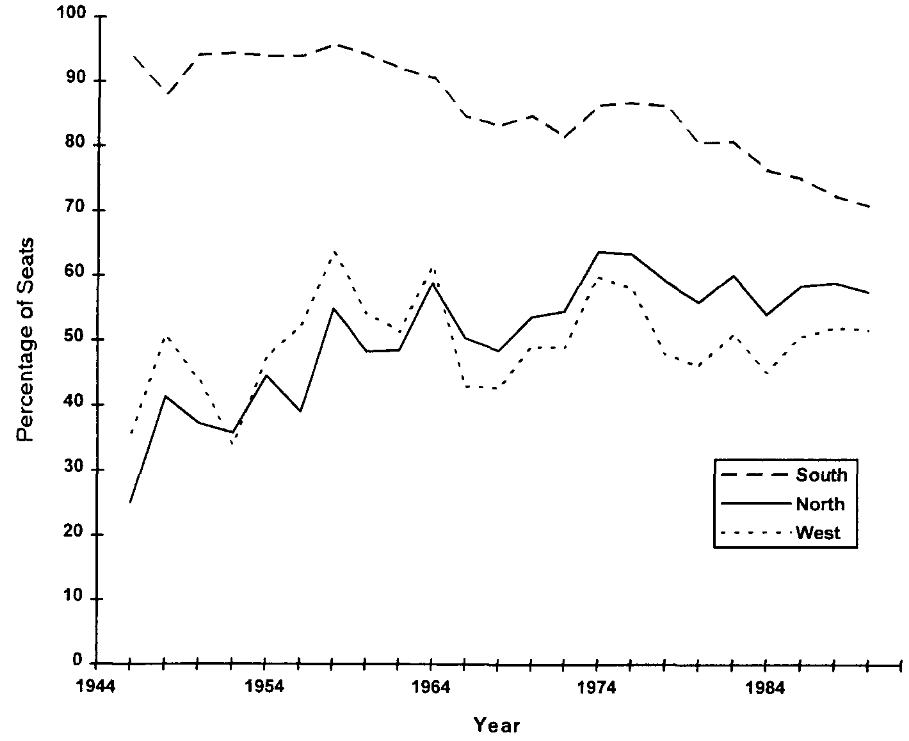
These image changes affected party alignments in the North. In the 1960s northern low-income congressional districts were as likely to elect a Democrat as a Republican. By the 1990s such districts with a high percentage of nonwhites stopped electing Republicans and elected Democrats (Stonecash and Lindstrom 1996). Analyses at the individual level indicate the same pattern. In nonsouthern states, an analysis of National Election Survey (NES) data from 1952 to

---

[1] The North consists of Connecticut, Delaware, Illinois, Indiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, New Hampshire, New Jersey, New York, Ohio, Pennsylvania, Rhode Island, Vermont, West Virginia, and Wisconsin. The South consists of Alabama, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. All others are placed in the "other" category, except Hawaii and Alaska, which are excluded.

[2] These data are taken from *The Tribune Almanac and Political Register* (New York: The Tribune Association, various years). The almanac contained all states except Pennsylvania in 1903 and Nevada and Oklahoma in 1907, so results for these states for these years are not included. In addition, three states (Arizona, New Mexico, and Oklahoma) were not admitted to statehood during all the 1900–1909 period; Arizona and New Mexico became states in 1912, Oklahoma in 1907. These three states are excluded from the analysis.

FIGURE 1. Percentage of Seats Held by Democrats in State House, by Region, 1946–90



1994 indicates that, among whites and nonwhites, the tendency to vote Democratic in congressional elections among lower- to middle-income individuals steadily increased from the 1950s onward (Stonecash 1997a). By the 1980s the Democratic Party in northern states was primarily comprised of liberals and middle- to lower-income people, while Republicans attracted conservatives and upper-income constituents (Baum and Patterson 1992, 200; Bibby 1992, 152; Moakley 1992, 232; Stonecash 1995, 451–7). This carried over to state legislative elections, and the median family income for districts is now a powerful predictor of Democratic success (Stonecash 1997b).

These changes improved the fortunes of Democrats in the North. The percentage of Democrats in nonsouthern state legislatures rose steadily as new issues eroded older alignments. The gradual rise is presented in figures 1 and 2, which show the proportion of state legislative seats won by Democrats from 1946 to 1990 for the South, North, and the remainder of the country.

This political shift also occurred in congressional and gubernatorial elections. Figure 3 presents the average percentage of U.S. House seats held by Democrats since 1946. The pattern parallels that for state legislatures. The average percentage of votes won by Democratic gubernatorial candidates in the Northeast also rose gradually and steadily from the early 1900s until the 1960s (Mayhew 1967).

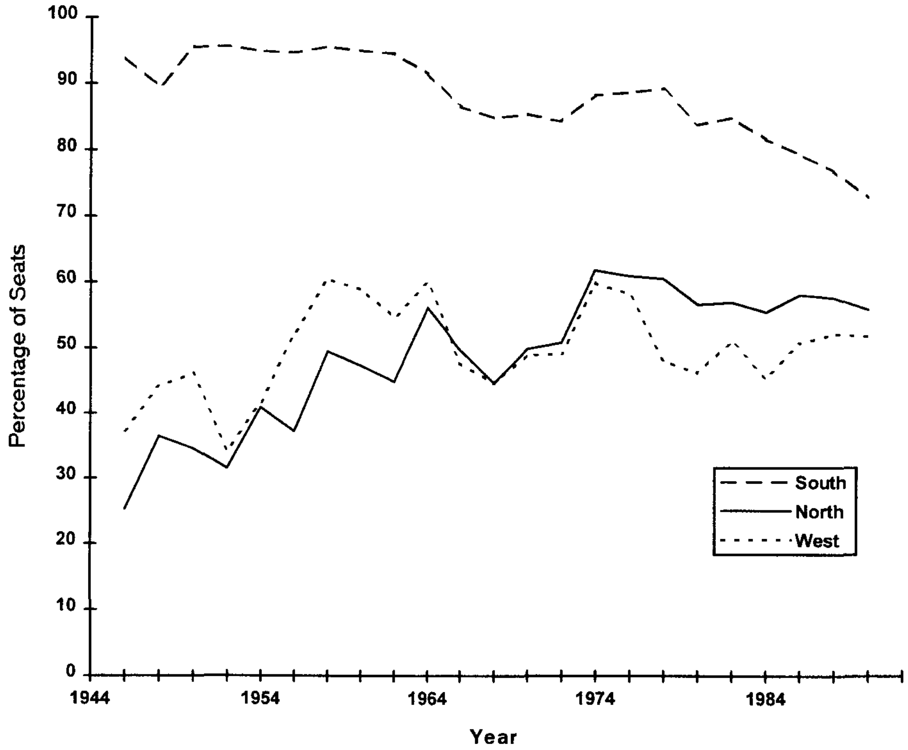
Fiorina suggests that the rise in Democrats in nonsouthern legislatures occurred because sessions became longer and salaries higher. There is no question that session length and pay grew, that the percentage of Democrats increased in nonsouthern legislatures, and that all these trends covaried in nonsouthern states, but the argument that there is a causal connection has significant problems. First, there is a very logical alternative explanation for Democratic success: the long-term realignment in nonsouthern states between constituencies and parties. Second, the trends in partisanship suggest two major problems with the conclusion that salaries and session length were causal factors. The percentage of Democrats in nonsouthern state legislatures *and* Congress increased over time, but state legislative salaries and sessions cannot explain congressional trends. Even if it is argued that the latter can be explained by higher salaries and longer sessions for Congress, that argument is contradicted by the fact that salary changes have been uniform across all congressional delegations, and the trend in the South is the opposite of the argument.

The argument is not that these trends have a spurious relationship, such that some common phenomenon, *x*, is causing *y*, partisanship, and *z*, professionalization. *Y* and *z* each has its own separate causes, and each deserves separate study to understand why change has occurred. Because each has separate sources, no analysis can introduce a third variable and show the "true" relationship between salary and partisanship. Many trends covary without any connection, and this is one of them.

AGGREGATION PROBLEMS AND CONTRADICTORY RESULTS

Fiorina chooses to pool his data to derive an average relationship between salary, session length, and per-

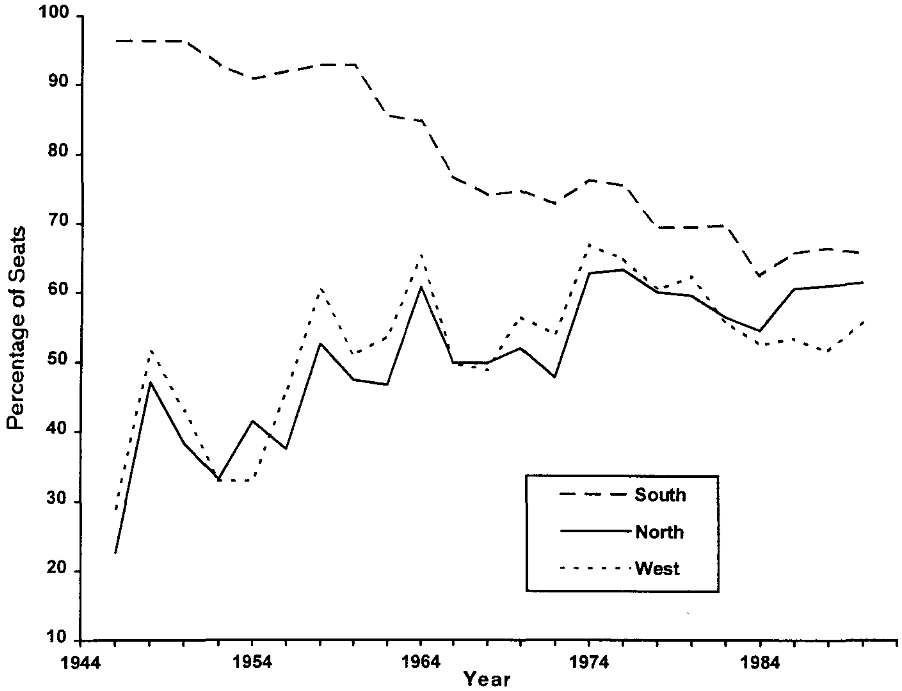
FIGURE 2. Percentage of Seats Held by Democrats in State Senates, by Region, 1946–90



centage of Democratic seats. While that information is valuable, it does not tell us whether the pattern holds state by state. If a relationship holds, it should hold uniformly, not just on an average basis. To explore the issue of uniformity, we regressed the percentage of Democrats in each lower house on salary and on annual sessions from 1946 to 1990.

We did not use the national political indicators (off-year elections, presidential vote, and so forth). We have serious doubts about the appropriateness of in-

FIGURE 3. Percentage of Seats Held by Democrats in the U.S. House, by Region, 1946–90



cluding them. Fiorina states they are "conventional indicators" in such studies, but he presents no rationale for including them beyond that. We know that short-term changes in partisan voting are affected by national conditions, but it is difficult to see how short-term fluctuations in the popularity of different presidents of different parties can be the source of long-term shifts in partisan attachments in specific states, especially when the regions are moving in different directions. The same is true with the economy. Unless some regions blame the president for long-term economic fluctuations and others do not, it is difficult to see the reason for using such indicators.

Furthermore, if a regional realignment is occurring which affects votes for the state legislature, Congress, and the president, then to include such variables as a lag of the dependent variable, the presidential vote, and the gubernatorial vote means to include variables which covary with (and will pick up variance in) the dependent variable, but they are not really independent variables. They are theoretically and statistically redundant variables (Gordon 1968), and including them reduces the variance in the dependent variable without a clear rationale for doing so. More important, including (controlling for) these partisan trend indicators *removes* the trend in partisan composition and leaves us with an analysis in which salary trends explain only the *residual* variation in partisan composition. In addition, with a focus on individual states, using these variables in state-by-state regressions, with only 23 cases for each state, would leave few degrees of freedom (too many variables for the number of cases). Our concern, when examining specific states, is the more direct and simple one of the relationship between partisan composition and professionalization indicators.

We also did not use Fiorina's days variable, which contains serious and systematic measurement error. To construct this measure, he used the *Book of the States*, which lists the total number of days a legislature is in session.[3] For a two-year session he lists many states as being in session more than 700 days by the 1980s. New York, for example, is listed as moving to sessions of 736 days in 1978. While this is formally true, it is a *very* inaccurate indicator of the actual days the legislature was in session. Beginning in the 1970s, New York (as have several other states) decided *never* to adjourn so the governor would no longer have control over calling the legislature into session. One local member appears each day and responds to a call to order. To count these days enormously exaggerates the length of session since this practice began and significantly distorts this measure starting in the 1970s. There is a serious upward bias in Fiorina's indicator after the 1970s in *several* states, not just New York. To obtain an accurate measure would require sitting down with the clerk in each legislature and going through historical information to count the number of days actually in session. Given the clear errors in Fiorina's days variable as well as the great difficulty of gathering accurate information, we have chosen to focus on salary and annual sessions.[4]

TABLE 2. Coefficients for Regression of Percentage of Democratic Seats on Salary and Annual Session for Nonsouthern State Houses, 1946–90

	Variable	
Coefficient	Salary	Annual Sessions
Negative	14	12
Positive	24	15
Total	38	27

The question is whether salary and annual sessions have the expected positive relationship with the percentage of Democratic seats. These regressions indicate further evidence problems. The results are given in Table 2. For the 38 nonsouthern states (Fiorina includes Kentucky and Tennessee in this group), 24 salary coefficients are positive, 14 are negative. For annual sessions, 15 have positive coefficients, 12 have negative ones. Some states had annual sessions across the entire period, so coefficients could not be estimated for them. Among nonsouthern states, approximately 40% of the cases had relationships contrary to the hypothesis. We also ran the same analysis for upper houses, and the results mirror those for lower houses. The aggregate relationship, as an average relationship, conceals the fact that the pattern is the opposite of that expected in a large proportion of cases.

INCLUDING THE SOUTH

Fiorina chose to exclude the South because Republicans never held a southern house after 1946. The focus of his analysis is trends and their relationships, not the presence or absence of control. Political trends in the South are as relevant to his hypothesis as are those in any other region of the country. Along with the rest of the nation, the South has experienced political change, particularly since World War II. The Democratic Party was increasingly seen as liberal on civil rights and welfare issues, and conservatives in the South moved to the Republican Party (Black and Black 1987, Carmines and Stimson 1989, Cotter 1992, Parker 1992, Petrocik

---

[3] Fiorina shared his data set with us, and we greatly appreciate that. Forming a response to him would have been very difficult without his data.

[4] To obtain an indicator of salary which is not plagued by the problems with days, we gathered new information on remuneration. Fiorina multiplies all possible per diems and reimbursements by the days variable to compute total remuneration. Since the days variable contains a systematic upward bias in later years, it inflates total remuneration in later years. We recorded announced salary, since it is the amount of income a legislator can count on. We computed this as actual session salary or per diem times the maximum number of days the legislature could meet or might have met, given the records of session length for each year in the *Book of the States*. We then used this variable for our regressions and found the results to be only slightly different from those we found using Fiorina's salary figure. To provide comparability with his numbers, we decided to use his data.

TABLE 3. Coefficients for Regression of Percentage of Democratic Seats on Salary and Annual Session, All State Houses, 1946–90

	Salary			Annual Sessions		
Coefficients	Non-South	South	Total	Non-South	South	Total
Negative	14	9	23	12	5	17
Positive	24	0	24	15	2	17
Totals	38	9	47	27	7	34

1987, Stanley 1992). The Republicans began a steady rise in this region, and as figures 1–3 indicate, the percentage of Democrats dropped steadily after 1946.

Professionalization changes in the South have been comparable to those elsewhere. From 1946 to 1990 the average salary (using Fiorina's data), in real 1990 dollars, in nonsouthern legislatures rose from $9,815 to $48,872. In the nine states Fiorina classifies as southern, the increase was from $7,246 to $36,546. While the southern salaries still lag the rest of the country, the relative change, a fivefold increase in real dollars, is very similar to that nationwide. The change in the South has been significant, and the absolute salary level (compared to the national median family income of about $35,000) surely seems high enough to attract people to office. The movement to annual sessions also has been comparable. In nonsouthern states the number of legislatures with annual sessions has grown from five to thirty-four since 1946, while in the nine Southern states the number has risen from one to nine.

If professionalization increases Democratic success, then the proportion of seats held by Democrats in the South should have increased from 1946 to 1990. Table 3 presents the coefficients for salary and presence of annual sessions for southern state houses along with the earlier results for nonsouthern states. Almost all the results contradict Fiorina's hypothesis. If Kentucky and Tennessee are included in the South, as they usually are, the results are all negative coefficients for salary, and two positive and six negative coefficients for annual sessions. Patterns in the South clearly contradict the hypothesis.

The results for all states are particularly interesting. For salary, almost half the coefficients are negative, and for annual sessions exactly half are negative. The coefficients for each state regression are reported in Table 4. It is very difficult to conclude there is a positive relationship between professionalization and the percentage of Democratic seats when half the cases contradict the hypothesis.

IMPLICATIONS

Empirical analyses have implications, intended or not. What is perhaps most important about Fiorina's research is its underlying implication. His proposition is that individual calculations about remuneration and time demands interact with pools of party candidates and affect partisan outcomes. His argument is similar to that made by opponents of political "machines," which implies that the connection between constituents and legislators is tenuous. It is claimed that Democrats, lacking opportunities in the private sector, go into politics because it is accessible and pays more than they can make elsewhere (Merton 1976). Critics maintain that Democrats become professional politicians and displace men of character whose concern is to do the "right thing" in politics and then return to the private sector (Allswang 1971, 3–35), an argument Fiorina (1977, 35–36) has made about Congress. The suggestion is that the election of Democrats does not reflect support from specific constituencies. Parties displace each other in office as salary levels and session length vary.[5]

An example may illustrate the logic involved in Fiorina's argument. Assume a northern district has roughly the same composition over time. If legislative salaries increase and sessions are longer, then the seat shifts from Republican to Democrat. If parties differ, then the views expressed in the state legislature for that district shift over time as a function of the professionalization of the legislature. The composition of the district has no connection to the party winning the seat or to representation. This is a regular criticism of machine politicians and is implicit in much of the legislative literature, ever since Mayhew (1974) suggested we view legislators as entrepreneurs calculating how to win reelection.

Fiorina's analysis has serious empirical problems. Alternative explanations are neglected, the data analysis is questionable, and the South is inappropriately ignored and contradicts his conclusion. But it is the implications about representation which should prompt some pause. Before Fiorina's analysis is accepted, we need to consider the explanation suggesting that partisan trends may reflect secular realignment, which may have enhanced representation (Key 1959). Establishing that argument is beyond this response to Fiorina, but it deserves consideration before his analysis is accepted uncritically.

---

[5] The argument also has been made that salary motivates legislators to stay in office and that higher salaries prompt higher reelection rates. The evidence is just as questionable as that for the effect of salary on partisanship. Benjamin and Malbin (1992, 291–3) provide evidence that members of Congress were reelected at the rate of 80% or more for the last 200 years. Real salaries have varied enormously over that time. A detailed analysis of New York from 1870 to 1990 also indicates salary has little relationship to seeking reelection or being reelected (Stonecash 1993). It does not appear that salary is a primary motivator of who runs and wins or of whether they seek to remain in office. We need to pursue another line of inquiry about what motivates the pursuit of office and what keeps people in office.

TABLE 4. Regression Results of Percentage of Democratic Seats in Lower Houses of State Legislatures, on Salary and Presence of Annual Sessions, 1946–90

	Intercept	Annual Session	Salary	R^2
Nonsouthern States				
Arizona	95.6	−28.0 (7.7)	−.00065 (.0002)	.70
Arkansas	100.2	*	−.00030 (.00005)	.57
California	38.5	*	.00028 (.00003)	.62
Colorado	40.5	7.5 (9.3)	−.00009 (.0002)	.03
Connecticut	32.7	3.5 (10.2)	.00090 (.0005)	.43
Delaware	57.4	6.3 (10.4)	−.00040 (.0003)	.10
Idaho	42.6	−13.7 (6.3)	−.00002 (.0005)	.43
Illinois	46.8	2.6 (6.5)	.00005 (.0001)	.22
Indiana	21.1	−15.6 (12.7)	.00108 (.0005)	.23
Iowa	15.0	8.7 (9.6)	.00080 (.0003)	.75
Kansas	18.6	13.2 (3.4)	.00040 (.0001)	.77
Kentucky	79.1	*	−.00047 (.0002)	.20
Maine	20.2	−0.5 (8.0)	.00160 (.0004)	.71
Maryland	69.0	10.5 (6.1)	.00013 (.0001)	.50
Massachusetts	41.6	*	.00042 (.00008)	.54
Michigan	24.6	16.6 (6.7)	.00017 (.0001)	.64
Minnesota	76.1	1.5 (11.0)	−.00038 (.0005)	.09
Missouri	50.5	−6.3 (4.6)	.00042 (.0001)	.42
Montana	47.7	*	.00038 (.0004)	.04
Nevada	63.6	*	−.00007 (.0003)	.00
New Hampshire	40.8	−8.2 (3.4)	−.00600 (.004)	.23
New Jersey	25.6	*	.00062 (.0002)	.29
New Mexico	68.3	−13.9 (6.1)	.00158 (.002)	.22
New York	27.8	*	.00025 (.00004)	.68
North Dakota	2.7	*	.00350 (.0009)	.40
Ohio	15.4	11.9 (6.0)	.00048 (.0002)	.80
Oklahoma	84.1	−3.7 (2.9)	−.00016 (.0001)	.70
Oregon	35.4	*	.00050 (.0002)	.21
Pennsylvania	41.9	8.4 (5.2)	.00002 (.00007)	.30
Rhode Island	80.8	*	−.00133 (.001)	.15
South Dakota	26.3	20.3 (9.6)	−.00109 (.001)	.23
Tennessee	76.2	−2.5 (10.0)	−.00031 (.0003)	.44
Utah	36.6	−12.2 (7.3)	.00172 (.002)	.10
Vermont	16.1	20.8 (5.2)	.00041 (.0002)	.80
Washington	51.6	−.3 (6.4)	.00013 (.0002)	.03
West Virginia	68.6	−2.1 (5.9)	.00052 (.0003)	.20
Wisconsin	27.3	6.3 (9.9)	.00038 (.0002)	.63
Wyoming	44.5	6.5 (7.0)	−.00180 (.001)	.07
Southern States				
Alabama	101.4	−11.7 (3.6)	−.00011 (.0002)	.39
Florida	92.4	−20.7 (6.0)	−.00008 (.0001)	.70
Georgia	101.3	−3.2 (2.6)	−.00040 (.00008)	.67
Louisiana	102.2	−1.8 (2.5)	−.00023 (.0001)	.49
Mississippi	100.7	−2.8 (4.6)	−.00009 (.0002)	.40
North Carolina	95.4	7.3 (4.2)	−.00079 (.0002)	.58
South Carolina	95.4	*	−.00010 (.00004)	.20
Texas	91.9	*	−.00014 (.0002)	.10
Virginia	97.9	15.1 (12.5)	−.00122 (.0004)	.80

*State had annual sessions for all years examined.

REFERENCES

Allswang, John M. 1971. *Bosses, Machines, and Urban Voters*. Port Washington, NY: Kenikat Press.

Baum, Laurence, and Samuel C. Patterson. 1992. "Ohio: Party Change without Realignment." In *Party Realignment and State Politics*, ed. Maureen Moakley. Columbus: Ohio State University Press. Pp. 192–209.

Benjamin, Gerald, and Michael J. Malbin. 1992. *Limiting Legislative Terms*. Washington, DC: CQ Press.

Bibby, John F. 1992. "Wisconsin Electoral Politics: Realignment Bypasses the Badger State." In *Party Realignment and State Politics*, ed. Maureen Moakley. Columbus: Ohio State University Press. Pp. 144–62.

Black, Earl, and Merle Black. 1987. *Politics and Society in the South*. Cambridge, MA: Harvard University Press.

Carmines, Edward G., and James A. Stimson. 1989. *Issue Evolution*. Princeton: Princeton University Press.

Carmines, Edward G., Steven H. Renton, and James A. Stimson. 1984. "Events and Alignments: The Party Image Link." In *Controversies in Voting Behavior*, 2d ed., ed. Richard G. Niemi and

Herbert F. Weisberg. Washington, DC: Congressional Quarterly Press. Pp. 545–60.

Cotter, Patrick R. 1992. "Alabama: The Unsettled Electorate." In *Party Realignment and State Politics*, ed. Maureen Moakley. Columbus: Ohio State University Press. Pp. 91–105.

Fiorina, Morris. 1977. *Congress: Keystone to the Washington Establishment*. New Haven: Yale University Press.

Fiorina, Morris. 1994. "Divided Government in the American States: A Byproduct of Legislative Professionalism." *American Political Science Review* 88 (June):304–16.

Gordon, R. A. 1968. "Issues in Multiple Regression." *American Journal of Sociology* 73 (2):592–616.

Jewell, Malcolm E., and Samuel C. Patterson. 1977. *The Legislative Process in the United States*, 3d ed. New York: Random House.

Keefe, William J., and Morris S. Ogul. 1986. *The American Legislative Process: Congress and the States*, 6th ed. New York: Prentice-Hall.

Key, V. O., Jr. 1959. "Secular Realignment and the Party System." *Journal of Politics* 21 (2):198–210.

Lemann, Nicholas. 1991. *The Promised Land*. New York: Knopf.

Lubell, Samuel. 1965. *The Future of American Politics*, 3d rev. ed. New York: Harper Colophon.

Mayhew, David R. 1967. *Two-Party Competition in the New England States*. Amherst: Bureau of Government Research, University of Massachusetts.

Mayhew, David R. 1974. *The Electoral Connection*. New Haven: Yale University Press.

Merton, Robert K. 1976. "The Latent Functions of the Machine." In *The City Boss in America*, ed. Alexander B. Callow, Jr. New York: Oxford University Press. Pp. 23–33.

Moakley, Maureen. 1992. "Secular Realignment in New Jersey." In *Party Realignment and State Politics*, ed. Maureen Moakley. Columbus: Ohio State University Press. Pp. 225–44.

Parker, Suzanne L. 1992. "Florida: A Polity in Transition." In *Party Realignment and State Politics*, ed. Maureen Moakley. Columbus: Ohio State University Press. Pp. 106–26.

Petrocik, John R. 1987. "Realignment: New Party Coalitions and the Nationalization of the South." *Journal of Politics* 49 (May): 347–75.

Squire, Peverill. 1992. "Iowa and the Drift to the Democrats." In *Party Realignment and State Politics*, ed. Maureen Moakley. (Columbus: Ohio State University Press). 179–91.

Stanley, Jeanie R. 1992. "Party Realignment in Texas." In *Party Realignment and State Politics*, ed. Maureen Moakley. Columbus: Ohio State University Press. Pp. 74–91.

Stonecash, Jeffrey M. 1993. "The Pursuit and Retention of Legislative Office in New York, 1870–1990: Reconsidering Sources of Change." *Polity* 26 (Winter):301–15.

Stonecash, Jeffrey M. 1995. *American State and Local Politics*. Fort Worth, TX: Harcourt and Brace.

Stonecash, Jeffrey M. 1997a. "The Increase in Class Voting in Congressional Elections in Non-Southern States, 1952–1992," Department of Political Science, Maxwell School, Syracuse University. Typescript.

Stonecash, Jeffrey M. 1997b. "Class, Race, and Party in State Legislative Elections." Department of Political Science, Maxwell School, Syracuse University. Typescript.

Stonecash, Jeffrey M., and Nicole R. Lindstrom. 1996. "Class, Race, and Party in U.S. House Elections." Presented at the 1996 Southern Political Science Association Meetings, Atlanta, Georgia.

Sundquist, James L. 1983. *Dynamics of the Party System*, rev. ed. Washington, DC: Brookings Institution.

Professionalism, Realignment, and Representation

MORRIS P. FIORINA *Harvard University*

The critique of my 1994 article by Stonecash and Agathangelou reflects a series of misconceptions and misunderstandings—about measures, methods, arguments, and findings. In this rejoinder I attempt to correct these. In addition, I clarify my methods and findings. First, I show that a formal statistical test indicates that limiting the analysis to the northern states is justified. Less formally, the professionalism hypothesis cannot work the same in the South as in the North unless levels of Democratic legislative strength can rise above 100%. Second, although clearly inferior to a pooled analysis, I show that a disaggregated (state-by-state) analysis is far more supportive of the professionalization hypothesis than the flawed results Stonecash and Agathangelou report. Third, despite the repeated assertions of Stonecash and Agathangelou, I demonstrate that there is no evidence that a long-term partisan realignment to the Democrats is occurring, and that, contrary to their methodological recommendations, the variables included in my analysis would capture it if it were. Finally, Stonecash and Agathangelou interpret my research as indicating a lack of relationship between constituencies and who gets elected. That is simply not a correct reading of my article.

My 1994 article (Fiorina 1994) was a narrowly focused piece, and I thought that the arguments, procedures, and results were clearly presented. Apparently, I was wrong. The critique by Stonecash and Agathangelou (1997) reflects a series of misconceptions and misunderstandings—about measures, methods, arguments, and findings. No doubt some of these are shared by others, so, I am grateful to my critics for providing this opportunity for clarification.

Because the various misunderstandings in Stonecash and Agathangelou are intertwined, the starting point for a reply is not obvious. I will first answer the questions they raise about research design, measures, and methods, showing that their criticisms are groundless. Then I address the alternative realignment hypothesis my critics offer, pointing out that there is no evidence to support it. Finally, I reply to the more general philosophical or normative concern that apparently motivates their critique. Here, they wrongly cast me as an adversary, when I am, in fact, on their side.

EXCEPT IN THE SOUTH

Long ago I recall hearing that if there were a first law of U.S. politics, it undoubtedly would be qualified by the phrase "except in the South."[1] Over the years, hundreds of analyses of myriad topics in U.S. politics routinely set aside the South, not because southern politics were inherently uninteresting but because of a general understanding that various dynamics were different there. Such was the case in the research at issue here.

The starting point for my article was the earlier realization that (1) unified government in the American states had greatly decreased in the postwar period, (2) the decrease was a result of a decline in unified Republican states, and (3) the Republican decline reflected a decline in Republican legislatures (Fiorina 1992, 32–6). In addressing the third point I noted that the Republicans had never controlled a southern or border state legislature during the postwar period (my analysis ended with the 1990 elections). Thus, it seemed natural to begin with the question, "What explains the decline in Republican legislative strength in the North?"

Having formulated a hypothesis—that increasing legislative professionalism advantages Democrats—there is no reason in principle not to apply it outside the North.[2] I considered doing so and compiled data for the whole country. Preliminary analysis, however, indicated that the processes at work were very different in the southern and border states. Generational replacement, the out-migration of African-Americans, and the in-migration of people and industry have interacted with changes on the national political scene to produce electoral outcomes distinct from those in the larger nation (Black and Black 1987). Ironically, Stonecash and Agathangelou take me to task for ignoring the possibility of a questionable realignment in northern states, but they estimate equations that make no attempt to account for a known realignment in southern states.

Statistically speaking, the national data do not pool. The equations estimated in Table 1 of my 1994 article (p. 306) cannot even be estimated when the southern and border states are included in the analysis. The estimations fail because levels of Democratic representation in many southern legislatures are virtually constant (at or near 100%) until the mid-1960s or even mid-1970s in some states. As a result, the lagged dependent variable in these states is nearly the same as the state intercept for much of the period.[3] If regional

---

Morris P. Fiorina is Professor of Government, Harvard University, Cambridge, MA 02138.

I thank James Alt and Gary King for their comments and suggestions.

[1] Like other remarks in a similar vein, this one is often attributed to Ray Wolfinger.

[2] Indeed, outside the United States for that matter. As noted earlier (Fiorina 1992, 50), a version of the hypothesis was articulated by John Stuart Mill writing about parliamentary compensation in nineteenth-century Britain.

[3] Substantively speaking, these high levels of Democratic legislative strength mean that the experience of most of these states provides little or no usable information, unless we believe that rising compen-

TABLE 1. Explaining Democratic Lower House Seat Shares, 1946–90: Is the Nation Homogeneous?

	Main Effects		Southern Interactions		Border Interactions	
Variable	Coefficient	*t*-value	Coefficient	*t*-value	Coefficient	*t*-value
South	242.120	2.14	—		—	
Border	262.120	1.32	—		—	
Year	.032	.91	−.151	−2.28*	−.142	−1.31
Democrats (*t* − 1)	.508	20.10**	.435	7.70**	−.067	−.52
Compensation	.007	2.08*	−.005	−1.03	−.007	−.59
Presidential year	−27.079	−11.10**	22.778	6.19**	11.520	1.35
Presidential vote	.498	9.40**	−.452	5.85**	−.234	−1.39
Gubernatorial year	−.18.540	−7.86**	15.111	4.08**	−.440	−.04
Gubernatorial vote	.364	8.10**	−.328	−5.52**	−.044	−.20
Off year	−10.042	−10.62**	6.800	3.80**	2.818	.82
GNP growth	.149	2.74**	−.157	−1.49	−.148	−.79

Note: Thirty-one state intercepts not shown. Coefficients are unstandardized.
$n = 1{,}035$, adjusted $R^2 = .89$, $SEE = 7.94$
$^*p < .05$; $^{**}p < .01$

dummy variables for the southern and border states are substituted for the individual state intercepts, equations can be estimated. Then, after forming southern and border interactions with all the other right-hand side variables, a standard *F*-test indicates whether the southern and border states are part of the same sample as the other 31 states.[4] The results indicate that they are not—at well beyond the .01 significance level.[5] As shown in Table 1 above virtually all the southern interactions are highly significant; the border interactions mostly have the same signs but are weaker in magnitude. In the South there is a clear trend away from the Democrats, presidential and gubernatorial coattails are nearly the reverse of the main effects, the midterm penalty is mostly wiped out, the national economy does not matter, and the retention rate of Democrats from the previous legislature is nearly unity (.508 + .435 = .943). Interestingly, the main effect of legislative compensation is still significant in the whole sample, and the net effect in the South (.007–.005) is slightly positive—suggesting that rising compensation has partly dampened the trend toward the Republicans, consistent with the original hypothesis—but I put little confidence in this pooled estimation.

In sum, a formal statistical test provides conclusive grounds for rejecting Stonecash and Agathangelou's argument that my 1994 analysis should have included the entire United States. More data do not always mean a more dependable analysis.[6]

MEASURES

Stonecash and Agathangelou criticize the crude nature of the variables used to proxy legislative professionalization. Everyone who has ever compiled data from the *Book of the States* knows that numerous errors are inevitable. I explicitly listed some of the sources of error in my article (1994, 308).[7] But imperfect data in no way distinguish research on state politics from any other research. If we have to wait for perfect data before doing any empirical analysis, we might as well all become theorists. The alternative I prefer is to recognize the sources of error in the data, use multiple indicators, and proceed carefully. In the case at hand, the errors are in the independent variables and generally work against my hypothesis. Consider the New York example noted by Stonecash and Agathangelou. The error in measuring "days in session" clearly works against the professionalization hypothesis: If the "days" measure exaggerates the burdens of legislative service in New York, then Republicans would not be as discouraged as the measure would predict.

Once again, I find Stonecash and Agathangelou's own procedures perplexing in light of the criticism they level: "Given the clear errors in Fiorina's days variable, as well as the great difficulty of gathering accurate information, we have chosen to focus on salary and annual sessions" (p. 152). But *Annual Sessions* is a *dummy* variable that assigns the same value to New York and to Wyoming. Because there are errors in "days in session" Stonecash and Agathangelou think it preferable to assign only values of zero and one? In the 1989–90 sessions using "days" has the effect of assigning 729 days to New York and 79 to Wyoming.

---

sation is sufficiently important to push levels of Democratic legislative strength above 100%.

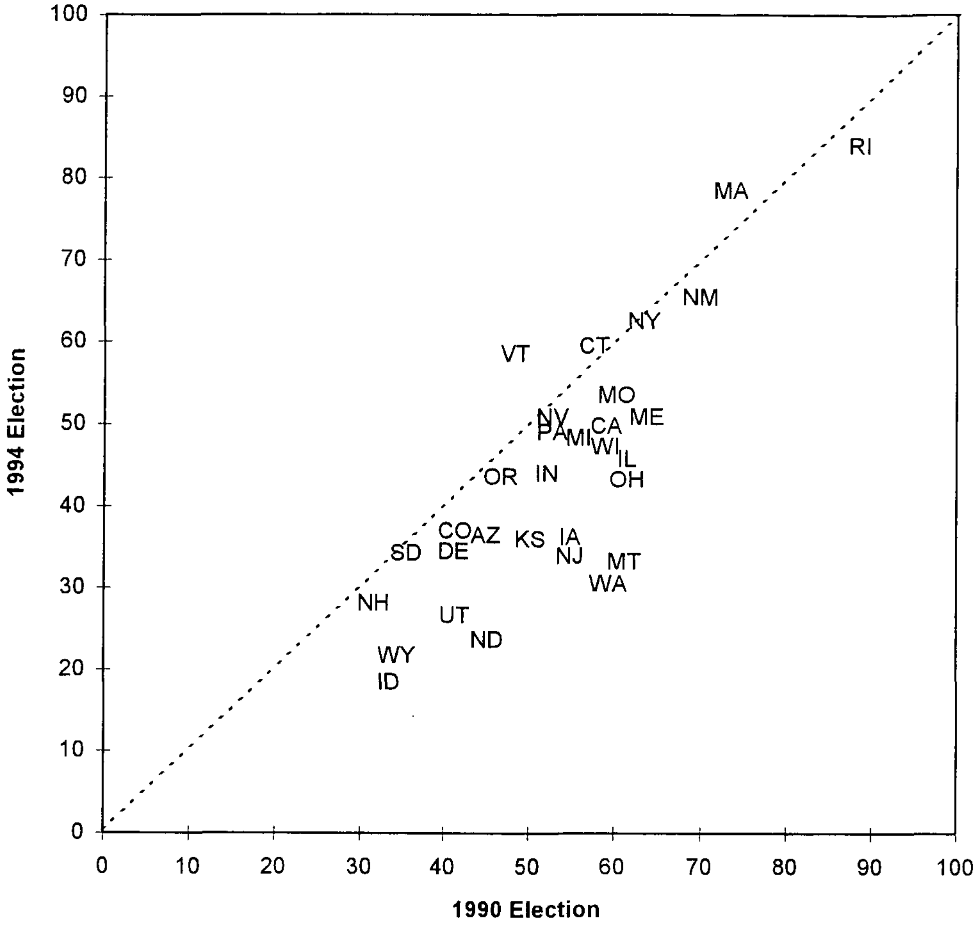
[4] As in the original article nonpartisan Nebraska is omitted, as are Alaska, Hawaii, and Minnesota, which lack data for a significant part of the series.

[5] The sum of squared residuals in the additive model is 74,230, compared to 61,460 in the model with south and border interactions. With 1,035 observations, 42 variables (including 31 state intercepts and south and border dummies) in the additive model, and 60 variables in the interactive model, the standard formula yields an F statistic of 11.24.

[6] I would not defend to the death the decision to omit all the border states, which include West Virginia, a state that nicely fits my hypothesis (i.e., it falls in the top category of Table 2), but since the border category includes states such as Kentucky and Oklahoma that are often categorized as southern (e.g., by Congressional Quarterly), I eliminated all of them rather than be suspected of picking and choosing among them. Incidentally, Stonecash and Agathangelou comment that I coded Tennessee as a border state rather than a southern state. That was a coding error in the data file; it did not affect the analysis because both southern and border states were omitted.

[7] For a recent discussion of some of the questions that arise see Carey, Niemi, and Powell (1996, 25–7).

FIGURE 1. Comparison of Democratic Proportion of Lower House Seats after the 1990 and 1994 Elections



Inaccurate though it may be, "days" captures the difference between the two state legislatures better than does a dummy variable.

METHODS

With short time series—23 elections in this case—individual state analyses are impractical. The ratio of noise to information is too high. Inclusion of the theoretically hypothesized variables plus relevant control variables eats up too many degrees of freedom to permit confident conclusions. But short time series across similar units can be pooled to increase efficiency and extract the maximum amount of information from the data. When diagnostics revealed no peculiarities in the data (Fiorina 1994, n. 15), I turned to standard estimating techniques. The results were statistically significant and robust across specifications.

Stonecash and Agathangelou reject the advice of the methodologists I consulted and claim that regressions on individual states are the more appropriate statistical technique. Using the data I provided they regress percentage of Democratic seats on the *Annual Sessions* dummy variable and the *Real Compensation* variable and report that only about 60% of the two-variable regressions have the right sign for *Compensation*, the workhorse variable in my analyses. This exercise is misconceived.

Stonecash and Agathangelou claim to get around the degrees-of-freedom problem by including only two right-hand side variables, the dummy variable for annual sessions and the real compensation variable that emerged significant in my pooled analysis. They do not include a lagged variable (percentage of Democratic representatives in the previous legislative session), a statistical necessity.[8] Nor do they include presidential and gubernatorial coattails, national economic conditions, or the midterm penalty, saying that I have "no rationale for including them" beyond the fact that they are "conventional indicators" (p. 152). Following are some rationales:

---

[8] Stonecash and Agathangelou's omission of the lagged percentage of Democratic legislators ignores problems of autocorrelation, but even more important, the omission of a variable strongly related to the dependent variable results in biased coefficients. In Stonecash and Agathangelou's case, since lagged Democrats is positively correlated with annual sessions and compensation, both those coefficients will be biased downward.

1. Look at Figure 2 (1994, 305) of the original article: Years such as 1958, 1964, and 1974 stand out against the general trend. These are well known as years of electoral turbulence in which strong national tides were running.
2. How do indicators become "conventional"? The main reason is that previous research has clearly established their relevance. In this case, articles on state legislative elections, some of them recently published in this *Review*, find highly significant effects for national and state-level political forces (Campbell 1986; Chubb 1988; Simon, Ostrom, and Marra 1991).
3. Consider the 1994 elections that not only gave control of Congress to the Republicans but also gave them control of more state legislatures than any election since 1968 (Fiorina 1996, 139–40). I have not updated the original data set beyond the 1990 elections, but judging from figures A-1 and A-3 of my article (1994, 313–4) annual sessions and real compensation had leveled off by 1990—all but four states included in the analysis had annual sessions by then, and legislatures obviously were not raising their pay at a time when their state electorates were clamoring for term limits. Thus, Stonecash and Agathangelou's equations that include only these two variables make the out-of-sample prediction that the same level of Democratic legislative strength will exist in 1994 as in 1990. Figure 1 illustrates the inaccuracy of such a prediction. On average the Democrats won 10% fewer lower house seats in 1994 than in 1990; only 3 of 31 states did not see Democratic representation in their lower house fall. This systematic misprediction indicates that Stonecash and Agathangelou's equations omit important features of state legislative elections.

In contrast, the equations in my article make the out-of-sample prediction that the replacement of Republican President Bush by Democratic President Clinton would result in 9–10% fewer Democratic seats in 1994 than in 1990. The coattails of victorious Republican governors (24 of 36 in 1994) would have subtracted a bit more (.4% for every percentage point they gained above 50.5%), while the coattails of victorious Democrats would have dampened the midterm loss by a similar amount (the state that falls farthest above the line is Vermont, where Democratic Governor Dean's landslide victory–69% of the vote—largely offset the Republican tide). The "conventional indicators" that Stonecash and Agathangelou dismiss are in fact the only way of capturing the systematic changes evident in Figure 1, as recognized by virtually all previous research.

Why would Stonecash and Agathangelou inexplicably dismiss national and state political forces and estimate such misspecified equations? Part of the explanation appears to be that they are under the erroneous impression that I am analyzing "long-term shifts in partisan attachments" (p. 152) for which short-term national and state political developments are irrelevant. This is not so. And that is not the hypothesis they "test" with their simple regressions. My article analyzes year-to-year variations in Democratic lower house seats; so do Stonecash and Agathangelou.

TABLE 2. Relationships Between Real Compensation and Democratic Legislative Strength

Coefficient	States	Kurtz Category[a]
Positive ($1.73 < t < 2.8$) ($p < .05$)*	California	red
	Connecticut	white
	Illinois	red
	Iowa	white
	Maine	blue
	Michigan	red
	New Jersey	red
	New York	red
	Ohio	red
Positive ($1.33 < t < 1.73$) ($p < .10$)	Indiana	white
	Kansas	white
	Missouri	white
	North Dakota	blue
	Pennsylvania	red
Positive ($t < 1.33$)	Arizona	white
	Massachusetts	red
	Montana	blue
	South Dakota	blue
	Utah	blue
	Washington	white
	Wisconsin	red
Negative	Colorado	white
	Delaware	white
	Idaho	blue
	Nevada	blue
	New Hampshire	blue
	New Mexico	blue
	Oregon	white
	Rhode Island	blue
	Vermont	blue
	Wyoming	blue

Source: Kurtz 1991.
*one-tailed test, $df = 20$
[a] red = professionalized; blue = nonprofessionalized; white = in-between. See footnote 9 for further detail.

Still, is it at all disturbing that stronger indications of professionalization effects do not emerge in simple state-by-state regressions? In fact, stronger indications do emerge. In my original article I indicated that while exploring the data set I did some individual state analyses (1994, 309). If the results had been as negative as those Stonecash and Agathangelou achieved, I probably would not have continued the research. Table 2 should allay any suspicions that sleight-of-hand is behind pooled data and multivariate regressions. The table reports estimates of the effect of real compensation on Democratic legislative strength in the 31 states included in my original analysis. Like the equations estimated by Stonecash and Agathangelou, the equations summarized in Table 2 include a constant and real compensation, but they also include the lagged value of the dependent variable. These equations too are misspecified in that they exclude the initial effect of short-term forces that Stonecash and Agathangelou exclude, but the lagged dependent variable does capture the continuing effects of such shocks and greatly improves the fit of the equations.

Nearly 70% of the coefficients on real compensation are positive, and two-thirds of these have *p* levels of at least .10, even with only 23 observations per regression. States whose electoral histories support the hypothesized relationship span most of the country, from New York to California and Michigan to Missouri. In contrast, only one of the negative coefficients (for Idaho) is larger than its standard error. None of the nine states in Kurtz's (1991) "red" (most professionalized) category has a negative coefficient, and seven of the nine have coefficients with *p* levels of at least .10. In contrast, states in Kurt's "blue" (least professionalized) category dominate the category of states with negative coefficients.[9] In sum, states whose legislatures professionalized the most experienced significant gains in Democratic legislative strength. States whose legislatures professionalized the least experienced little or no gains in Democratic strength.

I do not wish to make too much of these simple regressions, but at a minimum they suggest that the hypothesized relationship is sufficiently robust to appear even in the absence of important control variables. It is not some sort of statistical artifact teased out of a large number of observations by complex methods and multiple variables. On the contrary, the original pooled analysis only makes it possible to get a statistically more precise and causally purer estimate of the relationship suggested by the individual state analyses.

WHERE IS THE REALIGNMENT?

According to Stonecash and Agathangelou, there was a realignment to the Republicans in the North in the 1890s that eroded in the 1920s and was overturned by the New Deal realignment. We are all aware of these developments, but what is their relevance for my analysis, which covers the 1946–90 period? The 1946 starting point was not chosen just to avoid the war years but also in recognition that the New Deal realignment took some time to work its way through the country (Key 1959). I hoped that by beginning in 1946 I would confine the analysis to the stable phase of a party system, which allows the individual state intercepts to capture the average or historical level of Democratic support in the state during the period (net of the other variables included in the equations).

Stonecash and Agathangelou repeatedly assert that the period I studied in fact cuts across a secular realignment to the Democrats, but they provide no evidence for their assertions beyond an observation that congressional voting has polarized by income between 1952 and 1980. They conclude that the latter "improved the fortunes of Democrats in the North" (p. 150), a conclusion that is not obvious to me, given declining turnout and the well-known propensity of people with lower incomes to vote at lower rates than do the more affluent. Moreover, their hypothesed pro-Democratic realignment is doubtful on its face. As noted in my 1994 article, the greatest part of the rise in Democratic legislative strength took place between the early 1960s and mid-1970s, a time of severe strain for the Democratic coalition.

The American National Elections Studies (NES) provide more direct evidence. Figure 2 shows the distribution of party identification in the nonsouthern and nonborder states between 1952 and 1992. Evidently, there is no evidence of a secular realignment to the Democrats during this period.

Stonecash and Agathangelou may firmly believe that the increased Democratic strength in northern state legislatures is due to an underlying realignment, but they offer no reason for us to share their belief. In the absence of evidence distinct from the dependent variable itself, claims that realignment explains increasing Democratic legislative strength are tautological. Thus, Bullock (1988) attempts to identify regional realignment by examining trends in partisan control of offices. But if the hypothesized *result* of a realignment is used to indicate its *presence*, we are locked in a circle from which there is no empirical escape.

Of course, future research may produce direct evidence of realignment during the period under consideration. But as I indicated (1994, 311), I am skeptical that the trends in northern state legislatures reflect secular realignment. On the contrary, it seems to me at least as likely that the hypothesis of a secular realignment is a reflection of the observed increase in Democratic state legislative representation that may well have other causes, including professionalization.

Whether my skepticism is justified is immaterial, however, for contrary to Stonecash and Agathangelou's claim, I do understand the difference between causation and correlation, and the statistical findings I reported do not confuse the two. Rather, my results indicate that *over and above* any realignment that might be occurring, professionalization matters.

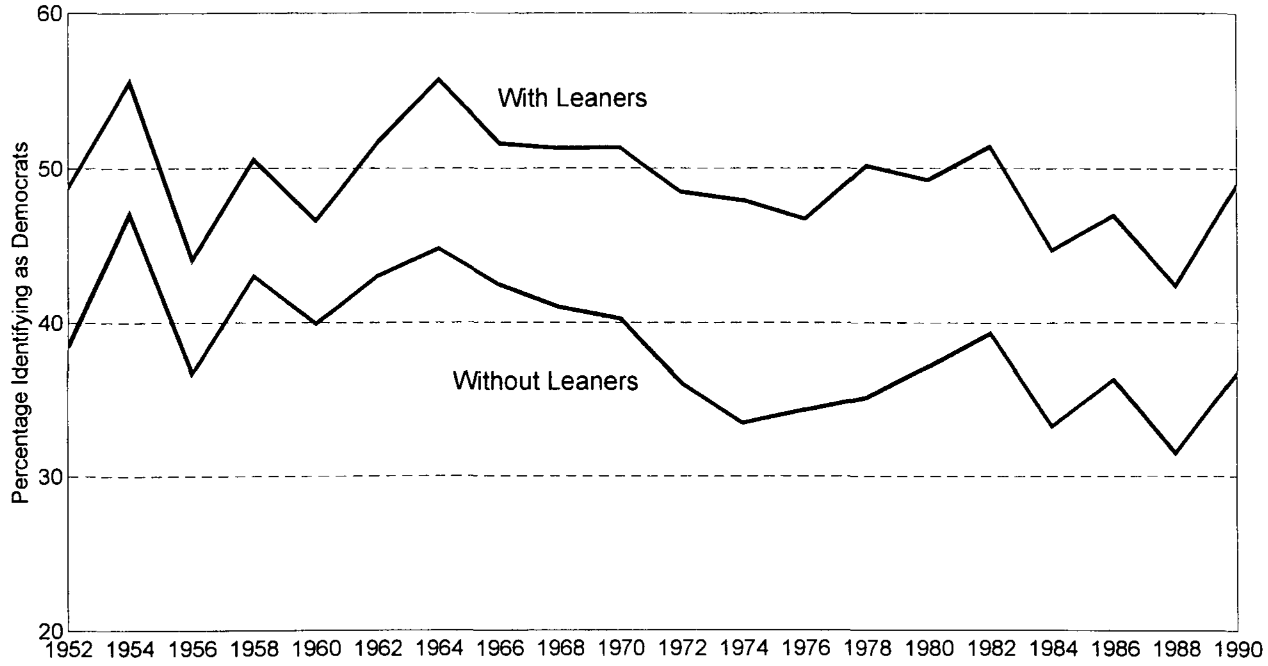
To explain, recall that the regressions in my article (1994, Table 2) include the percentage of Democrats in the preceding legislature, presidential and gubernatorial fortunes in the state, and (in equation 1) a time trend. The regression coefficients on these variables would not provide direct estimates of realignment (except perhaps for that on the time trend), but if any secular realignment were going on, it would be picked up by these variables. Stonecash and Agathangelou recognize that, but regard this as a further reason to leave such variables out of their regressions, since they would be "theoretically and statistically redundant (p. 152).[10] Redundant in what sense? These variables might be redundant if Stonecash and Agathangelou had a measure of partisan realignment, but they do not, and in the absence of state-by-state, year-by-year data on

---

[9] Kurtz (1991) measures professionalization with three indicators: length of session, legislator compensation, and size of staff. He categorizes nine legislatures as "red" or professionalized, eighteen as "blue" or nonprofessionalized, and the remainder as "white," or in between.

[10] Stonecash and Agathangelou seem to have discovered the one source (Gordon 1968) published in the past thirty years that advocates omitting important variables from equations.

FIGURE 2. Democratic Party Identification, 1952–90, outside of Southern and Border States



Source: National Election Studies.
Note: Entries are percentage Democrats of all respondents.

party identification, party registration, or other proxies for underlying partisan sentiments, these so-called redundant variables are in fact the only way to pick up any realignment that might be occurring.

What my statistical results show is that over and above any secular realignment that would be incorporated in the various control variables, and over and above short-term forces incorporated in presidential and gubernatorial coattails, the national economy, and midterm effects, there is a statistically significant effect of legislator compensation, a professionalization variable. Moreover, the effect is substantively significant—the direct effect cumulated over the 1946–90 period averages falls in the 5 to 10% range in the more professionalized states.[11]

LARGER IMPLICATIONS

Stonecash and Agathangelou (p. 153) conclude with what appears to be the deeper basis for their critique of my article:

> Empirical analyses have implications, intended or not. What is perhaps most important about Fiorina's research is its underlying implication .... His argument is similar to that made by opponents of political "machines," which implies that the connection between constituents and legislators is tenuous .... The suggestion is that the election of Democrats does not reflect support from specific constituencies.

Such an implication is entirely in the eyes of my critics. If Stonecash and Agathangelou's charge were correct, my article would represent a complete repudiation of my previous work. In *Representatives, Roll Calls, and Constituencies* (1974) I questioned the then-prevailing view that representatives were largely unresponsive to largely uninformed constituents and suggested theoretical rationales for how responsiveness could coexist with the known empirical facts. During the mid-1970s, culminating in *Retrospective Voting in American National Elections* (1981), I challenged the prevailing view that national elections were largely reflections of arational social attachments and the personalities of the individual nominees, arguing for a mechanism through which relatively uninformed voters could enforce an acceptable degree of electoral accountability. In *Congress—Keystone of the Washington Establishment* (1977), I addressed aspects of representation that legislative scholars had overlooked and expressed a concern that assiduous representation on these dimensions could lead to an erosion of representation on more traditional policy dimensions. Finally, in *Divided Government* (1992, 1995), I took exception to the argument that divided government was an accident of congressional incumbency and suggested that there might be some policy basis for the divided government condition in which the country found itself.[12] Now, in 1997, Stonecash and Agathangelou accuse me of suggesting that electoral outcomes are unrelated to the interests and preferences of the constituencies who determine them.

---

[11] There is also an indirect effect incorporated in the generally rising values of the lagged Democratic seats variable.

[12] My fellow political scientists have been slow to recognize the empirical veracity of this argument, but it now appears to have gained the status of conventional wisdom: In the closing days of the 1996 campaign, the Republican National Committee put millions of dollars into ads urging voters to elect Republicans to Congress to check and balance President Clinton.

My critics do not seem to understand the nature of a marginal effects argument with its "all other things being equal" assumption. *All else being equal*, the move from an amateur to a professional legislature advantages Democrats relative to Republicans. That does not imply that the Democrats are overrepresented, let alone that there is no relationship between constituencies and those who represent them. In the Conclusion of my article (1994, 312–3) I explicitly state that my results cannot support any judgment of whether Democrats today are overrepresented in state legislatures. There are many sources of advantage and disadvantage. Perhaps the party based in poorer socioeconomic strata is always disadvantaged to begin with (in resources, for example), so that the shift from an amateur to professional legislature helps redress the balance. Or, to mention another obvious factor, analysts have found that the postwar electoral system was biased toward the Republicans (King and Gelman 1991), so the shift to professional legislatures would have helped offset this bias.

More than many colleagues I am convinced that U.S. politics is sufficiently responsive and accountable that in the long run in most places there are reasonably close links between constituencies, those they elect, and the policies the elected produce. But at some times and in some places the links between constituencies and elected officials can be weakened by any number of factors. Districting arrangements, campaign resources, and differential participation are factors that have received study. My 1994 article only called attention to the possibility that features of legislative life might also affect the linkage. It was a piece that invited further research not a veiled indictment of American democracy.

REFERENCES

Black, Earl, and Merle Black 1987. *Politics and Society*. Cambridge, MA: Harvard University Press.

Bullock, Charles S., III. 1988. "Regional Realignment from an Officeholding Perspective." *Journal of Politics* 50 (August):553–74.

Campbell, James E. 1986. "Presidential Coattails and Midterm Losses in State Legislative Elections." *American Political Science Review* 80 (March):45–63.

Carey, John M., Richard G. Niemi, and Lynda W. Powell. 1996. "The Effects of Term Limits on State Legislatures." Presented at the annual meeting of the American Political Science Association, San Francisco.

Chubb, John E. 1988. "Institutions, the Economy and the Dynamics of State Elections." *American Political Science Review* 82 (March): 133–54.

Fiorina, Morris P. 1977. *Congress: Keystone of the Washington Establishment*. 1st ed. New Haven, CT: Yale University Press.

Fiorina, Morris P. 1974. *Representatives, Roll Calls, and Constituencies*. Lexington, MA: D. C. Heath.

Fiorina, Morris P. 1981. *Retrospective Voting in American National Elections*. New Haven, CT: Yale University Press.

Fiorina, Morris P. 1992. *Divided Government*. New York: Macmillan.

Fiorina, Morris P. 1994. "Divided Government in the American States: A Byproduct of Legislative Professionalism?" *American Political Science Review* 88 (June):304–16.

Fiorina, Morris P. 1996. *Divided Government*. 2d ed. Needham Heights, MA: Allyn & Bacon.

Gordon, Robert A. 1968. "Issues in Multiple Regression." *American Journal of Sociology* 73 (2):592–616.

Key, V. O., Jr. 1959. "Secular Realignment and the Party System. *The Journal of Politics* 21 (May):198–210.

King, Gary, and Andrew Gelman. 1991. "Systematic Consequences of Incumbency Advantage in U.S. House Elections." *American Journal of Political Science* 35 (March):110–38.

Kurtz, Karl T. 1991. "Understanding the Diversity of State Legislatures: The Red, White, and Blue Legislatures." Denver: National Conference of State Legislatures.

Simon, Dennis M., Charles W. Ostrom, Jr., and Robin F. Marra. 1991. "The President, Referendum Voting, and Subnational Elections in the United States." *American Political Science Review* 85 (December):1177–92.

Stonecash, Jeffrey M., and Anna M. Agathangelou. 1997. "Trends in the Partisan Composition of State Legislatures: A Response to Fiorina." *American Political Science Review* 91 (March):148–61.

BOOK REVIEWS

Political Theory

Lenin, Hegel and Western Marxism: A Critical Study. By Kevin Anderson. Chicago: University of Illinois Press, 1995. 311p. $49.95 cloth, $15.95 paper.

G.W. Smith, *University of Lancaster*

In creedal movements, theory frequently becomes an urgently practical matter. This was never more true than for Lenin, when in 1914 the unexpectedly nationalistic behavior of the working classes of the belligerent states struck at the very foundations of Marxian doctrine, bringing into doubt the basic principle of international proletarian class struggle and sending the Marxist political compass spinning. In these terms Lenin's ensconcing himself in the library in Bern between August and December, although superficially at odds with the popular Napoleonic *on s'engage et puis on voit* image of the great revolutionary's consummate political pragmatism, is by no means odd. What is puzzling is his choice of reading matter, namely, Hegel and, of all his writings, the rebarbative and icily abstract *System of Logic.* Out of this encounter came Lenin's *Philosophical Notebooks*, the pivot of Kevin Anderson's *Lenin, Hegel and Western Marxism.*

Anderson's explanation is that the events of 1914 convinced Lenin that international society had entered a period of convulsion far too seismic to be grasped in the mechanical categories of Second International orthodoxy. Only the cataclysms of the great French Revolution bore comparison with contemporary developments, and it was precisely Hegel who, struggling to comprehend the deep causes and ramified consequences of the earlier revolution, evolved a method which, although vitiated by Idealism, nonetheless provided the vital clue to the dynamics of societies caught in the birth pangs of profound and violent epochal change. And as the key to Hegel is the dialectical method, most elaborately developed in the *System of Logic*, Lenin's unorthodox choice of reading matter can be readily understood as a bold attempt conceptually to re-jig Marxism in the face of developments which he recognized as beyond the explanatory scope of theoretical orthodoxy, the dialectical materialism inspired by Engels, formalized by Plekhanov, and officially subscribed to by Lenin himself. For Anderson, then, the real puzzle, the puzzle which sets the terms of his argument, is not Lenin's resort to Hegel but the subsequent fate of the *Notebooks.* Why have these important writings remained largely unremarked, and why has Lenin's reputation remained obstinately linked with antihumanist Soviet orthodoxy, and even with Stalinism?

Anderson's problem makes perfect sense, of course, against an image of Lenin as a bold and inventive theorist, brilliantly anticipating the central preoccupations of the subsequent Western Marxists (Lucaks, Korsch, Marcuse, Gramsci, etc.) by initiating a pathbreaking recovery of the dialectical method that Marx himself had originally critically appropriated from Hegel, but that subsequently had been misunderstood by lesser men living in quieter times. But even Anderson is not prepared to subscribe unqualifiedly to this picture. Although he cannot entirely resist the temptation to compare the unpublished *Notebooks* with Marx's unpublished writings on Hegel, Anderson is too sensible to claim they really compare, either in substance or in quality. Indeed, the *Notebooks* are notably exiguous, comprising excerpts (frequently fairly lengthy) from the chapters on Being, Essence, and Notion in the *Science of Logic*, together with an incohate patchwork of Lenin's comments, some of which are of paragraph or, occasionally, page length, but most of which are little more than telegraphic one-liners. So, what we have here is a thoroughly incondite text out of which it would be unreasonable to expect to derive a clear or connected argument. Indeed, in the event, Anderson proves to be significantly more agnostic as to their value than his initial thesis might suggest.

The *Notebooks* are subjected to careful and intelligent scrutiny in the first three chapters of the book. Anderson gets to the heart of the matter of Hegelianized Marxism by putting his finger on the key issue, namely, Lenin's treatment of subjectivity. Whereas the Second International orthodoxy taught that dialectics is a science of laws deterministically applying to an objective world which includes both nature and human beings, Lenin catches the significance of the Hegelian position, according to which dialectics is the science of subject-object relations and of the dynamic principles governing their mutual interaction. Once acknowledged, the dimension of subjectivity opens up the possibility of a moment of self-transformation and hence of a degree of active intervention impossible in terms of positivistic materialism. Lenin follows Hegel further in conceiving self-activity in a thoroughly dynamic way—change occurring by way of internal developments in the subject which induce contradictory resistances, generating increasing internal tensions resolvable only by a sudden and explosive "transition to the opposite" (a revolutionary "leap").

There is no denying Lenin's intellectual boldness in his engagement with Hegel, but Anderson's thesis rests on the claim that the *Notebooks* anticipate major themes of Western Marxism, and this simply cannot be sustained. Consider Lenin's treatment of the central issue of the relation of Hegelian Idealism to materialism. Anderson argues that we can discern in Lenin the beginnings of a critique of "one-sided materialism" à la Marx of the first *Thesis on Feuerbach*, and hence an implied rejection of the positivistic dialectics of Engels and Plekhanov. Whereas Marx clearly appreciated, however, that the incorporation of "activity" entailed accepting consciousness as an irreducible moment within a modified materialism, there is no sign that Lenin is prepared to take this crucial step, or that he even grasps its significance. This is evident from his treatment of Hegel's transition from Mind to Nature. As Anderson himself points out (pp. 94–6), Lenin perversely regards the transition as involving the implicit reduction of the former to the latter, a move which entails, of course, a regression to a thoroughly undialectical (Spinozistic) materialism—precisely the kind of objectivity presupposed by Second International doctrine and against which Hegel's transcendental Idealism is primarily directed. The fact of the matter is that Lenin remains throughout firmly in the thrall of the positivist presuppositions of Russian Marxist orthodoxy. Anderson is too sharp not to recognize the significance of Lenin's failure here. But, once admitted, need we look any further for an explanation of the subsequent lack of intellectual effect of the *Notebooks*?

Lenin never published the *Notebooks* and never publically repudiated party orthodoxy on materialism. Nonetheless, Anderson maintains in the two illuminating chapters following, and with a good deal of plausibility, that Lenin's "ambivalent and secretive Hegelianism" (p. 98) bore a significant, if indirect, influence upon his subsequent politics. Thus, the key to understanding *Imperialism* lies in seeing that the development of capitalism to its monopoly stage both postpones and intensifies its internal contradictions, on the one hand producing worldwide class oppression and, on the other, creating the conditions for the "leap" to its opposite—an international revolutionary proletariat. Likewise, in *State and Revolution*, the razing of all bourgeois political institutions and establishment of an omnicompetent workers' state represents the similarly dialectically induced response to the threat to revolution represented by the worker aristocracy created by monopoly capitalism. In both instances what appear to be set-backs in reality represent an intensification of conflict presenting opportunities for decisive revolutionary intervention, though Anderson frankly points out that Lenin's insights concerning freedom and subjectivity unaccountably desert him when it comes to the question of democracy and the vanguard party (pp. 166–9). Indeed, one cannot help but feel that, as with pretty well everything Lenin wrote, his flirtation with Hegel has ultimately to be understood in terms of adroit rationalization of political instincts and party interests; it is surely no accident that his brief and highly tendentious rendering of Hegelian subjectivity perfectly suits his conception of politics as revolutionary activism.

The final chapters of this intelligent but oddly misfiring book are devoted to a densely informative account of the (for Anderson largely unjust) vagaries suffered to the reputation the *Notebooks* at the hands of the likes of Lukacs, Korsch, and Lefebvre as they swivelled to the changing winds of new necessities. Only Raya Dunayevskaya comes across as constant in her commitment to the vital importance of the Hegelianized Lenin. As for the author, he strikes one as a man whose head is not entirely at ease with his heart.

Foucault and Political Reason: Liberalism, Neo-liberalism and the Rationalities of Government. Edited by Andrew Barry, Thomas Osborne, and Nikolas Rose. Chicago: University of Chicago Press, 1996. 288p. $17.95 paper.

Jodi Dean, *Hobart and William Smith Colleges*

Anyone interested in Foucault or the extension of Foucauldian methods of analysis will benefit from sustained engagement with this excellent collection of essays on governmentality. That said, I am interested in the broader question of how the rest of us may benefit from *Foucault and Political Reason.* Why should someone who has not kept up with the voluminous Foucault literature expend time on this collection, which grows out of work that Foucault began late in his life and that appears mostly in scattered lectures and interviews? The book's subtitle provides a general reason: because this volume dissects the political practices and rationalities informing most European and English-speaking countries today. It interrogates the conditions in which freedom is said to be practiced, the contexts in which what is understood as freedom is produced as effect, possibility, demand. Thus, the more specific reason to read this collection is that the authors extend Foucault's work in such a way as to enable us to see liberalism differently, to move beyond foundational personages and texts and confront liberalism as an ensemble of specific techniques and problematizations.

Although these essays lack a degree of clarity and, indeed, disagree about what the liberalism is that they are discussing (a disagreement and unclarity also manifest among liberalism's theorists and supporters), one might say that if liberalism is understood as relying on the natural, then these essays ask how the natural was created. As several of the authors acknowledge, this question has been put to liberalism before, quite powerfully by feminism. Nonetheless, the inquirers in this volume distinguish themselves from previous explorations of liberalism in two ways. First, in keeping with their Foucauldian perspectives, these essays tend to refrain from articulating their studies with normative judgments. Second, as Graham Burchell theorizes explicitly in his chapter, they address various intersections of techniques of the self and technologies of domination.

Although the reader often suspects that Vikki Bell's account of freedom, say, or Nikolas Rose's analysis of the neoliberal response to the welfare state is not value free, precisely what the author espouses is concealed. For some readers, this failure to explore the rightness of a given set of practices will no doubt be quite maddening. Indeed, in places the effort toward value freedom effects an awkward theoretical gymnastics. Despite his insistence on the critical importance of "the problematization of what is given to us as necessary to think and do," Burchell explains that value freedom means "conformity to the procedures and criteria for doing evidential adequacy, conceptual and argumentative coherence, descriptive adequacy, appropriateness of method to material and problem," and so on (p. 32). My reading of the early Foucault as well as thirty years of feminist theory suggests that requirements of procedural conformity, conceptual coherence, and methodological appropriateness are products of intellectual and material arrangements of knowledge that cannot be divorced from the workings of power. Interrogating these requirements, moreover, denaturalizes them, contributing to our understanding of their relationship to and implication in power. Since Burchell presumably agrees that such an interrogation is necessary, his defense against possible charges of irrationality ends up as a lost opportunity to explore the politics invested in claims to rationality.

In contrast, Rose's elaboration on the politics of value freedom articulates normative restraint with an effort "to disturb those political logics of Left and Right within which judgement is easy" (p. 61). His analysis, like Barbara Cruikshank's fascinating account of the self-esteem movement in the United States, carefully explores the techniques of freedom that call for and are entailed in persons becoming aware of and responsible for themselves. In each chapter, the authors delineate the discourses and practices within which the contemporary focus on self-realization is constituted, making us aware of the contingency of this focus without necessarily rejecting it. As the editors explain in the introduction, today, since the death of state socialism, we face the "proliferation of political doctrines and programmes that are unstable and difficult to classify in conventional terms" (p. 1).

Understanding these doctrines and programs, the technologies through which they are implemented and audited, and the resultant effects on individuals and populations is necessary prior to formation of normative judgments. Thus, following the first set of essays (Burchell, Rose, Bell, and Barry Hindness, who discusses the paradox between the natural and artifactual notions of autonomy in liberalism), which take up the broad question of liberalism as a particular strategy of government, are detailed analyses of the local devices used in liberal government. These essays might be read as contributing to a Foucauldian notion of civil society. Insofar as they highlight the myriad practices and relationships within which individuals act and are acted upon, they detail the fabrication of the social space created for liberal government. That is to say, they explore how the activities of experts and the workings of technologies separate out and constitute a domain of human experience as that civil society which will delimit the scope of liberal government even as it serves as the target of governmental goals and policies.

In addition to Cruikshank's discussion of the self-esteem movement, Thomas Osborne contrasts the rise of a concern with public health to eighteenth century emphases on individual health. He sees the drains and sewers built by the Victorians as "a strategy of indirect government" that freed "the city of the detritus that should be external to it" and thereby established it as a vital and natural order (pp. 114–5). Ian Hunter stresses the contingency of school reform. His genealogy works critically against idealist liberal-democratic conceptions such as Amy Gutmann's as well as against Marxist notions of schools as vehicles for the reinscription of the interests of the dominant class. Hunter's basic claim is that the school system emerged piecemeal through exchanges between a state conception of the school as a tool of social government and a Christian emphasis on spiritual discipline. Rejecting efforts to establish liberalism through its separation from feudalism, Alan Hunt provides a nuanced account of sumptuary law. Pat O'Malley examines changes in the management of risk from welfare liberalism to late or advanced liberalism.

Given that the editors highlight the importance of technology and expertise for liberalism, the way liberal government is able to refrain from direct intervention in the lives of citizens to the extent the experts and technologies become authoritative, I was disappointed that the anthology lacked chapters on the computer and communication technologies so rapidly coming to frame experience for many at the turn of the millennium. The chapter that comes the closest to addressing these issues is Andrew Barry's fascinating account of the importance of the telegraph for liberal government. Whereas the more familiar Foucauldian (or paranoid) analysis stresses the place of communication technologies amid practices of surveillance, Barry articulates the growth of popular means of communication with liberalism: "If liberalism was suspicious of excessive State intervention and of the capacity of government to act, an effective communication and information system enabled the public authorities to judge the minimum level of action necessary" (p. 128). For Barry, communication technologies like the telegraph made possible the virtual community, the public sphere, that was/is the liberal state. Consequently, his conception of this public sphere is attuned to the effects of technology. Not only does he draw attention to the limited ability of the telegraph "to inscribe the reality of . . . distant events," but also he points out its influence on the contents of messages ("the telegraph was limited by its capacity to transmit only the shortest messages") and the norms of behavior of the lay public who followed developments in wireless technology ("non-scientists came to expect themselves to embody the self-discipline of science") (pp. 133–5).

These essays can be frustrating, resembling the techniques of accounting and audit Rose finds in advanced liberalism: They are "simultaneously modest and omniscient, limited yet apparently limitless in their application to problems as diverse as the appropriateness of a medical procedure and the viability of a university department" (p. 54). Accordingly, one wonders whether they are theory or history (a question that has, as Colin Gordon explains, plagued the British reception of Foucault). Nonetheless, altogether they are thoughtful and provocative, raising issues relevant to anyone interested in liberal government.

Serpents in the Sand: Essays on the Nonlinear Nature of Politics and Human Destiny. By Courtney Brown. Ann Arbor: University of Michigan Press, 1995. 175p. $34.50.

L. Douglas Kiel, *University of Texas at Dallas*

In any discipline it seems increasingly rare to read a book that genuinely challenges, edifies, and inspires. Courtney Brown's important recent work is an example of that rarity. It is a shame that this review must be categorized within a subfield of political science, for Brown's book speaks to the entire discipline. While its title may suggest biblical references to human nature and the character flaws of the politically ambitious, it instead refers to the nonlinear and dynamic nature of evolving political reality and its interaction with a malleable environment.

In this reviewer's experience, Brown makes the first comprehensive effort to apply the emerging field of nonlinear dynamics to political evolution. His proclaimed task is to present a nonlinear *Weltanschauung* that directly challenges the dominant "linear" world view. This dominant world view, which sees relationships between variables as largely stable and consistently proportionate in their outcomes, is seen by the author as the result of efforts to ease analysis and simplify computation at the cost of discounting the real complexity in human systems. Our reliance in political science on optimization models, such as linear regression, serves to confirm overly simplistic views of relationships in the political realm that taint our analyses and diminish the value of policy recommendations.

As a subfield of the sciences of complexity, nonlinear dynamics examines both the internal and contextual elements that both energize, enervate, and alter system dynamics. Brown thus seems to see as his task the development of analytical models of political dynamics that incorporate historical and cultural milieus but also include the nonlinearity that is an essential element of the human realm. He accomplishes his task in a convincing manner that should awaken many political scientists to the value of appreciating both the nonlinear nature of political evolution and the related methods that can enhance our view of the complexities of that evolution.

Brown's chapters, which are extensions of some of his previously published works, are used to display the multiple dynamics that nonlinearity may generate in political systems. These temporal dynamics range from stability to chaos to catastrophe for political systems. His analysis of voter rationality within the context of the individual's social context evidences nonlinearity in attitudinal change. An analysis of Lyndon Johnson's 1964 electoral landslide over Barry Goldwater reveals the nonlinear and contagious effects of multiple nonlinear interactions. Brown's assessment of the fall of the Weimar Republic shows that the nonlinear National Socialist shock to an already unstable system led to systemwide catastrophe. In a chapter of current interest to students of public policy, Brown reveals the complex periodicities and pseudorandom (chaotic) dynamics in environmental damage generated by alternating partisan control of electoral structures and regulatory systems.

Brown does not want his work to be seen as an essay in methodology. Yet, in order to reveal the weaknesses of linear regression in capturing political complexity, in light of the relative strength of nonlinear regression methods, he devotes a sizable portion of the book to the development and results of his modeling efforts. This comparative analytical approach is actually a plus in this book, as Brown shows the richness of the political landscape that can be explored using nonlinear regression methods. It is worthy of note that these comparative analyses may be demanding reading for those who are not methodologically sophisticated. The method of plotting data on phase diagrams can be challenging for those not accustomed to the graphical data presentation widely used in nonlinear dynamic analysis. A more extensive detailing of nonlinear systems theory and associated methods of nonlinear dynamic analysis would have resolved these issues for some readers. Brown, however, does a very effective job of conveying the need for and the value of the method he espouses.

As do other social scientists interested in applications of nonlinear dynamics to society, Brown believes that his emergent perspective may lead to a convergence of the sciences, much akin to that of the ancients. While some analysts may consider such notions the extremity of wishful thinking, one must recognize that a greater appreciation for the nonlinearities in the political world is likely to produce a political science more adept at making statements about what truly is stable, linear, unstable, and nonlinear in the social realm. The essential point is that since the human realm is highly nonlinear, political science must do a better job of incorporating the fundamental dynamics of human social interaction.

Abiding by Brown's recommendations for incorporating nonlinear analyses into political research presents a considerable challenge to most political scientists. Brown admits that developing nonlinear models is time consuming both in human labor and for high-speed computers. The psychic cost for some of moving from independent, linear, and mechanical models of people and social systems to contextual, nonlinear, and living systems models may also be wrenching. Brown's urgings, however, are not an arrogant attack against an entrenched status quo, but instead represent the language of new discovery asking others to participate in new forms of exploration.

Brown is asking for nothing less than a Kuhnian shift in political and social science. Such shifts inevitably take time. The next generation of political scientists, all of whom should be required to read Brown's works, may learn better than their predecessors to capture the nonlinear and complex realities of evolving political systems. This process of generational improvement is, of course, a basis for the evolution of the discipline. Perhaps the larger question raised by Brown's work is whether, like most pioneers, he receives multiple arrows in the back, in this case, arrows of neglect, or the acclaim he deserves for attempting to push research in politics to a new and much richer domain.

Gender Is Not a Synonym for Women. By Terrell Carver. Boulder, CO: Lynne Rienner Publishers, 1996. 133p. $35.00.

Christine Di Stefano, *University of Washington, Seattle*

Terrell Carver proposes to redress a deficiency of major proportion in contemporary political theory. While crediting feminists for introducing "gender" into the political theory lexicon, Carver argues that a gendered perspective on men has not as yet been sufficiently developed. Rather, feminist efforts to recuperate and refigure the role of women in political theory have proceeded with reference to a suspect background figure, "the monotonic, monolithic, and yet paradoxical degendered male as other" (p. 119). Just as females are not the only sex, women are not the only gender. Furthermore, just as feminists have worked to specify women further in terms of race, sexuality, class, and other constitutive and contested markers of identity, oppression, and resistance, so, too, do we need to look for "alternative and suppressed masculinities" (p. 8). "Masculinity" in the singular simply will not do as a covering law generic term for all or most men (and some women) and their behavior.

Carver distinguishes gender-critical analysis from gender analysis simpliciter in order to establish a crucial methodological difference among various theoretical invocations of gender. Absent the notion of "critical," gender analysis risks "capitulating to the universalizing and naturalizing narratives that construct the gendered world and insulate it from political challenge" (p. 36). Carver employs "a mild form of postmodernism" (p. 119), along with insights generated by the sociology of masculinities, to render "men and their masculinities more varied in terms of domination and subordination within the male group, as well as with respect to women" (p. 119).

To the extent that a cursory conception of men informs feminist constructions of women, "strange things happen" in feminist work. "Woman" is defined as nonmale, "that is, as womb-having" (p. 9), and men disappear from the scene. A conception of women as womb-having risks reinscribing the naturalized, essentializing, and reductionistic terms of traditional femininity, while the disappearing act recapitulates a previous and all-too-familiar absence: "Men's reproductive capacities and parental roles also tend to disappear in feminist theory, just as in traditional political theory these roles are generally forgotten" (p. 9). As long as we persist in forgetting that these roles have been forgotten, reproduction will continue to be mapped onto women, and men will be let off the hook.

Carver's working definition of gender as "the ways that sex and sexuality become power relations in society" (p. 120) and his attunement to the interdefinition of sex and sexuality with class, race/ethnicity, and other phenomena are deployed consistently, persuasively, and innovatively throughout the five essays in this collection, which is unusual and eclectic by mainstream political theory standards. It includes a literature review of feminist theories of politics and postmodern theories of gender; a critical rereading of Engels's *The Origin of the Family, Private Property and the State*; a biographical study of Engels that pays close attention to his relationships with women; a reinterpretation of the scandal surrounding the rumor of Marx's illegitimate son; and an interpretation of the Clarence Thomas–Anita Hill hearings, where, as Carver observes, private man was forced to go public against his will. The collection as a whole also enacts Carver's prescriptions for relaxing the form and broadening the content of political theory so that it is more directly engaged with cultural materials that are all too often taken for granted. Among those "commonalities of the lifeworld shared by theorists, classic authors, and political actors" (p. 121), men and masculinity/ies are particularly "normal" and "obvious" and therefore worthy of subversive attention by political theorists.

In his cross-grained readings for men and masculinity/ies (which is to say that his readings are never on sympathetic behalf of the masculinity/ies that he pursues), Carver comes up with a number of interesting and provocative findings. For example, Engels's classic text, "while appearing to be a history focused on women" (p. 40), leaves men in the driver's seat of history. Furthermore, it exculpates conventional notions of masculinity by figuring men as natural sexual predators. Carver argues that the feminist pedigree enjoyed by *The Origin of the Family* merits critical reassessment.

Conventional commentary in political theory ignores biography altogether or treats it as a background footnote to the classic texts. In his biographical essay on Engels, Carver works in the opposite direction, from the lifeworld of the author to the classic (and not-so-classic) texts. Using an interpretive frame of masculinity, gender politics, and sexuality, Carver pays close attention to Engels's sexual politics and practice. The point is not to discredit Engels's political theory on the basis of a demonstrated "contradiction" between his professed political beliefs and his bourgeois heterosexual masculine behavior with respect to his working-class lovers, but to rewrite the narrative of political theory so that the political theorist is "not just an author of books and pamphlets, but an author of power relations" (p. 77).

Carver revisits the story of Marx's "illegitimate" son as it has been handled by a number of biographers in order to explore the idea that "gender . . . is a feature of intellectual communities, and not merely of the people that intellectuals write about" (p. 79). While weighing in with his own verdict on the question of Freddy Demuth's paternity (which I will not give away in this review), Carver is actually more interested in the class positions of those involved and the various ways that gender politics was, or might have been, practiced within the Marx family setting. He also draws attention to the gender politics of the biographers and to the ways that their characterizations of the affair have stuck as a tradition.

Carver's fascination with "good stories" and the "intertextuality of political theory and lived experience" (p. 99) carries over into his interpretation of the Thomas-Hill hearings as reported by the *New York Times*. One of his most interesting findings is that Clarence Thomas and his allies used a "strategy of narrative multiplication" (p. 100) to discredit Anita Hill and that this strategy is available to men who want to disadvantage women. Carver also observes that Thomas had another card up his sleeve besides the race card. By claiming reputation rape, Thomas played "the 'Tootsie card': men are better at being women than women are" (p. 112).

With this collection of essays, Carver has made a significant contribution to feminist and gender-critical analysis in political theory as well as to Marx-Engels scholarship. Furthermore, he has demonstrated that political theory can and ought to be practiced with a variety of materials, ranging from classic works and authors to the texts of contemporary culture. Finally, his "incorporation of a gendered perspective on men into political theory" (p. 119) is a stimulus to further research on political man, private as well as public.

The Idea of an Ethical Community. By John Charvet. Ithaca, NY: Cornell University Press, 1995. 226p. $35.00.

Steven Kautz, *Emory University*

In this antirealist defense of Rawlsian contractarianism, John Charvet's starting point is the "abandonment of the realist perspective on the good" that characterizes much of contemporary moral and political theorizing. Moral realism is bankrupt, says Charvet; human beings must now recognize that "their social practices have no other ground than their own reason and will" (p. 3). But this recognition, which is perhaps the defining moral quality of our age, gives rise to a moral and political crisis, as Charvet admits, if only because we have for so long been in the habit of justifying our own (liberal) principles and practices by means of (moral realist) appeals to their imagined religious or philosophical foundations ("myths," says Charvet [p. 1]). (Consider the Declaration of Independence.) If we must now be antirealists, can we nevertheless defend our particular ethical life as liberals committed to what Charvet calls a "strong equality principle"? More generally, asks Charvet, how is ethical life possible at all—since it depends upon the recognition of "authoritative" moral norms—if shared norms are simply a "construction or invention of human beings themselves", acting in societies, as the antirealist holds (pp. 1–2)? What is the ground of the *authority* of such norms?

Charvet argues here that the contractarian strategy of the early Rawls, which is properly understood as an antirealist (rather than a Kantian realist) strategy (see pp. 163–73), can solve the problem of the authority of shared norms after antirealism; in particular, Rawls's early strategy is superior to that of the communitarians (as well as to that of the later Rawls). Charvet's book is thus a valuable contribution to two persistent debates among political theorists and moral philosophers today: the debate concerning whether fidelity to liberal democratic principles can be defended on the basis of the prevailing antirealism of our time; and the debate between Rawlsian liberalism and its communitarian critics.

Communitarians are antirealists, too, but their antirealism goes both too far and not far enough. On the one hand, communitarians and other antirealists often argue that socialization goes "all the way down." They thus abandon the very idea of "*human* being" itself, argues Charvet: "There could be only French beings, or British beings" (p. 3). But such relativism is in no way required by antirealism (pp. 2–5, 13–5), and so Charvet undertakes to defend a "*generalizing* antirealist ethical theory" that yields general and perhaps even universal principles (p. 4, emphasis added; cf. pp. 29–33, 65–6, 74–80, 85, 153–73, 189–92). (Communitarians are "above all" hostile to this generalizing impulse, Charvet rightly argues.) Strong antirealism goes too far because it understands moral choice as "ungrounded choice." This is a "morally desperate surrender to the powers of arbitrariness"; it certainly cannot vindicate the cause of equality against, say, racist and fascist defenders of inequality (p. 2). Yet, and as a consequence of the first error, the communitarian is compelled to defend an intolerable political position: "Each is to follow the norms of his society." This political principle, though antirealist in form, is surely far too indulgent of traditional forms of moral realism, says Charvet. Such a "blindly conservative and unreflective" principle ordinarily yields "a desperate appeal . . . to traditional authorities, even when those authorities adopt realist commitments" (pp. 4–5). More generally, communitarian theory seems to be incompatible with "the possibility of an individual's distancing himself from his communal arrangements" (p. 192); but this also is in no way required by antirealism, or even by the view that human beings are everywhere constituted by the societies in which they happen to find themselves, since at least some societies make possible the emergence of a self-conscious rationality that enables human beings to acquire reflective distance from the very society that has so constituted them (e.g., pp. 154–5, 83, 77–8, 65–6). That is, it is possible to acknowledge the communitarian insight that our moral beings are constituted through particular forms of social life, argues Charvet, without thereby abandoning the aspiration to achieve a critical distance from one's own society. A more consistent antirealism, one that is capable of sustaining rational self-criticism, is required. Charvet advances such an antirealism in his reconstruction of Rawlsian contractarianism (see p. 6).

Charvet's conception of the opposition between realism and antirealism is remarkable and perplexing, combining as it does many of the advantages of each. Thus, Charvet does not hesitate to speak of certain general powers and interests that belong to human beings as such and even of a "general human nature," from which "normative conclusions of universal import" might be drawn (pp. 3–4; see also pp. 71, 154). He speaks as well of our "essential human identity," which can be distinguished from "culturally specific features" of our identities because of its connection to certain "natural interests" or "natural goods" of human beings (pp. 77–9; cf. 194–5). He even makes use of quasi-Aristotelian talk about the "well-functioning of the [human] organism," "the fundamental drive of the organism to be directed toward its own good—its flourishing or self-affirmation," and "the aspiration of the organism to its good" (pp. 30–2; see also p. 10). In these respects, Charvet seems to borrow from the arguments of the moral realists; in any case, many antirealists will find it difficult to reconcile Charvet's subjectivism regarding the good with his claims about the universality of certain human interests and goods (but cf. p. 13). Yet, in his impressive reconstruction of the Rawlsian contract (see especially pp. 167–73), Charvet also insists that only an "existential commitment" can justify our embrace of Rawlsian impartiality and equality constraints on our deliberations in the original position (pp. 172–3; cf. p. 80). Indeed, our commitment to equality is after all a kind of "ungrounded choice" (p. 172; cf. pp. 2, 84), not strictly justified by reference to an idea of human nature; but because this choice is made reasonably only in common with others who share an existential commitment to a particular moral community, and because it is now possible to undertake that commitment in a fully self-conscious manner, it is, says Charvet, "no blind and irrational leap in the dark" (p. 172; cf. p. 85).

However that may be, the great merit of this book is to enable readers to think through the complex relation between antirealism and Rawlsian liberal theory, which together dominate the world of contemporary political theory. And there are valuable discussions of particular issues in political theory and moral philosophy throughout (e.g., on autonomy, pp. 63–85; on the relation between self-interest and morality, pp. 89–110; on the Rawlsian social contract, pp. 153–73). If Charvet, like Rawls, "speaks only to persons who are already committed to the political norms he advocates" (p. 6), perhaps that is simply in the nature of the Rawlsian project.

The History of Political Theory and Other Essays. By John Dunn. New York: Cambridge University Press, 1996. 235p. $54.95 cloth, $18.95 paper.

Stephen L. Esquith, *Michigan State University*

This collection of previously published essays from the first half of the 1990s, some translated into English for the first time, has three notable strengths. Once again, Dunn demonstrates his firm grasp of the history of early modern political theory, especially Hobbes and Locke. In the process he clarifies his own historicist view of the relationship between theory and practice, and he then illustrates the contemporary value of this approach in several insightful topical essays.

In the lead essay, "The History of Political Theory," Dunn provides a schematic account of the key questions historicist political theory should raise. What did the author mean? What does the text reveal about the context of composition? What has the text meant for others? What does it mean for us today? These heuristic questions can help partially untangle the causal links among political, economic, and social institutions and practices *and* the values that motivate and organize human action across their boundaries. Neither social science nor moral philosophy consistently can make this claim. "It is the single greatest merit of the history of political theory today," Dunn believes, that it avoids a positivist separation of causal analysis and the study of values (p. 30).

In the next three essays Dunn shows how earlier political theorists actually had a more practical grasp of the nexus of causality and value then do most contemporary theorists. The essay on "Contractualism" makes a strong case for classical contract theory over its twentieth-century reproductions. The major weakness Dunn identifies in theories such as Rawls's is that they are "purely philosophical" in the sense that they do not provide citizens a practical framework of interpretation. Reducing social contract to a hypothetical thought experiment may justify some redistributive institutional arrangements over others, but not from the perspective of engaged citizens and not for long. Contemporary contract theory, much like utilitarianism and socialist theory, lacks an adequate conception of practical reason.

Dunn argues that political obligation and trust, not distributive justice, should be the focus of contemporary theory, just as it was for Hobbes and Locke. Unlike distributivists today, seventeenth-century theorists understood that for political obligations to hold, trust must be based on more than narrow calculations of advantage. Dunn recognizes the limitations of Hobbes's theory of obligation and the need for Lockean revisions in Hobbes's ideal state. However, what earns Hobbes Dunn's respect is the way he combines a concern for security with an appreciation of the fact that obligations must be formed and felt by individual agents themselves. They cannot simply by byproducts of fairness.

Turning from political obligation to freedom of conscience, Dunn then argues that Locke had a more usable understanding of its scope and value than did Hobbes. While it would be a mistake to attribute to Locke a general theory of toleration that encompasses the variety of expressions that come under the legal rubric of protected speech, Locke's (and Bayle's) arguments for freedom of religious conscience have a sharp political focus and strength that still can be tapped in some situations today. This separation of religious freedom from other freedoms of expression runs contrary to the Rawlsian strategy of using the argument for freedom of religious conscience as a model for other political liberties.

At this point Dunn reverses his field. Instead of moving from the history of political theory to its relevance for recurring problems, he directly confronts several contemporary problems in the more distant light of past theoretical work. The most well-aimed chapters in this section of the volume concern property ownership, foreign intervention, and racism. For example, in "Specifying and Understanding Racism" Dunn argues that despite their epistemic absurdities racism and race consciousness cannot be written off as merely unscientific "folk genetics." They have played integral roles in competitive interpretations of group interests (p. 151). Today, this is especially clear in political battles over closed borders and immigration policy. Who is to be trusted, the classical contractarian problem, is inextricably tied to the current problem of who is to be included as a citizen, neighbor, and fellow worker; and race consciousness is a key factor in this equation. The essays on economic interdependence, the nation-state, and the environment have more structure and provide a more comprehensive perspective, but they lack this kind of critical pressure. In all of these topical essays, however, Dunn recognizes that the powerful link between political order and economic growth forces him to take distributive questions seriously (p. 133). There is no danger of an either/or choice between distributive justice and political security. The volume closes with a brief and less than sanguine reflection on the prospects for a resurgent European left.

There is no need to rehearse Dunn's many past accomplishments. As the reader of this volume soon discovers, he is not shy about referring to his own published work. For those who want to, it is easy to retrace his steps. The flow also is interrupted by too many parenthetical qualifications and admonitions, and sometimes favorite words and phrases reappear too quickly. One more draft might have eliminated these distracting bumps in the road.

In substance there is much less to fault. While Dunn underestimates the unwanted practical effects of contemporary contractualism and utilitarianism (their methods of reasoning have helped to sustain a widening range of professional practices and to reconcile citizens to a consumerist orientation toward politics), he is right to take them to task for their democratically anemic conception of practical reason. And while some of his historical probes may put even the literate nonspecialist on the ropes, unlike some historicists, Dunn does not hide behind trade secrets or avoid pressing issues in the name of academic modesty. All of his methods are above board, and his views, though often hedged, are never sidetracked by trivial scholastic debates. One can quarrel with the details of his treatment of particular theorists and problems, but thankfully there is no burking the project.

Natural Law and Moral Philosophy: From Grotius to the Scottish Enlightenment. By Knud Haakonssen. Cambridge: Cambridge University Press, 1996. 386p. $54.95 cloth, $19.95 paper.

Martha K. Zebrowski, *Columbia University*

Modern theories of natural law are of two essential types. Fundamental to one are the natural rights of individuals. In a theory of this type it follows that the bearers of subjective rights create human law and institutions by means of mutual contracts. The first to pursue this line of argument was Hugo Grotius. Fundamental to the other are the natural law and the moral duties it imposes on individuals whose performance of these duties is necessary to the maintenance of a complex of social and political relations. Where a theory of natural law and duty entails individual rights, it comprehends them as complementary but logically subordinate to natural duties, as the powers individuals require to perform the duties the natural law imposes. The first to pursue this line was Samuel Pufendorf. Knud Haakonssen undertakes to disentangle the two strains of thought. He argues that the line Pufendorf established dominated natural law theorizing in Europe and North America in the seventeenth and eighteenth century.

Haakonssen picks up the story with the modern response to the Scholastic natural law tradition and a survey of seventeenth-century European natural law theories. He sketches the common concern Grotius and Pufendorf had to remove jurisprudence from theology and the distinct courses they proposed to an independent science of morals. But his greater interest is in the eighteenth-century Scots who sought a more comprehensive science than either Grotius or Pufendorf achieved. Chief among those he places in the Pufendorfian mainstream of Scottish moral philosophy are Francis Hutcheson, Thomas Reid, Dugald Stewart, and James Mackintosh. Outside the mainstream, and following in the course of Grotius, are David Hume, Adam Smith, and John Millar. In the final chapter Haakonssen looks at eighteenth-century American jurisprudence in light of Pufendorf and his Scottish successors and finds Americans, Thomas Jefferson in particular, straining to push through and beyond the theory of natural law and duty to a theory of subjective and inalienable natural rights. Haakonssen is clear that his study is not a comprehensive history of natural law, moral philosophy, or the Scottish Enlightenment, but he claims that in the seventeenth and eighteenth century the ideas and arguments he does consider were common throughout Protestant Europe.

Haakonssen's method is twofold. He seeks to locate each writer in his proper sociolinguistic context and to retrieve the meaning proper to his arguments in just that context. Rendering the contours of a joint intellectual work that was in progress for more than 200 years, Haakonssen recounts how each writer explored natural law and moral philosophy from a distinct theoretical perspective and with a distinct technical vocabulary, contributed this perspective and vocabulary to the general pool of resources available for thinking about natural law and moral philosophy, and, by virtue of the arguments he made and the theory he realized, established the essential point of departure for the critical and constructive work of his successors. In addition, Haakonssen seeks a history of ideas that includes a close analysis of the internal logic and implications of ideas from a perspective outside their immediate sociolinguistic context. While he wants to avoid reading anything into a text that is not there, he also wants to tease out ideas the author himself may merely have glimpsed, in order to estimate more accurately what the author said against a full measure of what he did not.

Throughout his analysis Haakonssen underscores just how insistent seventeenth- and eighteenth-century theorists of both rights and duties were that the individual moral life is a social life. There is Hume, for example, whose enquiries were into the social and historical construction of moral ideas and practice, as well as Reid, whose initial concern with individual moral cognition, personality, and agency became an account of individual moral agents embedded in and responsible for acting in ways that conserve the natural social order. (Thomas Hobbes is evermore a special case.) Haakonssen also notes the modest place many assigned to the idea of contract in their discussions of obligation. Again there is Hume, whose explicit critique of the idea of an original contract is well known, and there are Hutcheson and Reid, whose consideration of obligation involved not contract but the moral duties the natural law imposes on individuals. With these points, and with the discriminating attention he pays to the variety in modern theories of natural law, Haakonssen is urging a critical scholarly reconsideration of exactly what principles were at the core of seventeenth- and eighteenth-century thinking about the individual in relation to society and about the nature and exercise of authority and responsibility within and without the public sphere.

This study is a survey, but there is nothing introductory about it. The precision of Haakonssen's vignettes, his ranging synthesis, and the generous bibliography of primary and secondary material make the book a valuable reference for anyone interested in the modern theory of natural law or the Scottish Enlightenment. Still, a reader who is not already quite familiar with the subjects at hand may want further guidance. Over the past decade Haakonssen has published separately the case studies that now, with some revision and adaptation, constitute chapters. Consequently, this is a book of fresh starts and new beginnings. The brief introductory remarks are not adequate to the complexity of chapters that include substantial discussions of philosophical history, political economy, and the epistemology of morals. It is not always immediately clear when Haakonssen shifts out of sociolinguistic context to look down a logical road not taken. Moreover, the technical theological, scientific, and scientistic issues that pervade the jurisprudential demand as informed an understanding as Haakonssen himself brings to morals and the law.

Natural Law and Moral Philosophy is an important canvass of a culture at work. It is a study not simply of the rich linguistic matrix that constituted and supported the culture, but also of the publications, translations, and institutions that forged the moral personality for two centuries in Protestant Europe and North America.

Toleration: An Elusive Virtue. Edited by David Heyd. Princeton, NJ: Princeton University Press, 1996. 242p. $39.50.
Tolerance and Community. By Glenn Tinder. Columbia and London: University of Missouri Press, 1995. 244p. $39.95.

Preston King, *Lancaster University*

It is convenient to distinguish between toleration and tolerance; the former has to do with *all* negations of intolerance (from universal brotherhood to putting up with noisy neighbors), and the latter has only to do with *one* negation of intolerance (the tolerator objects to an item but accepts it)—as in P. King's *Toleration* (1976, I, 12). There are many different meanings that attach both to toleration and to tolerance. To ignore the logical structure of the concept is to wander wilfully into a swamp—and that is a minor accomplishment of both books under review.

Years ago, Glenn Tinder (1976) published *Tolerance: Toward a New Civility*. The present book is a rewriting of that. The raison d'être of Tinder's new book, perhaps especially for being unacknowledged, is largely to defend the 1976 thesis against irrelevance, in light of the fair flood of new work that has since swept over us. The author, signalling his commitment, quotes Simone Weil: "Attentiveness is the rarest and purest form of generosity" (p. xii). Yet, the recursive force of this injunction appears entirely to escape him. Tinder makes no mention of any of the new literature, without which the case for rewriting cannot prove quite so clear or pressing.

The author gives the nod to Buber and to Jaspers, but for the rest he mainly lobs his missiles farther afield, at such antecedents as Locke and Mill. Tinder's position is that the tolerance of these men cannot quite hold up for us since it rests upon the leaky (as Tinder thinks) ideals of individualism, rationalism, and historical optimism. Tinder would defend tolerance, rather, by resting it on what he takes to be the stronger tripod of communalism, "transrationalism," and historical pessimism. What is useful in Tinder is that he sees tolerance quite firmly as a value. What undercuts this usefulness is (1) his unawareness or indifference to the way in which the argument has moved since 1976 and (2) the lack of penetration of the analysis that he does supply.

As regards (1), Tinder dismisses Mill's individualism, for example, not by overturning Mill's more powerful harm principle but by holding, as is conventional, that Mill's distinction between self- and other-regarding principles is not tenable. As regards (2), Tinder defends community, for example, as "that which is realized in the act of communication" (p. 67), without properly allowing that antipathy and disunity may equally be realized in "the act of communication." Tinder has it that "communication clearly depends on tolerance" (p. 67) and that "there is no community ... without tolerance" (p. 84). But this would allow one to infer that, for the master just to be able to address the slave, he must be tolerant. It would force the conclusion that an oppressive community, qua community, must be tolerant. When Tinder affirms that "people cannot speak to one another truthfully unless they have freedom" (p. 67), it is difficult to see how he might elide such an inference ad absurdum, as that "the truth is *never* spoken under torture." Here we confront the slippery conflation of the real (community) with the rational (community).

There are too many faulty moves and blunders: "Presumably, the laws established in many areas of the physical sciences can be characterized as absolute" (p. 111). Confusion, too: Tinder's "principle of uncertainty," for example, acknowledges no distinction between mere ignorance of fact and the procedural uncertainty or "formal doubt" that may be entertained with regard to fact. What makes Tinder's book brave, and certainly salient, nonetheless, is its emphasis upon the *value* of tolerance and especially upon the value of free expression. It is to be welcomed for underscoring the need of a more coherent and sympathetic analysis of tolerance as a value, even where it may fail itself to satisfy that need.

David Heyd's edited collection, the product of a conference, takes us a little farther. That is to say, the analyses are tighter, and contributors are inclined to regard tolerance as a virtue. But Heyd betrays none of Tinder's warmth for, or commitment to, the subject. The subtitle aptly signals the editor's conclusion that tolerance, if a virtue, is *elusive*. Indeed, Heyd's conclusion is that there runs through all the papers a thread of "deep theoretical doubt" concerning the possibility of tolerance acquiring "a stable philosophical ground" (p. 10). The individual analyses, though tighter than Tinder's, remain small-scale and varied, the editor arriving upon the scene after the fact, to tidy up what has randomly fallen out. The most superficial of these pieces is, alas, the editor's. He advises that "we do not tolerate opinions and beliefs, or even actions and practices" (p. 14)—by way of unnecessarily reminding us that there are none of these except as held by persons. Similarly, he advises that even "governments cannot strictly speaking be said to be tolerant," since tolerance is "only shown exclusively *to* people" and, indeed, "*by* people" (p. 15), which leaves us with the puzzling implication that to speak of government is never to speak of *people*.

Analysis of the problem of tolerance as a value in the Heyd volume is carried farther than in Tinder's book essentially in the sense that it is done better. But the individual articles in the collection are diverse and often mutually inconsistent, not being designed to fit a plan. Despite the intelligence in reserve, contributors criss-cross schematically, unsystematically, repetitively. Indeed, there is much reinvention of the wheel. At more than one point, analysis itself fails. Bernard Williams, a congenial mind, slips into the facile, opening claim that a group "can rightly be said to be 'intolerant' if it would like to suppress . . . others even if . . . it has no power to do so" (p. 19). Such a move would confusingly allow us to accept the prisoner in chains, angry with her captor, as the perfect paradigm of "intolerance." Williams concludes, at the close of his brisk nine pages, that "the best hopes for toleration as a practice lie . . . in international commercial society" (p. 26), which would appear to mean that a free market will guarantee free speech. The editor, revving up such a piece as this, confesses to finding it "masterly" (p. 5) and thus overshoots if not all understanding, then mine.

T. M. Scanlon is placed at the end of the book. He might as aptly have opened it. His first two pages respond again to the query: "What is tolerance" (pp. 226–7)? George Fletcher, too, tells us what tolerance is: Its core "lies in the internal conflict of the tolerant." Fletcher proceeds, as a part of this, to dilate upon religion, free speech, and sex (in the space of fourteen pages). In a very much longer essay, Joshua Cohen writes exclusively and intelligently on freedom of expression and perhaps for this reason is assigned the slot next to last.

The faults of the Heyd collection do not lie on the whole with the contributors. If they are asked loosely to write about toleration, then that is what we may expect. The fault, rather, lies in editorial design. If no firm structure is imposed, no appropriate division of labor established, if one builds beside, not on, perfectly reliable foundations laid down decades back, then we may expect intellectual artisans to clamber all over the site, colliding anarchically as they go, to achieve in time a pretty rum and rickety outcome. With Tinder, there is a framework, a program and sympathy, but inadequate skill. Among Heyd's authors, by contrast, there is in general ample skill, moderate sympathy, but no framework or program. Horton is informed and workmanlike. Kymlicka offers a useful discussion of the relative merits of attributing rights, and in this sense tolerance, to groups, as over against individuals. Arguably the most important essay, however, is that by Gordon Graham, who establishes the crucial, logical connection between the value of tolerance (best here to avoid "toleration") and support for objectivism. I suspect that this is the beginning of wisdom with tolerance—the path that leads most securely out of paradox and relativism.

Reason in Action: Essays in the Philosophy of the Social Sciences. By Martin Hollis. Cambridge: Cambridge University Press, 1996. 283p. $59.95 cloth, $17.95 paper.

Harold Kincaid, *University of Alabama at Birmingham*

Martin Hollis has written perceptively and eloquently about the philosophy of social science for many years. This book brings together various of his essays spanning three decades, but it is considerably more than a mere collection. Hollis has argued for the same basic picture of social explanation throughout his career, and these essays have been selected and shaped to make a continuous argument for that position. The essays are wonderfully written and nicely raise many fundamental issues in the social sciences. They are essays in the traditional sense rather than articles, however, so they lack both detailed references to the literature and elaborate argumentation.

Hollis focuses on a tightly interrelated set of topics: the strengths and weaknesses of rational choice theory and game theory; the nature of rationality; the place of norms and social context in social explanation; the (alleged) a priori constraints of rationality on interpretation; and the inadequacy of contract accounts of ethics and political obligation. Among his main claims are that (1) instrumentalist notions of rationality are inherently flawed; (2) substantive accounts of rationality are inevitable, must make use of some notion such as "real interest," and must leave a place for socially defined norms, habits, and expressive action; (3) interpreting the meanings, desires, beliefs, and so forth, of others cannot be done without a priori constraints of rationality; and (4) these previous points help show, among other things, why contractarian notions of ethics are misguided and why social constructivist views in sociology of knowledge cannot be right.

Hollis's most sustained discussion targets rational choice theory and game theory spin-offs. His complaints are serious though well known, no doubt in part because of his own work over the years. Rational choice explanations tend to be either plausible but trivial or interesting but empirically false. When rationality requires only the usual coherence requirements on preferences taken as given, then rational choice explanations are best partial explanations, for by taking preferences and social contexts as given, much that is important is left unexplained. Moreover, this thin notion of rationality frequently cannot even give us determinate results, as Hollis nicely illustrates by use of various paradoxes from game theory. If rational choice goes beyond the purely formal notion of rationality, it can indeed say more, but then we must buy into the egoism and individualism it assumes. Hollis thinks a more helpful notion of rationality for the social sciences will, indeed, be a substantive one, but one that builds in social context, describes real interests, and sees action as expressive rather than merely a means to external ends. Though most of Hollis's criticisms are telling, he is much less successful at describing his preferred alternative. We get little help in seeing exactly how the notions of real interests and expressive rationality are to be spelled out, and Hollis does not defend these ideas against their more obvious difficulties.

Hollis wants to give the social an important place in understanding rationality, but he has little sympathy for social constructivist approaches to science. His complaints have been made elsewhere, but they certainly bear repeating. Scientific beliefs, rational or irrational, may indeed be causally explained, as the Strong Program claims, yet that does not preclude beliefs about the evidence, reasons, and so on, from likewise playing a fundamental role. Moreover, social constructivism borders on being self-refuting: If all views are just the result of negotiation and interest advancement, then so is social constructivist doctrine, and we thus have little reason to believe it. Hollis lays out these kinds of difficulties in charming detail.

Hollis's comments on contractarianism and political obligation more generally echo views perhaps most closely associated with Charles Taylor. Given Hollis's doubts about formal notions of rational choice, he easily raises similar difficulties for the project of deriving morality from game theory considerations. His doubts about instrumental rationality likewise fuel an interesting discussion of notions of citizenship and community.

Hollis is least successful in arguing for his brand of rationalism. He holds that we are inevitably bound to interpret the actions, beliefs, and so forth, of other cultures and of others in our own culture as mostly rational. Moreover, he claims we must find them rational not only in the sense of being reasonable in light of their beliefs but also in an objective sense that involves truth and correctness. His argument for this claim is the familiar one that interpretation cannot proceed without prior assumptions. Thus, he argues that understanding others is fundamentally different from understanding ordinary physical objects or biological organisms—we must assume subjects are rational in a full-bodied objective sense. This argument is suspicious at every turn. First, all natural science depends on prior background knowledge, so such holism cannot by itself show that there is something fundamentally special about interpreting human beings. Second, there is an exhaustive literature, which Hollis ignores, arguing quite convincingly that principles of charity—"make the other person rational"—are empirically warranted if they are warranted at all. We can sometimes have good reason not to interpret others in a way that makes them as rational as possible. Indeed, Hollis's own discussions of rationality should have led him to this conclusion. He points out repeatedly how difficult it is to come up with any purely formal and general rules of rationality—much of his critique of rational choice theory and game theory depends on this point. But if we have no clear substantive notion of rationality, it is quite hard to see how we can even make sense of the claim that we must interpret so as to maximize rationality.

Reading Marx Writing: Melodrama, the Market and the *Grundrisse*. By Thomas M. Kemple. Stanford: Stanford University Press, 1995. 274p. $35.00.

Brian J. Shaw, *Davidson College*

Two decades ago Perry Anderson (*Considerations on Western Marxism*, 1976) argued that the triple catastrophe of Stalinism, Nazism, and Keynesianism had prompted the fatal retreat of Marxism from the revolutionary praxis of Lenin and Trotsky to the academic (and defeatist) "Western Marxism" of Lukacs, Adorno, Sartre, Colletti, and others. Today, the ignominious collapse of "actually existing socialism" in the former Soviet Union and Eastern Bloc, the spectacular hegemony of "democratic capitalism" throughout the Third World, and the thoroughgoing unfashionableness of Marxist discourse in Western academic life have occasioned yet another round of reflections on Marxism's prospects (if any). Derrida's *Specters of Marx* (1994), Aaronson's *After Marxism* (1994), Giddens's *Beyond Left and Right* (1995), and *Whither Marxism?* (1994), a collection of essays edited by Magnus and Cullenberg, number among the more notable.

Less well-known than these authors, Thomas Kemple also wonders what insights Marx might still afford into contemporary capitalist societies so radically unlike the industrial economies and polities of his own day. "Our question is . . . how [Marx] in fact arrived at this futuristic vision of our present, what its general outlines are, and in what ways might we interpret its implications" (pp. 22–3). Unlike Aaronson and some others, however, who bid a sorrowful farewell to Marxism, Kemple responds to queries like these enthusiastically in Marx's favor. While eschewing any labeling of his own thought as Marxist, Kemple nonetheless credits Marx with enabling us profitably "to glimpse our own dilemmas through the political and sociological imagination that informed his understanding of our common history" (p. 25).

Kemple proposes to liberate the implications of Marx's thought for our own situation by undertaking an ambitious and imaginative reading of Marx, especially the eight expansive notebooks Marx penned during the winter of 1857–58, which were edited and effectively published in the original German only in 1953 as the *Grundrisse*. By treating these notebooks as a "kind of 'pre-text' for examining the literary, political, and scientific imagination of Marx" (p. 2), and by reading them together with the work of a wide range of other authors, including fiction writers Marx admired (Goethe, Balzac) and later commentators on Marx (among them Habermas, Lukacs, Jameson, Marcuse, and Baudrillard), Kemple claims insights into topics of concern to contemporary disciplines as diverse as philosophy of science, aesthetics, and political economy. Indeed, far from having lost their relevance, Marx's writings emerge as well-neigh prophetic, illuminating events as remote from one another as Nixon's abolition of the gold standard in 1973 and contemporary appreciations of premodern iconic art. Marx's writings, Kemple admonishes, "provide us with anticipatory illuminations . . . of our own past, present and future" (p. 129).

Needless to say, in light of the tremendous transformations since Marx's death in 1883, more than a few readers may respond skeptically to assertions like these. To persuade them, Kemple seeks to justify these claims not by arguing for the continued "scientific" relevance of Marx's theories to late-twentieth-century capitalism (something he explicitly denies), but by pleading that the remarkable revelatory qualities of Marx's writings emerge from readings of them carefully attuned to their melodramatic, poetic, musical, and even "science fictional" (p. 128) character. Thus, far from taking seriously Marx's "profit law," Kemple urges us to recognize its "fictional origins and even farcical intentions" as "part of a *polemical joke* he is playing on his fellow political economists" (p. 158). Kemple also recommends abandoning Marx's labor theory of value in favor of a "leisure theory of value" or even a "concept of *tele-value*" (p. 163), notions which allegedly work better to explain the widespread docility of workers within contemporary capitalist societies than do any of Marx's own contributions to political economy.

In fact, attempting even to read Marx's writings as science, or worse, attributing to him the intention actually to write science, Kemple dismisses as hopelessly misleading. Instead, to glean from Marx's writings their fullest illumination of the present situation, we must open ourselves to their "rapturous delight" (p. 18), "hilarity" (p. 162), "delirium" (p. 30), and "even madness" (p. 105). Far from seeking systematically to grasp his own or any other age, "Marx's intention is . . . to expose the irreducible phantasmic dimensions of all science" (p. 137). Indeed, Kemple admonishes, this must be the case, since only the "fictional dimensions of Marx's theoretical abstractions" (p. 126) allow him—and us—adequately to capture a reality that is itself socially constructed. The ultimately literary character of Marx's writing captures best the "fictionality of our sense of reality" (p. 114). Marx's legacy thus resides not in any principles or empirical insights but in a unique "theory and practice of reading" (p. 48) by means of which he deciphers both literary and social texts.

In the end, how successful are Kemple's own readings of Marx and other authors, and how persuasive the claims he offers for Marx on their basis? My own judgment on both counts is "not terribly." Most strikingly, while Kemple appears at first blush to have spent a great deal of time in the company of an impressively large number of texts, his readings appear almost willfully (wantonly?) careless. Sustained readings and arguments to support them remain strikingly absent; instead, Kemple hurdles from one provocative and implausible assertion to another, pausing in most instances, if at all, merely to refer the reader to clusters of authors and texts (often without pagination), as if simply gesturing in their direction were enough to establish the plausibility of his claims.

Now, many of us who, like Kemple, enjoy the company of Marx and his commentators also appreciate the richly literary and aesthetic qualities of his writings. Almost half a century ago, after all, Edmund Wilson (*To the Finland Station*, 1972) rightly praised Marx as a fine satirist and even the "greatest ironist since Swift," an opinion confirmed during the last decade by Robert Paul Wolff's fine little book, *Moneybags Must Be So Lucky: On the Literary Structure of Capital* (1988). But to appreciate the literary qualities of Marx's writing as more than mere accouterments to his broader purposes is hardy to see in them the sort of "delirium," "raptures," "nightmares," and "madness" Kemple does. For this to happen, Kemple must offer more than jargon culled from hurried (and often similarly global) references to the writings of poststructuralists like Barthes and Derrida, jargon which might appear, at least to sympathetic readers of these authors, both to authorize Kemple's claims and to immunize him against the need to present arguments of his own.

Of course, Kemple knows that many readers will, in fact, insist upon raising precisely such demands, and these he seeks to deflect by adopting at least two strategies, neither of which works especially well. The first is simply to declare that, in keeping with the spirit of the *Grundrisse*, Kemple will imitate Marx's "unruly methods," including his habit of falling into frequent digressions and parenthetical commentaries and even deliberately "mistranslating his sources according to his own interests" (p. 207). More substantially, and again seeking to evoke Marx's ironic spirit, but actually evincing more the influence of Derrida and Barthes, Kemple simply renounces the very notion of faithful readings in favor of declaring the "writerly" rather than merely "readerly" quality of Marx's texts (p. 66). Marx's own "reading practices," Kemple insists, "require a reader's own inscription into a conflict of interpretations" (p. 60). Accordingly, it is of no consequence that other readers (like myself) might perceive irony and richly figurative language in Marx's writings but fail to glimpse the hilarity, madness, delirium, nightmares, music, or whatever else Kemple thinks he sees in them. Any attempt to challenge such readings both hopelessly betrays one's lack of literary sophistication and even voices an authoritarian demand for "magisterial" readings (p. 54). Not to yield uncritically to Kemple's interpretations, apparently, is illicitly to refuse to "implicate" oneself in a "complex hermeneutical structure of multiple readings" (p. 56).

Kemple's refusal to explore sensitively either the consequences of the hermeneutical strategies he adopts or the kinds of arguments others have offered for them would be worrisome enough (interpretation remains, after all, a notoriously contentious and thorny process!), but the problem goes even deeper. At the same time Kemple renounces any hermeneutical method which might invalidate his own "writerly" readings, or even demand that he actually defend them, he is not at all reluctant to disqualify others' readings. Thus, for instance, Habermas's influential (and certainly debatable) interpretation of Marx is simply (magisterially?) dismissed by Kemple in several pages as "rather simplistic" (p. 28), "limited," and reductive (p. 29); Baudrillard's "restrictive" and "ungenerous" reading is criticized for trivializing the "subtleties and niceties of Marx's analysis" (p. 99); Robert Tucker's analysis is both overly psychologizing and insufficiently "attentive" (p. 54); and Althusser's reading is dispatched as essentially authoritarian. Indeed, far from generously extending to others the interpretative license he grants himself, Kemple rather patronizingly lectures these authors and the other particular (mis)interpreters of Marx he summarily discusses. Kemple also repeatedly declaims against yet other, unnamed readers less "careful" (p. 47) and "versatile" (p. 71) than he.

In the end, Marx's reputation and relevance may or may not survive the traumatic developments that have prompted the latest round of reflections on his meaning for us. But if they do (and I think they will), this will come not from rescue missions like Kemple's, which seek to save Marx by transforming him into a sort of postmodern or Barthean *avant la lettre*, a writer more concerned to "develop a style" (p. 114) and "the theory and practice of reading" than politically to revolutionize an entire social structure. If Marx does re-emerge to speak to us anew, he will do so in the course of engagements with texts both we and Marx himself might more readily recognize as his than with anything Kemple presents us.

Tocqueville and the Nature of Democracy. By Pierre Manent. Trans. John Waggoner. Lanham, MD: Rowman & Littlefield Publishers, Inc., 1996. 148p. $40.00 cloth, $18.95 paper.

Sanford Kessler, *North Carolina State University*

Pierre Manent's book illuminates the core of Tocqueville's political philosophy as it appears in *Democracy in America* and *The Old Regime and the Revolution*. Manent defines his interpretive task, as Tocqueville did, by the nature and needs of his audience. Tocqueville sought to reconcile a dying but still powerful aristocracy to the inevitability of democracy by softening his portrait of democracy's dangers with a generally bright account of Jacksonian America. Manent presents Tocqueville to an audience largely transformed by democracy and too prone to think well of it. He therefore highlights those Tocquevillian themes which illuminate the often hidden democratic forces that adversely affect our personal lives and political destinies. Although not without flaws, *Tocqueville and the Nature of Democracy* is a stunning intellectual achievement worthy of a place in the first circle of Tocqueville studies.

Manent first clarifies Tocqueville's understanding of the nature of democracy. Its core, he shows, is neither the social state of equality, as Tocqueville first suggests, nor the fact of majority rule. It is, rather, the principle of popular sovereignty. This principle ultimately destroyed aristocracy and reshaped all aspects of human life according to its egalitarian requirements. Its most enduring and revolutionary accomplishment was to reconstitute aristocracy's hierarchical relationships on the basis of individual independence and autonomy. Manent artfully uses Tocqueville's often ignored discussion of the master-servant relationship to illustrate how this process works. Democracy did not eliminate this inherently unequal bond but put it on a contract-oriented rather than a status-oriented foundation. Thus, in this as in all such cases, equality lessened both the social distance between the parties involved and their capacity to exert personal influence over each other.

Manent also offers a fine explanation for one of the most salient features of Tocqueville's liberalism: his doubts regarding the future of political freedom. Manent's Tocqueville found some elements which support and others which endanger political freedom in both aristocracy and democracy. Aristocracy honors reason and virtue in principle, strengthens social ties, and nourishes genuine political sentiments. These, in Tocqueville's view, are essential prerequisites of healthy political life. At the same time, however, it oppresses the many and denies them the opportunity to fulfill their natural potential. Democracy offers political opportunities to all but weakens social ties, breeds indifference to politics, and renders the natural excellences suspect and vulnerable. In the end, Tocqueville tips the scales of justice in favor of democracy but warns that its triumph should give liberals pause.

The aim of Tocqueville's political science, according to Manent, was to check those elements of democracy hostile to freedom while strengthening those elements supportive of it. Tocqueville's Americans provisionally accomplished this task by linking freedom to various forms of enlightened self-interest and, above all, by using Christianity to spiritualize democratic life and to reconstitute civic and moral virtue on a democratic basis.

Manent considers religion the "strategic plane *par excellence* in the Tocquevillian doctrine" because Tocqueville, in his view, believed that faith alone could check democratic excess from a vantage point external to democracy: the natural human desire for eternal life (p. 106). Yet, as Manent shows, Tocqueville was fully aware that American Christianity had to compromise extensively with democracy in order to serve the cause of freedom. Tocqueville's Americans, who were skeptics at heart, supported only those forms of Christianity that accommodated to their prevailing ideas and interests and valued religion more for its utility than its truth. Tocqueville approved of these arrangements, Manent believes, because they enabled American religion to remain a potent social force.

Manent clearly admires Tocqueville's subtle and penetrating account of America's modified religion. He criticizes Tocqueville quite sharply, however, for placing too much confidence in this particular type of faith and for failing, as a result, to provide a secure theoretical foundation for freedom. The "central difficulty" of Tocqueville's religious statesmanship as Manent describes it is this: Religion's power over people's souls depends on the quality of their attachments to it. It can only thrive, and hence be useful in addressing the spiritual ills of democracy, if people consider it true. When skeptics are self-consciously religious for selfish reasons, as were Tocqueville's Americans, religion's moral efficacy becomes inextricably linked to selfish assessments of interests and thus compromised. As Manent puts it, "the religion of the Americans loses its utility proportional to their attachment to it for reasons of utility" (p. 91).

Manent's account of Tocqueville's views on religion is a scintillating tour de force to which this brief summary does scant justice. Yet, Manent overemphasizes the political significance Tocqueville attributed to religion and thus somewhat misconstrues the essence of his statesmanship. Tocqueville's categorical assertion that democracies need religion to sustain freedom is well known and undoubtedly sincere. Yet, Tocqueville believed that statesmen and moralists must adopt flexible strategies for preserving freedom as political needs and political resources change over time. If Tocqueville's strategy for preserving freedom relied as heavily on compromised and fragile faith as Manent suggests, he would have been more pessimistic regarding the future of democracy than in fact he was.

Manent's gloomy assessment of the prospects for Tocqueville's political science seems to have influenced his concluding chapter, which is the weakest in the book. In this brief chapter he excoriates two vaguely identified enemies: partisans of aristocracy who seek to restore properly discredited inequalities and extreme democrats whose preferred form of equality would exacerbate democracy's worst faults. The tone of these hurried and strident attacks departs sharply from the measured and philosophic tone that generally marks the analysis.

Manent ends by warning us, as Tocqueville did, that "to love democracy well, it is necessary to love it moderately" (p. 132). But in contrast to Tocqueville, who transformed his moderate love for democracy into a comprehensive plan for improving it, Manent offers us little hope or prudential guidance. Does he believe that the moral dangers threatening freedom today are insurmountable? What strategies would he recommend for coping with these dangers? These questions are left unanswered in this otherwise excellent book.

The Liberal Political Tradition: Contemporary Reappraisals. Edited by James Meadowcroft. Cheltenham, UK: Edward Elgar, 1996. 200p. $79.95.

Margaret Canovan, *Keele University*

Twenty years ago there were wide tracts of British and U.S. academia in which "liberal" was a term of abuse, just as it had long been in the German political and intellectual circles whose lack of a liberal tradition is described in Hans Vorländer's contribution to the book under review. While the banner of antiliberalism is still held aloft by some feminists, elsewhere the past two decades have seen the return to fashion of liberalism in many guises: first, the Kantian philosophical version revived by Rawls; then, the New Right's free-market brand; and latterly, even the more mundane political topics of civil society, liberal democracy, and limited government. Since the collapse of communism and the associated loss of credibility suffered by most forms of socialism, Western intellectuals unable to reconcile themselves to liberal hegemony have been reduced to seeking out such unlikely allies as Carl Schmitt.

How closely this intellectual fashion is interwoven with actual political life is another matter, even where avowedly liberal states are concerned. In the concluding chapter, "A future for liberalism or a liberal future?" Barker argues that liberalism has become "etherealised" and suggests in a vivid image that it is now "a universal Latin dialogue which holds the monastic community of philosophers together as the barbarians swirl about the walls, but one which becomes more and more distant from the political vernacular" (p. 181), and one which cannot become effective unless it finds its "Constantine."

The pessimistic tone of this passage, though curiously at odds with the supposed triumph of liberalism, is quite characteristic of this book, which is the outcome of a conference held in 1993 on the liberal political tradition. In his able introduction, James Meadowcroft stresses that "liberalism must be appreciated as an historical phenomenon. . . complex, multi-stranded, and open-textured" (p. 1), constructed by successive generations of thinkers in response to specific circumstances. A rather different way of stressing the complexity of liberalism informs Freeden's "The family of liberalisms: a morphological analysis." This chapter applies to liberalism a theory of ideologies as "thought-edifices" in which core concepts are "decontested." In Freeden's view, liberalism employs seven core concepts: liberty, individualism, progress, rationality, the general interest, sociability, and limited and responsible power. Persuasive though the piece is, the limitations of this attempt to get a firm conceptual grip on an inherently slippery phenomenon are demonstrated by Freeden's surprising judgment that Friedrich Hayek cannot be counted as a liberal except in a marginal sense.

The contributors to the volume are a distinguished group, although the level of the chapters is inevitably uneven. One of the more substantial pieces is Bellamy's careful critique of Rawls's so-called political liberalism for not being political enough. "A truly political conception of justice sees it as the very process of negotiation rather than as its metapolitical foundation" (97). Bellamy's own version of liberalism, therefore, is oriented to political practice, though in an open-ended and prudential way that is very far from the prescriptive confidence so characteristic of liberalism in the past.

Despite the editor's aspirations, in fact, among the most striking features of the book as a whole are the caution with which the various authors approach political practice and their lack of confidence about the future. It is revealing that the chapter closest in tone to the assurance of earlier liberalisms is Frazer's piece, "Feminism and liberalism," which puts liberalism firmly in its place as something that had its uses for women's liberation in the past but is now passé. Feminism, as she presents it, is a self-confidently "progressive" movement, with an ambitious political and social agenda and a sense of power to determine the future that is quite lacking in liberalism as presented by the other contributors.

It is true that in "Strains in and around liberal theory" Flathman does have a position to defend, that of "wilful or virtuosity liberalism," which he describes as "a radically individualized version of . . . perfectionism" (p. 51). What this amounts to, however, is a turn away from politics to a form of "self-making." Other contributors may come closer to the march of events but view it with considerable misgiving. For example, at the same time as noting the practical success of market liberalism, Barry acknowledges its "moral deficiency" (p. 57) and lack of ideological allure. Despite being given the task of considering "Liberalism after communism," Arblaster can envisage no radical future for liberalism, while in "Liberalism and postmodernism" Andrew Vincent argues that postmodernist nihilism is itself a dangerous outgrowth of liberal individualism which "can provide the groundwork for a return to extremes of nationalistic fervour, fundamentalism and appeals for charismatic leadership" (p. 155).

In combination with the book's stress on the historicity of liberalism, this fearful and pessimistic attitude to the future and unwillingness to offer political principles or prescriptions adds up to a striking example of what John Gray describes in *Post-Liberalism: Studies in Political Thought* (1993). Nevertheless, obituaries for liberalism may be premature; rather, it may be that the spirit of Gladstone sleeps, like Barbarossa, until summoned to fight a battle worthy of his sword. If we are indeed to appreciate liberalism as an historical phenomenon, then we should remember that the context from which it emerged was one of struggle against oppression, particularly militaristic and religious oppression. In one of the more optimistic contributions, Vorländer hails the rediscovery of American liberal universalism in Germany in the 1980s as a way of combatting resurgent nationalism. If the more pessimistic prognoses offered elsewhere in the book are fulfilled, and if there is to be a significant rise of ethnic and religious fanaticism in hitherto liberal societies, then the tradition may lose its present *fin-de-siècle* debility and come back in fighting form.

Reclaiming Democracy: The Sixties in Politics and Memory. By Meta Mendel-Reyes. New York: Routledge, 1995. 205p. $19.95.

Joseph M. Schwartz, *Temple University*

Reclaiming Democracy contends that the hegemonic conservatism of contemporary U.S. political life rests upon a dominant "politics of memory" that conceives the politics of the 1960s to have been an irresponsible, generational revolt which temporarily inculcated into the body politic a culture of "immorality, laziness, dishonesty, and violence." Today's conservative ideological project attempts to purge such disease by rolling back the Great Society in the name of "entrepreneurship and individual responsibility." Using the methods of both political theory and social history, Meta Mendel-Reyes wishes to recapture for our national conversation the promise of participatory democracy of the early 1960s, particularly as manifested in the work of the Student Non-Violent Coordinating Committee (SNCC) and the Educational Research and Action Projection (ERAP) project of Students for a Democratic Society (SDS). Mendel-Reyes holds that, through a participatory culture in which activists collectively deliberated upon the decisions that shaped their life, these movements both remade history and the life of their participants.

To the author's credit, the work is forthright, political, and engaging, abjuring the obscurantism and ahistoricism of too much contemporary political theory. But perhaps because the book is written in a semipopular manner, it avoids interrogating crucial theoretical literature on deliberative democracy. It also grounds its celebration of participatory democracy mostly through the reassertion of participant and secondary accounts rather than through a nuanced, textured investigation of what exactly transpired in all those "endless meetings."

These limits to the work are striking because the author notes all the key problems in the theory and practice of direct democracy, only to pull back from any detailed examination. Among the underanalyzed tensions she briefly notes are the potential for unaccountable leadership inherent in participatory democracy's anti-leadership ideology; the vanguardist threat derived from the gap in political experience between professional organizers and rank-and-file; and the greater ability of "structureless" movements to mobilize rather than to institutionalize political organization and empowerment. Given the next-to-last chapter's impressive insight into how the U.S. myth of the self-made man reinforced puritanical self-righteousness within the New Left, it is regrettable that Mendel-Reyes failed to elaborate upon the self-limitations of the movements of the 1960s.

Theorists highly sympathetic to participatory politics—such as Norberto Bobbio, Claude Lefort, Jane Mansbridge, and Carmen Sirianni—have written extensively about such tensions. Their work holds that only democratic politics can (temporarily) resolve the inevitable tensions between representative and direct democracy and expertise and public accountability that arise when a complex society attempts to increase political participation. In a world more participatory and egalitarian than our own imperfect representative democracy, there would persist scarcity of time, diversity of interests, and differential commitments to politics. The persistence of such real-world tensions eventually led the most thoughtful activists in the early SDS and SNCC to a more sober commitment to democratic and representative organizational structure.

Absent from Mendel-Reyes's chapter on the SNCC and ERAP is a concrete analysis of the substantive matters upon which the participants deliberated. Were they matters of tactics or strategy? How best to achieve a given public policy or what constituted the best policy? That both the SNCC and ERAP organized predominantly to secure existing rights denied to certain individuals and groups (civil rights, welfare rights) rather than radically transform socioeconomic policy may help explain why they could operate in a participatory, consensual fashion. The moment more controversial policy matters began to dominate the African-American and student movements, difference, faction, and conflict emerged. Neither movement succeeded in mediating such differences through democratic organizational practices—and here lies, in part, the origins of the intense factionalism and sectarianism of the "late" New Left and Black Power movements.

Implicitly, Mendel-Reyes sides with Wini Breines's interpretation in *Community and Organization in the New Left* (1989) that whatever the strategic limitations of structureless "movement politics," such "prefigurative politics" of transformative values would have been thwarted by traditional, hierarchial "strategic" or "organizational" politics. But Breines examines the mass feminist and antiwar movements of the late 1960s rather than just the SNCC and the early SDS. And she, like Mendel-Reyes, underestimates the cost to the U.S. polity of the New Left's failure to build lasting multiracial and multigenerational political organizations.

In addition, this work in the history of ideology is peculiarly insensitive to the unique historical moment of the 1960s. This apex of the affluent "American century" facilitated the appeal of a politics demanding the incorporation of the marginal among significant segments of a temporarily prosperous middle and working class. (The SDS's 1962 Port Huron statement speaks of a generation "bred in at least modest comfort.") This sense of altruism facilitated the modest changes in public policy which incorporated people of color as equals in the legal and political system, as well as support for those means-tested social welfare programs (Medicaid, AFDC expansion) which, in limited ways, alleviated mass poverty. But the demise of this mass prosperity, resulting from the global economic restructuring and stagnation that began in the 1970s, accelerated resentment among the white working class toward means-tested programs that benefited the disproportionately nonwhite poor and that did not alleviate working-class economic stress.

It is surprising that an author who spent much of the 1970s and 1980s doing union organizing does not address how the New Left's disproportionate middle-strata, student composition fueled this right-wing populist reaction. The predominant New Left assumption that the white working class was both prosperous and politically coopted contributed to the ability of "lunch pail Republicans" to mobilize major segments of that working class against the Great Society. (Herbert Marcuse [*One Dimensional Man*, 1964] advanced much of the theoretical underpinning for such attitudes, but this icon of the New Left is nowhere mentioned in the book.) This contested "politics of memory" did not begin with the Gingrich revolution but with Richard Nixon's "law and order" appeal to the "silent majority." The insidious racial appeal of the Republican attack on the "paternalistic welfare state" began not in the 1970s, 1980s, or 1990s, but in the 1960s. And the failure of U.S. liberalism in that era to achieve universal health and child care programs of benefit to all working people lay the groundwork for white working-class populist rejection of the left as a whole. One lesson of the 1960s is that the demise of the New Left was directly tied to the very weakness of the New Deal liberalism derided by the New Left.

The tragic alienation between middle-class radicals and the organized working class was the Achilles' heel of the New Left and distinguishes it from the Populist and great Depression mass movements, which Mendel-Reyes claims were comparable. Undoubtedly, the New Left's failure partially arose due to circumstances beyond its control—a largely prowar trade union hierarchy, which overestimated the permanence of the postwar "social contract" and a national failure to move from legal desegregation to socioeconomic integration, thus accentuating racial division. But as the hundreds of former New Leftists trying today to rebuild a democratic labor movement will attest, the New Left's disdain for blue-collar America contributed to the demise of the historic link between middle-class radicals and the labor movement.

Ironically, a book that wishes to celebrate the 1960s radically underestimates the era's profound transformation of U.S. society, if not socioeconomic policy. Only thirty years ago there were few, if any, African Americans working in construction, uniform services, banking and insurance, and white-collar civil service jobs. Only twenty-five years ago, law and medical school incoming classes were 10% women rather than today's nearly 50%. No doubt these gains are threatened, but gains they are nevertheless. And what college student today could imagine that in 1968, the year of the Columbia strike, two Barnard and Columbia seniors were expelled for cohabiting off campus! Of course, the malleable nature of commodified U.S. culture can more easily liberalize sexual and racial attitudes than redistribute racial, class, and gender power. But to paint the post-1960s era as one mass wave of reaction is to misread the complex ways in which the 1960s reconstituted our culture.

Mendel-Reyes continually refers to the participatory impulses evident in contemporary student, workplace, and community politics. Some of these energies were tapped by the conscious decision in the 1970s of many early SDS veterans to engage in bread-and-butter union and community organizing among the very white working class some of them had earlier dismissed. But the absence of an institutionalized, organizational expression of the left makes it unlikely that those mobilized by contemporary grassroots community organizations will encounter a coherent and explicitly leftist analysis of global corporate power and the right's assault on those public goods necessary for equal citizenship. Grassroots participation is integral to the rebuilding of a next left. But so, too, is the building of political and ideological organizations that the New Left's hostility to structure inhibited. The left needs both to deconstruct the right's "political memory" of the 1960s and to engage in a self-reflexive critique of its own 1960s legacy.

Nature, Justice, and Rights in Aristotle's *Politics*. By Fred D. Miller, Jr. New York: Oxford University Press, 1995. 424p. $49.95.

Judith A. Swanson, *Boston University*

While Fred Miller argues the increasingly popular thesis that Aristotle defends a doctrine of rights, he does so far more straightforwardly and painstakingly than any book presently available. Miller, does not go so far as to ascribe to Aristotle the Enlightenment theory of natural rights, but he goes far enough to elicit the complaint that any concept of rights is alien to Aristotle's thought.

To Miller's credit, he acknowledges at the outset the charge of anachronism and initially responds with a frank explanation of his reconstructive method, which "assumes that a concept or a claim may have a place in a philosopher's thought even though it may not be articulated in terms corresponding precisely to ours" (p. 22). Admitting the risk of "misrepresenting or distorting Aristotle by importing ideas or imposing structures that are inimical to his thought" (p. 22), Miller explores and presents Aristotle using current concepts, theories, techniques, and viewpoints.

Miller founds his controversial thesis about rights on a sound exposition of Aristotle's arguments about the relation between nature and politics. The most important task of politicians is "to bring the polis into a natural condition by framing the correct laws and constitution" (p. 79). Human beings cannot realize their natural ends or perfection without natural justice, and lawful or universal justice "is natural justice in so far as it requires actions which accord with virtues" (p. 78). Since exercise of the virtues includes service to the community or common good, just laws define the duties of citizens and their corresponding rights (p. 81).

These rights are not, Miller stresses, inalienable "in the sense advanced . . . in the American Declaration of Independence" (p. 88). The rights law creates are nonetheless natural in the sense of "based on natural justice." That is, Miller distinguishes between such natural_1 rights and natural_2 rights, which are "possessed *in a state of nature*, i.e., in a pre-political state" (p. 88). Thus, "the political rights which a citizen possesses in a just polis may be natural_1 without being natural_2" (p. 88).

Accordingly, Miller distinguishes Aristotle's concept of right not only from the modern nounal sense but also from the exclusively adjectival sense of "rightness." While Leo Strauss and Harry Jaffa argue that "natural right" in Aristotle refers to "the objective rightness of things," Miller contends that objective rightness requires that all human beings possess rights (p. 92). He maintains that rights locutions in Aristotle "have a logical force comparable to that of modern 'rights' locutions and that they can be used for 'natural' as well as 'conventional' rights claims" (p. 89).

In the most important and original chapter, entitled "Rights," Miller argues that Aristotle uses terms that correspond to the four distinct types of rights identified by W. N. Hohfeld, a twentieth-century legal scholar. The Greek term to *dikaion* as used by Aristotle corresponds to the concept of a claim right, *exousia* corresponds to liberty or privilege, *kurios* to authority or power, and *adeia* and *akuros* to immunity.

Aristotle's discussion of corrective justice illustrates the concept of a claim right. Disputants making claims on the same piece of property go to a judge for justice, to get what each says is his own; when a judge settles the dispute by legally ensuring that one of the parties gets "his own," he thus determines that claimant's right (*to dikaion*) to his property. Two criticisms of Miller's argument may be made. First, according to Aristotle, disputants who appeal to a judge seek justice, not enforcement of rights; as Miller's own citation of Aristotle indicates, parties who appeal to a judge assume that "if they get an intermediate amount, they will get justice" (p. 97). If I am robbed, I believe I deserve the return of my property and to see the perpetrator punished, but I may deem justice served if only one or the other can be carried out. Second, Miller not only says that the determinations of a judge or legal enforcement establishes the rights of claimants, their conventional rights, but also implies that they prove such rights—that such rights are (naturally) just. If Miller is arguing that only just judgments establish conventional rights, then he comes close to saying that rightness is the basis of legality and, thus, to a natural rights argument.

By privileges or liberty rights, Miller means freedom of choice. He persuasively presents evidence that the freedom to choose among alternatives (*exousia*), such as virtues and political decisions, is an important ideal for Aristotle (and Plato). Miller's choice to withhold the observation that, when convention or law confers the freedom to choose upon human beings it does so in imitation of nature, allows him to argue that Aristotle does not rank duties above privileges.

Authority rights refer specifically to the authority (*kurios*) granted by law to enforce obedience, or rule. That a man may have de jure authority but lack the power to rule indicates that such authority is entirely conventional.

In addition to conferring authority, law may confer immunity (*adeia*, *akuros*), such as safe conduct, amnesty, indemnity, and noninterference with trade, or in other words deny a right, such as to harm, punish, sue, or regulate.

Miller adds to this analysis "rights based on nature" and "political rights based on nature." Illustrative of the former is the (rightful) enslavement of a man naturally fit to be a slave and, of the latter, (rightful) rule by turns among equals. While Aristotle clearly argues that nature sanctions the rule of superior beings or elements over inferior ones and the justness of treating equals equally, the mechanism that transforms what is naturally right into (enforceable) rights is human law; nature continually exerts its force, but so do men, who may stipulate laws that treat the superior and inferior equally, thereby enforcing rights that are wrong by nature. In other words, the willfulness or choice-making capacity of men prevents natural right from becoming natural law.

Miller's argument suggests that the right nature gives us is the right to be wrong, to reject natural right. He does not so much counter as concede this charge, explaining that the autonomy of liberty that rights theories presuppose can be, as Joseph Raz argues, "interest-based": The right to autonomy holds only if it is in the interest of rights-holders "not to be subjected to the kind of oppressive paternalism which consists in running their lives for them allegedly in their own best interest" (pp. 116–7). Everything that Aristotle says indeed indicates that autonomy or freedom is good if it serves the interest or virtue of the free; but since freedom does not always do so, since not all individuals or populaces use freedom to deliberate prudently or make considered choices, autonomy is not a right but a privilege conferred by fortune and education. Miller himself states: "In assessing Aristotle's inegalitarianism, it is necessary to emphasize that the theory of natural justice as such neither entails nor excludes the doctrine that individuals have equal rights according to nature. From the standpoint of natural justice, individuals possess equal rights according to nature if, and only if, they are in fact equal according to nature" (p. 241). If Aristotelian natural rights are contingent on individual merit and not made mandatory by natural law, then there is no natural basis on which they can be guaranteed, which guarantee Miller argues is the standard of the best constitution.

Although this book may not establish eye-to-eye agreement about Aristotle, it is assiduously analytic throughout.

Eric Voegelin and the Good Society. By John J. Ranieri. Columbia: University of Missouri Press, 1995. 275p. $39.95.

Steven R. McCarl, *University of Denver*

This extraordinary work is the best single volume in existence on Voegelin as a political philosopher. It stands out because of the way Ranieri clarifies the connections in Voegelin's writings between order at the social level and order at the ontological level. (Two recent works, Glenn Hughes's *Mystery and Myth in the Philosophy of Eric Voegelin*, 1993, and Michael Morrissey's *Consciousness and Transcendence*: *The Theology of Eric Voegelin*, 1994, do not focus on the political and social.) The connections are made with great care for the nuances of Voegelin's thinking while taking into account virtually everything Voegelin wrote.

Ranieri proceeds by taking up these topics in the following order: the structure and movement of the broad contours of reality and the consciousness by which that reality is apprehended; how knowledge of reality has been differentiated in various cultures over the course of history; the nature of social reality (in relationship to the broader reality and consciousness); failure at the social level to respond to reality consistently with the whole of reality, that is, with the luminosity through which the higher order manifests itself; the struggle for order in Israel and Athens (including Voegelin's analysis of Plato and the polis); the extent to which the good society is realizable; and Voegelin's attempted "resolution" of the tension between the truth of order and its attempted social incarnation.

The essential nature of the experience of differentiation of consciousness is that while it occurs in the psyches of specific individuals, it is a good for all humans. Human beings realize themselves and reality through the experience of differentiation of consciousness. Thus, the incarnation of the truth of divine order at the social level (to the extent possible) serves all humanity. For this incarnation to occur, "the members of society (or at least their representatives) must not lose contact with the experience of participation in the ground in which the truth of order is encountered.... A great deal of Voegelin's work was precisely an attempt to recover these experiences by moving through symbols to the experiences underlying them" (p. 96).

The case studies of Israel and Athens show that social orders are in varying degrees in attunement with the higher order of reality, but there is always a gap between them and the higher order. The struggle for social order, however, cannot be dismissed as misguided or meaningless, because "the human effort to embody order in society through the ages is the very process that gives rise to the vision in which it is understood that human destiny lies somehow beyond the struggle and that history can only be meaningful when understood from this perspective" (p. 243).

Vision "is the technical term coined by Plato to describe 'the experiential process in which the order of reality is seen, becomes reflexively known, and finds its appropriate language symbols' " (p. 238). Vision serves further to illuminate the structure and movement of reality and is aware that it is itself an expression of divine presence. Ultimately, history is seen as the process of granting vision rather than a chronicle of changes in social institutions (p. 246).

Ranieri discusses the political realism in Voegelin's thought, quoting him: "While . . . might does not make right, it is unfortunately equally true that it makes an order, and that without it an order can be neither created nor maintained" (*Collected Works*, vol. 27, 86). The issue of the content of an order must be distinguished from the problem of establishing and maintaining that order. Moreover, Ranieri points out, Voegelin finds a certain inevitability to social process, "an insight that Voegelin attributes to Thucydides" (p. 253).

The author is critical of Voegelin in helpful ways. For example, he suggests speaking of reality as "intelligible" and as "intelligent," rather than external and luminous. Differentiations in the levels of being (i.e., social reality) become unified not through intelligibility in the data but, rather, through "understanding as somehow analogous to looking or picture-thinking, in which unity is attributable to a 'something' or an 'identical substance' " (p. 133). What Voegelin meant by external becomes the "intelligible," and the luminous becomes the "intelligent" (Ranieri thanks Dr. Patrick Byrne for suggesting this distinction). Ranieri's suggestions avoid the dualistic tendencies in Voegelin's language.

With regard to the literary exchange between Arendt and Voegelin over *Origins of Totalitarianism*, Ranieri finds a certain validity in her criticism that Voegelin treated the phenomenal level of social and political reality as secondary. Yet, Ranieri's fundamental sympathy with Voegelin is revealed in his comment that "if he was correct in his understanding of humanity as constituted by its relationship to the divine ground, then does not liberalism's agnosticism on such questions of ultimate ends reveal a view of humanity as impoverished as that of totalitarianism, which, in the long term, will exact an equally terrible price" (p. 193)? Perhaps. I cannot agree with Ranieri here. The fundamental respect for the individual within liberalism is priceless, and moreover, some of its historical roots are fully compatible with Voegelin's ontology. Obviously, this is a topic beyond our present scope.

Ranieri sheds light on many aspects of Voegelin's writings—beyond, but connected to, the relationship between social order and ontological order. The distinction between noetic differentiation (the process by which reality and human participation as consciousness become articulate to themselves) and pneumatic differentiation (the movement of the divine Beyond toward the human) is discussed in detail. On the basis of that discussion, Ranieri clarifies Voegelin's comparison of Paul's noetic vision with the noetic quest of Plato and Aristotle (pp. 58–76). Ranieri explains how resistance to reality "often has its origins in the quite valid resistance to disorder that accompanies the differentiation of consciousness" (p. 106). The eclipse of reality; the evolution of Plato's thought from the *Gorgias* through the *Phaedrus* and on to the *Republic*, *Timaeus*, and *Laws*; the Ecumenic Age; and many other such topics are analyzed with special clarity.

Seldom do we encounter a work of scholarship that so deeply, clearly, and comprehensively provides insight into a major philosopher. Ranieri's work is cause for celebrating the love of understanding.

Political Theory, Modernity, and Postmodernity. By N. J. Rengger. Oxford: Blackwell, 1995. 253p. $54.95 cloth, $18.95 paper.

Mark Reinhardt, *Williams College*

Given the spate of recent works on questions of modernity and postmodernity, the theory market that this book enters may well be nearing its saturation point. N. J. Rengger aims to turn that potential liability into an asset by explaining the reasons and providing a context for the recent explosion of work. Assaying a comprehensive overview and evaluation of what he calls "the modernity debate," Rengger introduces key players, maps the debate's main contours, and shows its significance for political life. The significance, he contends, is profound: Understanding the making and meaning of modernity enables a reinterpretation of the tasks of political theory and the possibilities of contemporary politics. The three main tasks are to tell us where we are and how we got here, to show where we should go, and to help point the way from here to there. In this hardly startling but useful characterization, Rengger largely follows John Dunn, but he thinks that neither Dunn's brand of intellectual history nor the mainstream of Anglo-American liberal and communitarian philosophy are up to the task. Above all, they do not adequately grasp the current political condition or its genealogy. A thorough-going encounter with the modernity debate, in contrast, will enable Rengger to sketch "an account of political judgment and practical reasoning that can serve as both a way of interpreting our circumstances and a method of evaluating and guiding our actions, and . . . an account of the institutional and practical setting which can permit such a view of political judgment the most room to manoeuver" (p. xiii). His attempt at this sketch is at once edifying and vexing, for he offers probing readings of a wide range of important texts while remaining vague and elusive in elaborating his major arguments.

To begin with the virtues, the book makes a useful contribution to the modernity debate by distinguishing between "modernity as mood" and "modernity as socio-cultural form," showing that much recent writing has suffered from eliding this distinction. Similarly, Rengger's classification of postmodern thinkers as "Epicurean" (e.g., Rorty), "hard skeptical" (e.g., Baudrillard) and "soft skeptical" (e.g., Mouffe), though certainly open to challenge, produces a discussion that is sensitive to important political differences within this putative camp. Such sensitivity cannot be taken for granted amid the current theory wars, but Rengger is an interpreter of admirable care and generosity. He works hard to unsettle common dualisms and to move through familiar impasses. He draws sustenance from writers ancient, modern, and postmodern. Rather than trying to score points, he seeks in each thinker the thought worthy of engagement. Bauman, Blumenberg, Connolly, Habermas, Lyotard, MacIntyre, Strauss, Taylor, and Toulmin—this list, though selective, gives a sense of the diversity of contemporary perspectives that the book engages respectfully and with illuminating results. The discussion of Strauss is particularly subtle and stimulating, and it is the linchpin of one of Rengger's main arguments, an attempt to supplement modern senses of the plasticity of politics and character with a recuperation of the "Platonic-Aristotelian" concern with the problem of nature and convention. This attempt frames an engaging discussion of the question of habit, a discussion that raises one of the book's most interesting—and political theory's oldest—questions: "What social and political conditions are most favorable for the development of virtuous habits and the suppression of vicious ones" (p. 149)?

Unfortunately, Rengger does not offer much of an answer, resting content with showing how recent thinkers have not taken the question seriously enough or have framed it too simply. The same holds for the closely related problem of judgment: The crux of the book's argument is that the key task of contemporary theorists is to formulate a theory of judgment, but Rengger does not develop a substantive account, as he concedes in the final pages. Certainly, he does not meet his goal of helping us understand the context of our actions and how to make our actions better. Of course, that is an unusually lofty aspiration, and he might settle for putting future discussion on a sounder footing. But the ground offered by the book is shakier than the author admits, for he makes a fundamental error in assuming that a theory of what judgment is will (somehow) make us better judges, that a philosophical account of practical wisdom can make us wise. It is possible for a political theory to illuminate the conditions under which the arts of judgment flourish and flounder, and it may be that *writing* by democratic political intellectuals can, in modest ways, help citizens become more discriminating in their judgments. But this book does not tell us much about how that can happen. Near the end, Rengger pauses briefly to take up a promising example, *Eichmann in Jerusalem*, but he is silent on what I would take to be the key to Arendt's effort to prompt thoughtfulness—her practice of storytelling. In Rengger's hands, her book is essentially reduced to a thesis, and a banal one at that: We learn that evil regimes promote . . . evil.

Nor is it ever made sufficiently clear how this crucial problem of judgment is bound up with questions of modernity. The main culprit is Rengger's account of the modernity debate itself. After claiming that the relationship between modernity as mood and modernity as form is crucial, he goes on to ignore the latter for most of the book. And though modernity as mood is at the center of most of his discussion, his own conclusions on the topic remain surprisingly fuzzy. His core arguments need both a crisper and a considerably fuller elaboration. For all the virtues of his specific readings, the cumulative effect of the parade of authors summarized is to deprive the book of much of the focus and force it needs. What is most lacking, in the end, is what Rengger most wants to provide—a sense of how coming to terms with modernity can inform our politics. It might have helped if Rengger had engaged recent debates over the relationship between modernity and such problems as colonialism and slavery, an omission which, along with the near total evasion of feminist work, weakens the book.

Despite these shortcomings, *Political Theory, Modernity, and Postmodernity* offers many valuable interpretations, providing a thoughtful introduction to a wide and complex body of work. It is unlikely to corner the market, but it has probably found a useful niche.

Nietzsche, Heidegger, and the Transition to Postmodernity. By Gregory Bruce Smith. Chicago: University of Chicago Press, 1996. 366p. $42.00.

Ronald Beiner, *University of Toronto*

Do postmodernists take seriously enough the "post" in postmodernity? This is a reasonable question, and it is posed quite acutely in this ambitious study by Gregory Bruce Smith. What would count as a genuinely *post*modern culture? A culture must offer a distinctive ethos, an integral basis for a distinctive mode of dwelling, for the possibility of human rootedness in the world. Both Nietzsche and Heidegger saw modernity as such as undermining residual experiences of ethos, dwelling, and rootedness, and they desperately longed for something radically new that would offer an exit from the modern impasse. What reason is there to think that contemporary art, religion, or intellectual life locates us in a cultural space outside the boundaries of modern experience? As Smith rightly observes, so-called postmodernity stands in a basically parasitical relation to previous culture (p. 53 n.). Rather than help to create culturally novel possibilities beyond the modern horizon, contemporary postmodernism offers what one might think of, following Rorty, as a "culture of irony." Seen from Nietzsche's point of view, this is not a real culture at all but the sign of an incapacity for fashioning a genuine culture (pp. 88 n. 13, 126, 164). Unless one can draw from radical thinkers like Nietzsche and Heidegger at least intimations of a true (and not merely ironic) culture capable of being created in some epoch to come, human beings face the despairing prospect of being trapped forever in a Kojèvian "end of history," where all cultural possibilities have been played out and the only diversion available is to "deconstruct" the fragments already deposited by the history of human culture (pp. 60 n., 72, 88, 114 n., 151, 163, 165).

This is a powerful thesis, but unfortunately the project has trouble sustaining its initial promise. The book is divided into four sections. Part 1 lays out a somewhat plodding survey of modern thought from Machiavelli to Hegel, although this part of the book is to some extent redeemed by a quite incisive account of Hegel (seen through a Kojèvian lens). Part 2 offers an interesting but idiosyncratic reading of Nietzsche. Part 3 is devoted to Heidegger, with a long, jargony exposition of *Being and Time* that thoroughly fails to deliver on its pledge to "pin Heidegger down to something concrete" (p. 198 n. 1). This is followed, however, by several very good chapters, certainly the best in the book, on Heidegger's middle to late works. Part 4 tries to flesh out, not very successfully, how the Nietzschean and Heideggerian themes of the middle sections of the book can furnish guidance about generating new forms of *praxis* that would allow us "to transcend the closure of modernity, to construct a *transition* to something novel" (p. 62).

Smith combines an Arendtian lament for the eclipse of "the political" and a Gadamerian celebration of the autonomy of human practice with an appreciation for the radicalism of Nietzsche and Heidegger's challenges to modernity. This is an attractive package of theoretical concerns, and Smith's lack of enthusiasm for contemporary pseudo-Nietzschean fashions makes it even more attractive. What Smith basically means by modernity is nicely encapsulated in Nietzsche's statement, in the *Genealogy of Morals*, that "our entire modern way of life . . . has the appearance of sheer *hubris* and godlessness. . . . Our whole attitude toward nature, the way we violate her with the aid of machines and the heedless inventiveness of our technicians and engineers, is *hubris*." Smith is right to insist that modernity so described is not something we have left behind, it is *where we are now*; and it is indeed the case that Nietzsche and Heidegger each deploy a kind of rhetoric that helps to articulate the longing for a relation to the world that involves less *techne* and more depth, less instrumental rationality and more mystery. But, again, the book is more successful at cataloguing the ills of modern theory and practice than at making clear how this antimodern rhetoric can get us any closer to the longed-for Nietzschean-Heideggerian Shangri-La of postmodernity.

Those who arrive at chapter 24 still hoping that the vague Heideggerian formulas offered throughout the book will finally be given some more concretely political content will find that they have waited in vain. The conclusion is a disappointing hodgepodge of hedged bets and unhelpful generalities. All one can say for sure is that the author is more sympathetic to liberal democracy than were either Heidegger or Nietzsche. Yet, it is no accident that one is left with this unsatisfying outcome. Smith ends the book with the suggestion that the United States "might provide the site where something genuinely postmodern first comes forth" (p. 343). What is this "something" likely to be? To answer this question is to fall back into the characteristically modern temptation to have theory dictate to *praxis* (p. 324). But not to answer it leaves one wondering what point there is to the whole enterprise of sketching "a speculative alternative to late modernity" (p. 342). This is the basic conundrum of the whole book.

One cannot help applauding the scope and ambition of Smith's enterprise, but one does wish he had found a more effective literary means of pursuing his project. The book is far longer than it needs to be to make its point, and it relies too much on the mantra-like repetition of certain favored slogans ("overcoming the Spirit of Revenge," "affirmation of self-presenting difference," "recovering the autonomy of *praxis*"). Indeed, it is surprising that Smith's editors did not prod him to prune the thing down to more manageable proportions. The book has its rewards, but it requires readers with a lot of patience.

The Limits of Lockean Rights in Property. By Gopal Sreenivasan. New York: Oxford University Press, 1995. 162p. $29.95.

John W. Danford, *Loyola University Chicago*

John Locke's account of the origins and foundation of private property has been the focus of a surprising amount of attention over the past two decades and more. This is partly no doubt because "Of Property," the celebrated fifth chapter of the *Second Treatise*, is as confusing as it is important to Locke's account of the basis of civil society. But the spate of recent attention is probably explained by the fact that so many academic writers have grave doubts about the legitimacy of the institution of private property itself.

Gopal Sreenivasan has given us a succinct and lucid account of some of the central issues. The book offers "a somewhat surprising pair of conclusions: Locke's argument has contemporary relevance and a defensible regime of private property rights is egalitarian" (pp. 3–4). The combination is surprising, of course, because those who believe the latter are unlikely to accept the former. This reviewer found himself surprised by a slightly different pair of the book's features: Sreenivasan treats Locke's text with scrupulous care and almost Straussian seriousness, yet he seems to think Locke did not understand the implications of his own account.

Sreenivasan divides the book into two main parts, each consisting of two chapters. The first two substantive chapters present a detailed analysis of Locke's account of property. One chapter is devoted to what the author calls the "consent problem" (i.e., how—if everything initially belongs to humankind in common—anyone comes to have a private property right in anything). The next considers Locke's explanation of the appropriation of things by means of labor or, more specifically, "*why* it is that labour legitimates appropriation" (p. 59). Here, Sreenivasan is justifiably critical of the traditional interpretation and makes use of Nozick's objection to the labor-mixing doctrine of appropriation, presenting instead a persuasive account of what is termed the "doctrine of maker's right" (p. 62). Each chapter includes cogent treatments of various other commentators (mainly Waldron and Tully in chapter 2; Tully and Nozick in chapter 3). These two chapters are preceded by an introduction that serves to dispose of definitional issues and to direct the reader's attention to the very important issue of the context in which Locke wrote: To precisely what audience were Locke's remarks directed (and, one might add, what was his rhetorical aim)? The author makes use of Ashcraft's insights here, and one wishes they had been more consistently in the author's mind for the remainder of the book.

Part II (chapters 4 and 5; 6 is a conclusion) takes the careful analysis of Part I and seeks to draw out its more general relevance, especially to contemporary debates about the legitimacy of private property. Chapter 4 ("Limitations of the Original Theory") investigates "how far Locke's argument succeeds on its own terms" (p. 95), while the fifth and last substantive chapter asks how far it succeeds on contemporary terms. The latter is primarily an extended argument with Nozick and his essentially liberal property regime. Sreenivasan seeks to supplant the latter by arguing that the Lockean view traced earlier justifies an egalitarian and nonliberal regime of property (one in which, for example, exchange would be permissible, but not gifts, bequests, and inheritance). Such a regime also would require periodic redistributions of property (to the level of the "greatest universalisable share" [p. 117]), since "the legitimacy of Lockean property requires that everyone's access to the means of production be maintained in a manner that conserves her liberty to produce a surplus" (p. 7).

The Limits of Lockean Rights in Property has a number of virtues. The book is clear and focused; the argument is compact; relevant secondary literature is respectfully treated. One loose end is rather troubling, however. The introductory chapter raises the issue of context, and specifically Ashcraft's claim that Locke sought to appeal to a potential Whig audience of "merchants, tradesmen, artisans, shopkeepers, *and* yeomen farmers and the gentry" (p. 15, quoting Ashcraft). The gentry, in particular, were concerned about the stability of property rights, and thus an argument was needed to justify private property and at the same time undermine the "idle, unproductive" landed aristocracy. But this claim is difficult to square with Sreenivasan's assertion that Locke's account justifies an egalitarian property regime (with periodic redistributions). Perhaps more attention should be directed to Locke's rhetoric. And for one who claims to be critical of the traditional reading of Locke, Sreenivasan presents a remarkably conventional reading.

By staying with the received interpretation, Sreenivasan fails to address the most confusing aspect of Locke's "Of Property." It is not difficult to show that two opposed conceptions of the natural state of things lurk in Locke's chapter. On the one hand, Locke sometimes writes as if the natural condition of mankind is one of plenty—he even quotes the biblical formulation that "God has given us all things richly" (II, 31). On the conventional reading, followed by Sreenivasan, there are two periods in Locke's state of nature, the age of abundance and the age of scarcity (see, e.g., p. 35). The latter results only after the introduction of money ("a little piece of yellow metal") makes it possible for individuals to appropriate "larger possessions."

On the other hand, there is another, conflicting account running through the same paragraphs. As Sreenivasan notes, Locke attributes wealth (first 9/10 of value, then he boosts it to 99/100) to the input of human labor (II, 40). The author fails to note that Locke goes even farther, suggesting that land without human labor would be "not one thousandth" the value of land under cultivation (II, 43). These proportions (and the way Locke builds up to the final figure) suggest that the spoilage limitation should be seen in a different light. When there was nothing both lasting and scarce (money), it was not merely "dishonest" for an individual "to hoard up more than he could make use of"; to do so "was a foolish thing," as Locke notes (II, 46). In other words, there is no reason to be industrious. When we combine this insight with Locke's observation that what we have plenty of, by nature, is land, and that land, without cultivation, is *waste*, we come to see the invention of money from the proper perspective: Money makes *possible* human industriousness and gives human beings the *opportunity* to create wealth.

The natural condition is not a condition of plenty, disrupted by the invention of money, as the traditional view taught; rather, mankind has by nature (or from nature) a plentiful supply only of worthless land (waste), and the spoilage limitation effectively prevents men from laboring to improve their situation. In this great insight Locke anticipated the claims of, for example, Adam Smith. I believe Locke did grasp the implications of his own account, but his radical view necessarily ran up against the teaching of the tradition, and especially of the Christian church, with its fundamental misunderstanding of both the source of wealth and the role of money. Locke's rhetorical strategy—his confusing presentation, or what might be called the "double teaching" of chapter 5—can be seen as his response to the context in which he wrote, and in a deeper sense than Ashcraft's purely political or tactical account would suggest. In terms of contemporary relevance, it is worth noting that the same profound misunderstanding confronted by Locke is evident in today's debates about whether wealth should be viewed as a pie to be divided more equitably or as something generated when human energies are liberated (by allowing people to keep more of what they produce).

There is much of value in Sreenivasan's study, but its virtues are to some degree vitiated by the narrow academic concerns within which she is content to remain. I kept thinking, as I read the book, of a passage from Gibbon's *Decline and Fall of the Roman Empire*. Gibbon drily observes that "it might perhaps be more conducive to the virtue, as well as happiness, of mankind, if all possessed the necessaries, and none the superfluities, of life. But in the present imperfect condition of society, luxury, though it may proceed from vice or folly, seems to be the only means that can correct the unequal distribution of property" (chapter 2; Womersley edition, vol. I, p. 80).

American Politics

The UAW and the Heyday of American Liberalism 1945–1968. By Kevin Boyle. Ithaca, NY: Cornell University Press, 1995. 338p. $35.00.

North American Auto Unions in Crisis: Lean Production as Contested Terrain. Edited by William C. Green and Ernest J. Yanarella. Albany, NY: State University of New York Press, 1996. 246p. $59.50 cloth, $19.95 paper.

Stephen Amberg, *The University of Texas at San Antonio*

The transformation of American governance has been stalemated in one area central to capitalist economies, namely, employment relations. A possible reason is that the dominant labor governance mechanism dating from the New Deal is an industrial relations system that is permissive of private ordering among contending interests, and the large-scale management-initiated changes of recent years have undermined the bargaining position of organized labor. Thus, because employers are getting what they want and unions have collapsed as political actors, the policy status quo serves Republicans and Democrats alike rather well. While labor is as central as ever, the argument is widespread that unions have outlived their usefulness. They were too self-centered in the past to contribute to solving the country's problems, and they are too adversarial now that the economy demands teamwork.

But that interpretation may be too hasty. Kevin Boyle's history of the relationship between the United Auto Workers (UAW) and the Democrats from 1945 to 1968 shows in rich detail that not all of organized labor subscribed to the postwar politics of interest group liberalism. What brought down union-backed liberalism was to a large degree beyond union influence rather than a consequence of inherent union failings. The edited volume by William Green and Ernest Yanarella, which examines contemporary industrial relations in the automobile industry, argues that the imbalance of power in restructured workplaces is a critical determinant of national economic performance.

The automobile industry has long been considered a pivotal case for understanding twentieth-century political developments. Recent studies have explicitly connected changes in auto industrial organization to questions about governance, such as Campbell, Hollingsworth, and Lindberg's *Governance of the American Economy* (1991), Turner's *Democracy at Work* (1991), Kenney and Florida's *Beyond Mass Production* (1993), and Amberg's *Union Inspiration in American Politics* (1994). Thus, Boyle's book adds to a substantial scholarship about auto labor and politics but also makes its own distinctive contribution in two ways. First, this is one of the first scholarly histories of labor politics in the 1960s; second, Boyle argues it was in that decade the UAW leaders—especially president Walter Reuther—departed from their historical social democratic agenda in order to gain influence over the antipoverty program. "That proved to be a grievous miscalculation" (p. 7), in Boyle's assessment. As the author admits, his analysis is in many respects a traditional political history focused on the leaders' policy priorities; there is little in the book about workplace conflicts and the institutional mechanisms of consent in industrial relations. But the book presents, with a remarkably assured tone and a mastery of materials, a persuasive narrative of the shortcomings of postwar liberalism from the labor perspective that was so important then and is so often ignored today.

The first two chapters lay out the "vanguard" role that Reuther and his leadership caucus at the UAW adopted in the postwar 1940s. In contrast to some New Left analysts, Boyle argues that the UAW's social democratic commitments survived the onset of Cold War and the Taft-Hartley Act (1947). And in contrast to pluralist interpretations of U.S. unions, the Reuther leadership acted on the belief that the CIO could lead the United States to social democracy. Fighting for what once was called the "middle way," the Reutherites battled against the authoritarian communist left in the union movement and the reactionary right represented by autocratic corporate managements and southern white supremacists. Reuther's vision of industrial democracy went beyond workplace contractualism and interest group liberalism (p. 25).

Rooted in Socialist and Progressive traditions still vibrant in the 1930s, Reuther advocated corporatistic institutions at the company, industry, and national levels to plan industrial modernization and the distribution of productive "abundance" to the general population. Although his plan was not as unusual then as it would be today and had the support of those identified by Boyle as "liberal statists" (p. 37), labor's social democrats had to contend with social Keynesians (p. 37) and commercial Keynesians (p. 45) as well as conservative opponents. By the end of the 1940s, when reformism was blunted by a vicious red scare and when commercial Keynesianism had become the practical policy of the federal government, the leaders of the UAW settled on a strategy to realign the Democratic Party into a social reform party. They became "a vital part of the Democratic electoral coalition and the party's political machinery and there was always the hope that the UAW leadership could use that position to inject its program into public debate" (p. 60).

The following eight chapters detail the UAW's persistent efforts in the two decades after the 1948 presidential election to gain Democratic endorsement of the social democratic agenda. Boyle describes how UAW leaders were frustrated by the institutional boundaries of postwar consensus. One issue, however, stood out for its potential to break the boundaries, namely, race, and the UAW used it (p. 108). The UAW made some progress through the New Deal industrial relations system by bargaining for rules of employment security and income for all auto workers, but general equal employment opportunity and universal welfare entitlements were still on the farther shore of federal intervention to secure racial equality. Moreover, as long as southern white supremacists retained a veto in Democratic Party councils and senior positions on congressional committees, labor's agenda seemed doomed. To bust open that cleavage, the UAW and its allies pushed national legislation for racial equality.

In the 1960s, as the civil rights movement became influential, UAW leaders asserted their leadership in civil rights but did so, according to Boyle, only at the cost of abandoning social democracy. In the social democratic perspective, racial discrimination was tied to economic injustice. The solution was to compel corporate leaders to provide more opportunity for all, a condition which could be unleashed by the existing technology of production if it could be managed for the public interest, rather than by a zero-sum competition among the races. The interest group approach, however, was more politically congenial to President Lyndon Johnson than was dealing with the economic democracy problem. His administration provided specialized benefits for poor blacks, fully aware that this policy framework jeopardized the Democratic Party's mass base. What was "extraordinary," Boyle comments, was that UAW leaders embraced it and abandoned their class perspective (p. 191). Boyle's reconstruction of the fragmentation of liberal perspectives is a first-class contribution to our understanding of a crucial episode in U.S. political development.

Boyle's book could be usefully read with the Green and Yanarella volume, which provides a highly critical account of the sequel, that is, what happened after the successful entry of Japanese auto companies into the U.S. market in the mid-1970s. The UAW and other manufacturing unions were left to use the industrial relations system the best they could to cope with the greatest changes in industrial conditions since the Great Depression, and the best was not very good. The UAW lost almost half its peak membership.

In the New Deal industrial relations regulation, unions can try to win agreements about industrial change, but there is no legal requirement that managers bargain over plant closings, investment policies, technology, and a host of other decisions that vitally affect employment and income. Of course, these decisions were made by U.S. managers before the Japanese appeared, but what Green and Yanarella call Japanese Production Management (JPM) forced U.S. firms to accelerate the pace and change the quality of work relations. For many observers of Japanese auto companies in management and academia, JPM is the core of a better, cooperative style of governance than that associated with New Deal pluralism, antitrust, and adversarial industrial relations. But such positive assessments receive critical scorn from most of the authors in Green and Yanarella.

Eight essays compare how U.S. and Canadian auto workers are being affected by JPM in Japanese transplants, Japanese-American joint ventures, and in GM's Saturn. Informing them is a theoretical framework grounded in Antonio Gramsci's "Americanism and Fordism," which foresees in Henry Ford's moving assembly line a new paradigm of work relations and consent to capitalist hegemony. The New Deal brought Fordist consent to fruition by marrying a physically demanding work regimen to contractual due process and consumer claims on the productive surplus. In contrast, the new consent anticipated by JPM is based on enterprise unions that collaborate with production managers, pay linked to firm performance, and labor market competition that divides the working class. In short, JPM subordinates workers to the capitalist labor process in ways that are less innovative than reminiscent of the pre–New Deal era (p. 35). Several of the authors call JPM neo-Fordism rather than post-Fordism.

The essays are uneven in quality. Carl Dassbach subjects leading positive assessments of JPM to sustained critique: Self-managing teamwork doubles as a technique "to create smaller units which can be more closely supervised and more completely exploited" (p. 22), cross-training is really "multitask" training rather than the development of skills (p. 25), the constant improvement system leads to "super-exploitation"—both physical and intellectual (pp. 26–9, 36), and the commitment of firms to employees merely matches those in UAW-organized plants in the United States and Canada (pp. 30–2). Ernest Yanarella describes the break-away of the Canadian region of the UAW in 1985 as a resolution of the growing gap between the UAW's accommodating posture toward JPM and the Canadians' opposition. He interprets the UAW's collaboration with JPM as the loss of union direction since the Reuther days and a threat to auto workers' ability to protect their vital interests. Laurie Graham reports on participant-observation at the nonunion Subaru-Isuzu (SIA) plant in Indiana. She found that SIA depended heavily on inculcating workers with a "myth of egalitarianism" but that the gap between cooperative SIA rhetoric and workers' experiences with regimented work fostered individual resistance and "informal collective bargaining" (p. 66).

In the most informative essay, Steve Babson reports on survey research at the UAW-organized Mazda plant outside Detroit. The results cast considerable doubt on the implementation of JPM. Workers overwhelmingly reported that their jobs are highly regimented—as in traditional Fordist plants—with virtually no opportunity for employees to alter the work routine; changes are imposed by management. Moreover, half complained that training is poor and that job rotation is corrupted by favoritism. According to Babson, the UAW local intervened to demand contract language to improve training and job rotation opportunities, but he raises a broader point. He agrees that JPM does depend more heavily on the willing participation of the work force than do traditional Fordist plants. In Japan, a number of social and political institutions enable employers to win employee consent. But in the United States, he says, Japanese transplants have two options: go nonunion and employ traditional U.S. management techniques to control labor, or "nativize" JPM with the cooperation of an independent union. Even in the latter case the future of the new workplace is problematic because U.S. corporate governance privileges shareholders and managers over employees: Corporations will have a difficult time delivering the long-run employee security that is the quid pro quo in Japan.

James Rinehart, David Robertson, Christopher Huxley, and the Canadian Auto Workers Research Team report on the unionized GM-Suzuki joint venture in Ontario, where the implementation of teamwork failed because of experiences similar to those at Mazda. A chapter by Yanarella highlights training as the primary method for winning consent in JPM workplaces. He presents data on the use of government training funds—sometimes quite substantial—to support the implementation of JPM and then focuses on what types of training are provided at the Toyota plant in Kentucky and at Saturn. Yanarella reserves judgment about industrial relations at these plants because he argues that JPM still depends on political "forces operating at the local, national and continental levels" (p. 149). William Green argues that the New Deal labor governance is in crisis because of JPM (p. 169). This seems to overstate the case in light of the previous chapters' evidence about workers' continuing ability to shape worker-management relations. The most direct challenge occurs when non-union plants form work teams because this raises a dilemma in the interpretation of NLRA Section 8(a)(2), which prohibits company-dominated labor organizations: Is a work team a labor organization?

Finally, Donald Wells broadens the perspective from the workplace to the environmental conditions for the "striking" stability of JPM plants (p. 197). Rather than locate consent in the transcendence of the Fordist division of labor or the internalization of teamwork norms, he argues that workers' consent is based on widespread economic insecurity outside the core factories associated with the end of Keynesian policy. If true, then faster economic growth could result in industrial instability as workers assert their own conception of teamwork.

Fifty years ago political scientists were attracted to New Deal industrial relations governance to explain problems of consent, regulation, and political participation, but few studies were undertaken in the 1950s and 1960s. Now, with the changes in the conditions of labor, perhaps political scientists will again turn their talents to explaining the institutional connections between work and politics. These two volumes will be vital documents for the development of theories of governance.

The Politics of Rage: George Wallace, the Origins of the New Conservatism, and the Transformation of American Politics. By Dan T. Carter. New York: Simon and Schuster, 1995. 572p. $40.00.

Charles W. Dunn, *Clemson University*

Etched like an epitaph on a tombstone are Dan T. Carter's central conclusion and closing words about George Corley Wallace: "He was the most influential loser in twentieth-century American politics" (p. 468). Can this inscription withstand the weathering winds of criticism? How about William Jennings Bryan in the Democratic Party? Three times a candidate for president (1896, 1900, 1908) and once a presidential kingmaker (1912), Bryan anticipated the issues that would make the Democrats the majority party during the New Deal. His 1896 Cross of Gold speech ranks among the most important political addresses ever delivered, foreshadowing his influence in the Democratic Party over several decades. His 1912 address to the Democratic convention broke a party deadlock and delivered the nomination to Woodrow Wilson on the 46th ballot.

Or, how about Barry Goldwater? Once a presidential candidate (1964), Goldwater presaged the appeal of states' rights and the emerging importance of the South in U.S. politics. He laid the political foundation for the resurrection of conservatism and the resuscitation of the Republican Party. During the 1960 GOP convention in Chicago, Goldwater sounded the conservative battle cry to capture control of the party from eastern establishment Republicans with these famous words: "We can take this party back." Goldwater's bestseller, *The Conscience of a Conservative* (1960), became the right wing's clarion call to arms.

George Wallace never delivered a speech as important as Bryan's Cross of Gold nor wrote a book as influential as Goldwater's. Wallace reacted tactically to circumstances, consumed by the passion of political ambition. Indeed, Carter aptly titles the most important chapter of his book "The Moral Compass of Ambition" (p. 68). Political survival, not philosophical reflection, tattooed the arm of Wallace's career. He personified political pragmatism at its worst, so much so that a dark cloud hangs over his latter-day civil rights conversion. No eraser can wipe from the chalk board of human memory the image of Wallace standing in the schoolhouse door, pronouncing: "Segregation today, segregation tomorrow, segregation forever."

Competing forces squeezed out of Wallace his true character. Throughout life Wallace faced the tensions of the Old South versus the New South, and political opportunity versus personal loyalty. Until the sun began to set on Wallace's life, he chose the Old South and political opportunity. Wallace pressed hard on the red-hot button of racial prejudice to activate his constituency.

"The Origins of the New Conservatism," emblazoned in the subtitle like a flashing neon sign on a marquee, promises a revelation about the rising tide of conservatism in U.S. public life. But does the book live up to its billing? A microscopic search of the Table of Contents and the Index reveals only one use of the word *conservative*, "The Conservative Society of America" (p. 295). That, however, reveals Carter's misuse of the word. This organization, headed by a former John Bircher, joined with a motley group of political personalities and organizations to help choreograph Wallace's career on the national political stage. Strong racist and anti-Semitic ties bound these entities together. They included Joseph Welch's John Birch Society, the Reverend Carl McIntire's Twentieth Century Reformation, Dr. Fred Schwarz's Christian Anti-Communism Crusade, the Reverend Billy James Hargis's Christian Crusade, Edgar Bundy's Church League of America, Dean Clarence Manion's Forum, Texas oilman H.L. Hunt's Life Line Foundation, William Simmons of the White Citizens Council, Willis Carto of the Liberty Lobby, former governor of Mississippi Ross Barnett, and Louisiana segregationist Leander Perez.

These persons and groups, like Wallace, were not conservatives but reactionaries. They merit nary a mention in almost all the voluminous literature on post-World War II conservatism. For example, Dunn and Woodard's (1996) *The Conservative Tradition in America* contains zero references to Wallace and all the others. They were a backwater in U.S.

politics. George Wallace failed to affiliate with the burgeoning conservative movement but instead cast his lot with forces intent on turning back the clock of progress. To the extent that he had any liaison with conservatism, it was purely pragmatic and tactical, consistent with his career, and opportunistically designed to further his own political ambitions.

Now, did Wallace identify a new constituency to which contemporary conservatism could appeal? Yes, he did, but so did others in different ways. And long before Carter, other authors documented that constituency. Carter adds nothing new here.

Wallace identified a deep alienation from America's two major parties, especially among lower-middle-class and lower-class whites both in the North and South. His third-party candidacy in 1968, which received 13.8% of the popular vote, signaled to the major parties the depth and breadth of this alienation. Richard Nixon and the Republicans saw the signal first, but today both parties respond to Wallace's social agenda on welfare and other issues.

Despite these weaknesses, Carter's book is outstanding: especially well written and thoroughly documented. Carter captivates the reader with superlative integration of personality, family, politics, ethics, society, rhetoric, federalism, economics, tradition, religion, leadership, group dynamics, and history. A tough of intrigue and a dash of conspiracy sharply season the plot. Carter tells a good story, and political scientists would do well to return to the days when they told stories well. The resurrection of political biography, a lost art among political scientists, would expand our discipline's appeal and influence. The literature from such fields as political leadership, political psychology, and political sociology would strengthen the literature on political biography. As it is, political scientists leave historians to dominate this field of play. Perhaps that is why some political scientists now identify with a new and growing national scholarly organization, The Social Science History Association.

Label *The Politics of Rage* as "a must read" for anyone wanting to understand the transition of the Old South to the New South and for any political scientist desiring to resurrect the scintillating disciplinary interest of political biography! Should the American Political Science Association establish a new section on political biography?

The Politics of Child Abuse in America. By Lela B. Costin, Howard Jacob Karger, and David Stoesz. New York: Oxford University Press, 1996. 200p. $29.95.

Douglas J. Besharov and Jacob W. Dembosky, *University of Maryland*

News stories daily remind us of the horrors of child abuse and of the seeming inability of public agencies to protect even the children already reported to them. What is to be done? After surveying the hundred-year history of child abuse programs, Costin, Karger, and Stoesz propose the creation of regional "Children's Authorities" to implement a rights-based approach to child protection. The chapter containing this policy recommendation is the weakest part of the book. But, overall, the volume is an honest, thoughtful, and informative recounting of how much mischief has been done in the name of protecting children.

Beginning with an analysis of the political culture that surrounds the child protective system, the authors argue that contradictions between myth and reality frustrate its efforts. They point out that many professionals in the field insist on viewing child abuse and child neglect as unrelated to social class, even though most maltreated children are poor—and are members of various racial and ethnic minorities. Similarly, while the media and the public have become obsessed with sensational cases of sexual abuse, most cases involve moderate physical abuse or neglect. "Instead of sensitizing the public to the real problems of child abuse," the authors complain, "the media have used shocking details to transform child abuse from a social problem into a social spectacle" (p. 7).

The authors claim that the result has been a rapidly expanding "child abuse and neglect industry" (p. 23), consisting of high-priced lawyers, psychotherapists, and other specialists who cater to wealthy and middle-class children and adults who claim to have been abused. They argue that, "while poor children are forced to seek help in understaffed and underfunded public welfare agencies, middle-class victims are counseled by a growing cadre of mental health professionals who specialize in the trauma of sexual abuse" (p. 40). Here, the authors, like the media, may be overreacting to a few sensational cases. There is a child abuse industry, but it is fueled by government and foundation funding for services that go mainly to poor families. They are correct, however, to describe how polarized the issue has become, with those who lobby for system expansion (various child advocacy groups that benefit from increased spending) pitted against those who want to reign in the system (mainly an organization called VOCAL, Victims of Child Abuse Laws). So far, most politicians have not taken sides in the debate, perhaps because they do not understand what is happening.

Ironically, the first child savers were deeply aware of the relationship between poverty and maltreatment. In the late nineteenth century, middle-class reformers tried to alleviate such social problems as poverty and crime (and child abuse) by removing poor, immigrant children from dangerous homes and neighborhoods. This early movement gained strength from intense media interest in the Mary Ellen case (the first highly publicized case of child abuse), growing public support for government intervention into "underclass" family life, and the backing of the women's rights and animal rights movements. In the Northeast, especially, private charities known as Societies for the Prevention of Cruelty to Children (SPCCs) became responsible for child protection. The SPCCs successfully justified their interventions into family life with the notion of "child rights."

During the Progressive Era, child maltreatment receded from the public agenda. It was not "rediscovered" until the 1960s by physicians, who, with the aid of X-rays, became more adept at diagnosing nonaccidental injuries. A new paradigm emerged that cast maltreatment in medical terms, signaled by the emergence of the now familiar medicopolitical term, the "battered child syndrome." Over the next decade, a group of physicians led by C. Henry Kempe persuaded key members of Congress, including then Senator Walter Mondale, to enact the 1974 Child Abuse Prevention and Treatment Act (CAPTA). It established a National Center on Child Abuse and Neglect and awarded states special grants if they adopted common standards for defining, reporting, and investigating child maltreatment. As a result, all states now have mandatory reporting laws that require, under threat of civil and criminal penalties, most child-serving professionals to report suspected maltreatment. Most communities now also have specialized child protective agencies to investigate reports.

These reporting laws and associated public and professional awareness campaigns have been strikingly successful. In 1963, about 150,000 maltreated children were reported to the authorities. By 1994, the number reached more than three million, a twenty-fold increase. As a result, many thousands of children have been saved from serious injury and even death. The best estimate is that over the past two decades child abuse and neglect deaths have fallen from more than 3,000 a year—and perhaps as many as 5,000—to fewer than 1,300 a year. Despite this progress, substantial problems remain. As the authors describe, many endangered children continue to go unreported. Studies suggest that professionals fail to report more than half the apparently abused and neglected children they see. At the same time, however, agencies are being inundated by inappropriate reports. Since 1975 the proportion of cases substantiated after an investigation has declined from about 65% to as low as 25%. This flood of inappropriate reports is overwhelming the limited resources of agencies, and may help explain why 25–50% of child abuse deaths involve children previously known to the authorities.

The authors also complain about the weaknesses of research on the subject and inadequate funding for services, which lead to poorly trained staff, high turnover, poor working conditions, and insufficient screening and investigative procedures. Finally, and most important, they argue that the current "psychotherapeutic" paradigm, which stresses the psychological attributes of abusers, distorts public and professional responses. This explains the public's obsession with sexual abuse and the mistaken belief that child maltreatment is an "equal opportunity disease" (p. 150), affecting all social classes equally. To the authors, "ignoring the connection between socioeconomic class and child abuse does an injustice to the victims of child abuse, because it undermines the development of effective approaches for dealing with the problem" (p. 151). Thus, we should not be surprised that current approaches based largely on a psychological understanding of child maltreatment fail to help most families who pass through the system.

We are particularly sympathetic to this last point, having argued it ourselves. That is what makes the authors' policy recommendations all the more perplexing to us. They propose a new "paradigm" that reconceptualizes child abuse as a "public safety issue" (p. 165). Child protection would be "predicated on the rights of children to receive the same level of protection afforded other members of society" (pp. 10–1). "In order to provide children with safety, it is necessary to criminalize child abuse and neglect" (p. 181). To implement this new paradigm, they would create regional "Children's Authorities" to replace existing protective agencies. Similar to local school boards, these authorities would have an elected board of directors and an executive director overseeing all services for maltreated children. The authorities would screen all reports, investigate accepted reports, prosecute perpetrators, place children in protective care, conduct research, and provide various social services (mainly through contract), such as family support, foster care, and prevention and community education.

Most significantly, investigations would no longer be conducted by social workers but by police officers, because, the authors argue, child abuse "is first and foremost a criminal act requiring police intervention" (p. 181). Officers would pursue a "pro-arrest" policy (p. 181), immediately arresting all suspected abusers. Although such a policy has never been tested in cases of child maltreatment, the authors cite evidence (thrown into question after its initial publication) that, in some cities, the approach has reduced repeat offenses in cases of spousal abuse. They offer no other evidence that their ideas have been tried somewhat, somewhere, successfully.

Unfortunately, there is an analytic disconnection between the authors' proposal and their recognition that child maltreatment is intertwined with poverty, drug abuse, and related social problems. Their proposal is based on the mistaken assumption that parents fall into discrete categories of "abuser" and "nonabuser." The majority of parents reported for maltreatment are not criminals; rather, they struggle to care for their children under conditions of severe poverty. Child abuse is deeply rooted in the way we live and organize our society. Hence, the rhetoric of "rights" simply does not lead to the kinds of remedies that will work. As the authors themselves state, policymakers should focus on "the harsh economic and social problems experienced by poor households, many of which experience child abuse" (p. 151). Unfortunately, they failed to take their own advice.

Roads to Dominion: Right-Wing Movements and Political Power in the United States. By Sara Diamond. New York: The Guilford Press, 1995. 445p. $19.95.

Michael P. Federici, *Mercyhurst College*

The rise of the Right in the latter half of the twentieth century has gained the attention of scholars in a variety of academic disciplines as well as that of the popular media. The conservative movement in the United States warrants analysis because it has played an important part in shaping political culture and politics in recent decades. Sara Diamond's book, which began as her doctoral thesis in sociology, is about "how right-wing movements in the United States, from the 1940s to the present, moved from the sidelines of U.S. politics to center stage" (p. 2). She focuses on how right-wing groups succeeded in gaining political power, a topic that has been discussed in several scholarly and popular books. What differentiates Diamond's analysis from similar studies is the scope of her book. She is primarily interested in how "the Right," as a social movement, became politically powerful. Most studies of the conservative movement focus on organizational, ideological, or intellectual aspects of the rise of the Right. Diamond, by contrast, not only claims to be "historical" but also attempts to provide a more comprehensive study of the Right than the existing literature on the topic offers. She identifies her work as a "history of the U.S. Right" that explains the instrumental role played by right-wing groups in the recent conservative shift in public policy.

As a way of explaining the connection between right-wing movements and public policy, the book draws the distinction between "oppositional" and "system-supported" (terms that are omitted from the index) right-wing movements. *Oppositional* means opposed to "prevailing power structures," and *system-supported* means that the movements are supported by the existing power structures. This distinction is not particularly helpful to understanding the nature of right-wing movements, but Diamond uses it to distinguish her analysis of those movements from that presented by Seymour Martin Lipset and Daniel Bell. She argues that the Right has been more system-supported than the prevailing scholarly view contends. Her assumption seems to be that right-wing movements are anchored to established power structures and thus are more powerful and have greater political viability than other scholars have assumed.

Diamond identifies four "movements" that make up the Right in the United States: the anticommunist movement, the racist Right, the Christian Right, and the neoconservative movement. According to her, these have converged to form a power base which has been used to create the social and political change that she finds "tragic" and "perplexing." In fact, the title of the book indicates Diamond's ultimate point: The emergence of the political Right in America is dangerous because it is antiprogressive and noninclusive. Certainly, the Right includes groups that exemplify racism and other inimical ideas, but in the attempt to be "historical" and comprehensive, Diamond lumps together very diverse figures and groups, such as David Duke, George Bush, the KKK, Russell Kirk, militia groups, Ronald Reagan, Pat Robertson, and others. The Right is portrayed as a largely monolithic ideological movement that is determined to destroy civil rights and civil liberties in the United States.

While Diamond exaggerates the political and ideological continuity of the Right, her study of its rise explains how integral anticommunism was to forming a working policy consensus among right-wing factions. Since the fall of communism, the Right has lost one of its central unifying causes. The consequences for the Right's political success in the post-Cold War era seem to be the unraveling of its political coalition. Diamond's analysis leads to the conclusion that without an equivalent unifying cause, the Right is unlikely to return to the solidarity of the Reagan years.

The book does provide some useful but limited description of the various parts of what Diamond calls the "New Right." Her analysis follows the work of other scholars and divides the mainstream conservative movement into three groups separated by three public policy concerns. Each group—the Christian Right, Neoconservatives, and traditional or paleoconservatives—has a different focus. The Christian Right focuses on social issues, Neoconservatives emphasize foreign policy, and paleoconservatives focus on nativist issues.

Unfortunately, Diamond's analysis is colored by her ideological bias. She openly states that she is not sympathetic to conservative ideology. While she claims that her sympathies can be separated from her analysis, readers will have no trouble uncovering her bias. The book is littered with pejorative assertions about the Right and such comments as: "This book sets the stage for our understanding of some of the tragic and perplexing events we now witness" (p. 16). It ends with the statement: "Despite many obstacles to unity, together the Christian Right and the Republican Party hoped, by 1996, to throw the Democrats out of the White House and to enforce a new era of moral righteousness and economic severity, with a vengeance" (p. 312). The lack of scholarly detachment and philosophical depth detract from the credibility of the author's conclusions.

Perhaps the greatest shortcoming of the volume is its lack of theoretical and historical substance. The book is journalistically written and argued, and it lacks any serious philosophical analysis. The intellectual roots of conservatism need to be part of any work that claims to explain the motives of the political Right. Analysis of the intellectual and political roots of conservatism bring to light the significant differences among the various groups, figures, and ideas that Diamond discusses in her book.

While there is very little original scholarship in *Roads to Dominion*, readers may find it a useful review of the events and personalities that shaped the Reagan coalition and the ensuing conservative movements. The book, however, lacks a substantive conclusion and is preoccupied with public policy and politics, which gives it a narrow and short-term focus. To put the rise of the Right in perspective, a more historical and philosophical approach is needed. Moreover, Diamond's analysis lacks an understanding of the U.S. political process and constitutional system. Political movements cannot be understood apart from the political system that shapes their behavior. Nor can the rise of the Right be properly assessed outside the broader context of cultural developments in Western civilization.

Unions and Public Policy: The New Economy, Law, and Democratic Policies. Edited by Lawrence Flood. Westport, CT: Greenwood Press, 1995. 224p. $59.95.

Broken Promise: The Subversion of U.S. Labor Relations Policy, 1947–1994. By James Gross, Philadelphia: Temple University Press, 1995. 404p. $49.95.

Ruth O'Brien, *John Jay College of Criminal Justice, CUNY*

With U.S. labor unions still in a sixteen-year decline, historians and political scientists alike are searching for causes and explanations. The book by James Gross and the edited collection by Lawrence G. Flood do just that. At the same time, these books present recommendations for labor reform.

With *Broken Promise*, Gross completes a trilogy on the history of modern federal labor policy. Although he concentrates on the postwar period to the present, he opens with an explanation of the National Labor Relations Act of 1935, commonly called the Wagner Act, arguing that it was enacted to promote collective bargaining and industrial democracy. In 1947, the passage of the Taft-Hartley Labor-Management Relations Act undermined this policy by legitimizing employer opposition to unionization. It underscored the worker's right not to join a union. But Gross maintains that instead of supplanting the public purpose underlying the Wagner Act, Taft-Hartley Act another, contradictory policy to direct national labor policy.

Rather than focusing on the procedures and doctrines of The National Labor Relations Board (NLRB) or the federal courts' review of its decisions, Gross analyzes how labor policy was shaped by the president, Congress, the Supreme Court, resistant employers, public opinion, and NLRB members themselves. Reviewing the regulatory process, he maintains that this policy has been influenced by the board's manipulative tactics more than legislative changes. Under each administration, Gross argues, different members on the NLRB have held disparate views on collective bargaining, industrial democracy, and employer resistance to unionization.

Beginning with the first disagreement over the issue of noncommunist affidavits in 1947, Gross offers an account of the controversies the NLRB faced during the different administrations until 1994. To him, one of the most important changes brought by the Taft-Hartley Act was the institution of an independent office for the general counsel, who alone had the discretion to investigate and prosecute charges of unfair labor practices. Yet, since Congress had drafted only vague guidelines about the NLRB's day-to-day operations, controversy plagued the board throughout the 1950s. The NLRB's chairman, Paul Herzog, and its first general counsel, Robert Denham, each tried to gain the upper hand in determining what powers and duties the respective offices would hold. After President Eisenhower took office, Gross suggests, neither his administration, Congress, nor the NLRB did a better job determining whether labor policy should promote collective bargaining or the rights of individual workers.

Under the chairmanship of Frank McCulloch, who served on the board during the Kennedy and Johnson administrations, the NLRB gained more direction as it not only reversed the Eisenhower board doctrines but also facilitated worker self-organization and industrial democracy. But, Gross argues, the NLRB's effect on national labor policy was diminished in the 1960s by the Supreme Court, which rendered a narrow interpretation of the employer's obligation to bargain with workers. At the same time, more employers began to resist unionization. The Democratic Party's relationship with organized labor had also changed by the 1960s. According to

Gross, liberal Democrats no longer recognized unions as a force for social reform.

The NLRB, run by Edward Miller during the Nixon years, further exacerbated organized labor's problems. The author purports that the Nixon administration initially adopted a conciliatory relationship with organized labor because of the latter's support during the 1968 election, among other things. Yet, pressured by the business community, the Nixon administration changed its position, and the Miller board gave employers' more discretion over management issues than had the McCulloch board. Chaired by John Fanning during the Carter administration, the NLRB withdrew some of its predecessor's doctrines, but Gross maintains that the congressional fight over the common situs bill exposed what little support organized labor had left among the Democrats. Finally, when President Reagan appointed Donald Dotson, who often represented the employer's interest over organized labor, labor policy was modified once again. According to the author, the Dotson board made a more dramatic change than any NLRB since 1947 by essentially giving employers a "green light" on union busting.

Gross concludes that U.S. labor policy has been at cross-purposes since passage of the Taft-Hartley Act. It is governed by the politics and personalities of those who serve on the NLRB as well as the ideological battles waged over labor-management relations in Congress, the executive branch, and on the federal bench. By contrast, Gross insists that the "beauty" of the NLRB established by the Wagner Act stemmed from the fact that its legislative authors built it on the basis of "empirical evidence" gathered during their involvement with the two preceding labor boards.

Gross's comparison between these two periods is the least convincing aspect of a book that otherwise provides a much-needed and well-argued history of the postwar operation of the NLRB. Gross also overstates the significance of the provisions for industrial democracy in the Wagner Act. Few labor historians would agree that Senator Robert F. Wagner's preference for industrial democracy was fully accommodated by the NLRB. As a result, it is not surprising that the opponents of the initial NLRB had little difficulty undermining this policy from 1947 onward. Since Gross does not answer these questions nor engage with the historians and political scientists who do, his conclusion that industrial democracy should be regarded as a prerequisite for a democratic polity seems more like a pious wish than a feasible recommendation for reform.

Unions and Public Policy also adds to our understanding of modern labor policy. The essays are divided into three substantive parts, and the first set explores how the new "fragmented" workplace and employee involvement in management have affected organized labor. Françoise Carré, Virginia duRivage, and Chris Tilly chart the emergence of the fragmented workplace. While some workers prefer the flexibility of temporary, part-time positions, most have no choice but to accept these conditions. According to these authors, organized labor should respond to these changes by trying to secure the same wages and work conditions that permanent, full-time workers receive. In an essay on employee involvement, Stan Luger examines how the global economy affects the shop floor. After seven years of private disinvestment between 1969 and 1976, 30% of U.S. plants were closed. For Luger, employee involvement represents the best hope of stopping this trend.

In the second part of *Unions and Public Policy*, which is devoted to economic issues, John Russo provides a description of corporate restructuring, the most neglected aspect of union decline. Corporate managers justify restructuring with the argument that they must maximize their profits and ensure U.S. competitiveness in the global marketplace. But to Russo, such restructuring has proved inefficient. He concurs with organized labor's assessment that this strategy has been used primarily to reduce labor costs. In a discussion of unions and capital, Peter Pitegoff submits that organized labor could wield enormous power over the market through the way it manages its $2.5 trillion pension plan. Labor leaders, for instance, could target investment patterns to promote unionization. Finally, Gerald Glyde reviews free-trade policy, offering an explanation of the different motivations behind the actions of the U.S., Canadian, and Mexican governments. He suggests that NAFTA should be modified to reduce the amount of harm it causes workers.

The final segment of *Unions and Public Policy* addresses democratic policy. Susan Jennik argues that labor leaders should maintain better self-regulation than they have in the past. Analyzing the Landrum-Griffin Labor Management Relations Act of 1959, among other pieces of legislation, Jennik contends that union democracy law favors institutional rather than individual workers' interests. Bruce Nissen discusses a different type of worker participation. Like Jennik, he is critical of national labor leaders who do not get involved in local battles against plant closures. Local union leaders, moreover, often respond to shutdowns by securing severance benefits for their members when they could fight for job retraining, search, and transfer rights. Lawrence Flood, William Scheuerman, and Sidney Plotkin investigate the ideological struggle over the role of the state and how it affects public sector unions. Since the 1970s, they argue, those unions and their workers have been maligned as slothful and overcompensated. To counter these attacks, the authors suggest that public sector unions develop strategies to address directly the political nature of the federal government's actions, for instance, by cultivating support from their own clientele. Finally, James Atleson evaluates the possibility of enacting stronger labor laws, arguing that organized labor's marginal position is the result, rather than the cause, of these weak laws.

Some of the essays in *Unions and Public Policy* share the problems of *Broken Promise* with regard to offering feasible recommendations for reform. For example, the essays by Luger and Jennik criticize the strategies chosen by organized labor without fully addressing the opposition it faced and how this opposition continues to make employee involvement and union democracy problematic. Pittegoff and Glyde present useful information about trade policy and capital but do not place it in a wider political context. Finally, Atleson does not fully explain how a stronger labor movement could have secured greater legal protection. Nonetheless, both books provide valuable information and new insight about modern labor policy.

On the Limits of the Law: The Ironic Legacy of Title VI of the 1964 Civil Rights Act. By Stephen C. Halpern. Baltimore, MD: Johns Hopkins University Press, 1995. 391p. $55.00 cloth, $18.95 paper.

Stephen L. Wasby, *SUNY at Albany*

In this major addition to the literature on civil rights policymaking, Stephen Halpern focuses on an understudied section of the Civil Rights Act of 1964, Title VI, which forbade discrimination on the basis of race or national origin in programs receiving federal funds and authorized termination of such funds. In providing Title VI's history from its origins through the Reagan administration, Halpern gives particular attention to its use in the fight to desegregate educational systems. This volume adds much to earlier studies by Orfield and Radin of the HEW guidelines for school desegregation and extends over a much longer period than those works. Although Halpern examines some major litigation, his primary focus is on implementation of a regulatory regime. That makes this study an important complement to more frequent standard treatments of desegregation litigation, a result of political scientists' focus on constitutional law and on civil rights litigation, which leads to insufficient attention to, and undervaluation of, other means for achieving constitutional goals. This book is an important corrective to that skewed emphasis, although Halpern also gives considerable attention to litigation to enforce Title VI.

Halpern first provides Title VI's legislative history and reviews the provision's elements, which were limited by compromises necessary to obtain its passage. It should be noted that when the 1964 Civil Rights Act was being debated, Title VI did not receive the most attention; that honor went to the public accommodations provision, Title II, because of the publicity the sit-ins had received. Title VI might have encountered more resistance or might have taken a different form had it received more attention, which might have occurred if major programs for federal funding for education already had been in place; they came a year *later* in the Elementary and Secondary Education Act.

Next, Halpern looks at the beginnings of the HEW guidelines and their intersection with judicially developed policy, noting the irony of dependence on courts for a provision intended as an alternative to courts (p. 76). He then moves on to the guidelines' alteration during the Nixon-Ford administrations, when there was the shift to the focus on procedural rights and managerial issues (p. 81) that was to characterize much of the remainder of the period examined. In the Carter years, there was also a shift to attention on higher education at the same time Congress constrained Title VI's enforcement through the Eagleton-Biden Amendment, which restricted requiring busing to achieve desegregation. Under the Reagan administration came the *Grove City College* case (1984), in which the Supreme Court undercut Title VI by upholding the administration's position that it applied only to programs receiving federal funds, not the institutions of which they were a part; this ruling was overturned in the aptly named Civil Rights Restoration Act. According to Halpern, this period shows us the "irrelevant formalism of legal battles" (p. 190) as those seeking the enforcement of Title VI in litigation were put on the defensive.

Throughout his study, Halpern tracks the inadequately known *Adams* litigation, usual in that plaintiffs sought to force the Office of Civil Rights (OCR) to carry out its statutory (and moral) responsibilities. Halpern explores this crucial case, one of the most important in civil rights history, although it never produced a ruling from the Supreme Court, from its initiation in the 1970s through its continuation in the Carter administration, when there was a shift away from accomplishing school desegregation to dealing with management issues, to its ultimate dismissal during the Reagan years over a question of standing to sue resulting from the Supreme Court's *Allen* v. *Wright* (1984) decision. Despite the *Adams* plaintiffs' partial victory at intermediate stages, Halpern correctly argues that the case shows an inadequate return from extensive litigation. In it we can see how, during the litigation's extended duration, other interests wanted a piece of the action and thus "piled on" the case; ironically, this gave the OCR an opportunity to avoid acting on behalf of those whose rights it was created to implement, since it provided excuses not to concentrate on race and education because other groups' discrimination claims also needed attention.

The other major case to which Halpern devotes attention—*Fordice* (1992), the Supreme Court's only major contemporary ruling on desegregation in higher education—was itself a result of *Adams* in that HEW referred the case to the Department of Justice, which brought suit. Halpern calls *Fordice* "anticlimactic and disappointing" (p. 279) because it settled little and gave little guidance. In his view, this case both reaffirms the importance of litigation and illustrates that legal constructs bury important educational and social justice issues (pp. 252, 255).

In concluding his study, Halpern examines enforcement of Title VI and factors affecting that enforcement, including organized groups; particularly important is the need for (and absence of) clear standards. Title VI, he asserts, is "a weapon of substantial potential power" (p. 319), but both the statutory provision itself and litigation undertaken to enforce it were failures (p. 321); moreover, "a high level of frenetic but ineffectual activity" (p. 284) can undermine the achievement of civil rights.

Aside from what it effectively tells us about the use of the executive branch as a mechanism for achieving civil rights policy, Halpern's volume is also an important addition to recent studies of the consequences of thinking in terms of rights and the law. Here, his arguments serve as a complement, and at times a reinforcement, to others' views. When Halpern refers to legal rights as a limited strategy which is not a substitute for political action, his argument is like Stuart Scheingold's in *The Myth of Rights* (1974). Halpern believes that, with respect to education, thinking in terms of law and rights turns one way from the quality of education; in saying that and also that law is irrelevant to the contemporary urban dilemma, Halpern sounds like Derrick Bell, whose influence Halpern acknowledges. Some of the conclusions noted above are also like Gerald Rosenberg's in *The Hollow Hope* (1991). Halpern is not as pessimistic as Rosenberg, however, and does not take the latter's broad stance as to law's inability to bring about rights. *On the Limits of the Law* is instead a tough-minded, close look at what law can and cannot accomplish using a particular tool (Title VI) in a particular area of policy (education).

The Higher Civil Service in the United States: Quest for Reform. By Mark W. Huddleston and William W. Boyer. Pittsburgh: University of Pittsburgh Press, 1996. 229p. $49.95 cloth, $19.95 paper.

B. Guy Peters, *University of Pittsburgh*

Political appointment as a means of selecting top management and policy positions in the executive branch of the U.S. government is a familiar target for reformers. The failure to develop a cadre of permanent civil servants for managing federal organizations and programs has been cited frequently as one of the failings of the system and a reason, if not the reason, for its allegedly poor performance. Huddleston and Boyer join in the attack on the "government of strangers" by examining the quest for a senior management group composed of career civil servants throughout much of U.S. administrative history. Unfortunately, this book adds little to the ongoing debate, other than a better understanding of how long the struggle has been going on.

The authors do perform an admirable service in detailing the history of the senior civil service in the United States from the early twentieth century until the mid-1990s. The passage of the Civil Service Reform Act and the creation of the Senior Executive Service during the Carter administration were the culmination of a long struggle to create a leadership group in the civil service that would be independent of particular organizations and that could function as such groups did in Western Europe. This history points to the complexities of "the separated system" in the management of the civil service and to the consequent difficulties faced by the chief executive in managing his own branch of government. Almost every president has sought to gain greater control over the civil service, and especially the senior civil service, but significant legislation was denied them until the Carter administration. That legislation has been far from successful in solving the policy and management problems in the federal government, and the pursuit of an improved system continues.

The historical aspects of this volume are very useful, but for a book interested in and advocating contemporary reform, the description and analysis tend to end prematurely. The discussion stops abruptly with adoption of the Civil Service Reform Act of 1978. There is very limited examination of the Reagan administration and almost none of the Bush or Clinton years. Certainly, if the issue needing reform is political appointments, then the Reagan administration's use of the Senior Executive Service for partisan appointees should have been a central feature of the analysis. Likewise, attempts to reassert the importance of the senior civil service through the Volcker Commission and other groups remains largely unmentioned. Some contemporary issues for reform are mentioned, but they are treated in an overly general and largely dated manner.

One of the more welcome aspects of the analysis, at least in principle, was the comparative discussion. This sought to place the U.S. experience in the context of other industrialized democracies and of imperial China. In practice, however, this comparative thrust appears to have two serious flaws. The first is that the comparison was introduced at the beginning of the book in rather sketchy form (only 16 lines to describe Germany) and then disappeared from view, whereas it should at least have been reintroduced in the conclusion to illuminate the evidence about the United States.

The second and more serious problem with the comparative discussion is that it is now very out of date. The interesting point to consider is that many of the countries being extolled by the authors for having a permanent senior civil service are moving rapidly in the American direction of short-term appointments for reasons of partisan loyalty and/or presumed administrative efficiency. This change can be seen in the United Kingdom and several of the Commonwealth countries, notably New Zealand. Furthermore, the comparative discussion in this volume tends to disregard the well-documented politicization of the senior civil service in Germany, France, and other European countries. The interesting question then appears to be not why the United States has politicized appointments but why this presumably inefficient practice is becoming ever more popular.

Reform of the civil service, and the executive branch in general, is a continuing quest in U.S. government. This quest has extended over many decades but may be little closer to producing the type of civil service sought by reformers. This volume tells a great deal about the history of this quest, but it tells all too little about the present or the future. Administrative reform currently is on the agenda of almost every country in the world, including the United States, and there is a need for considering issues and options that are touched all too lightly here.

Diversity and Citizenship: Rediscovering American Nationhood. Edited by Gary Jeffrey Jacobsohn and Susan Dunn. Lanham, MD: Rowman & Littlefield Publishers, Inc., 1996. 136p. $49.50 cloth, $18.95 paper.

Richard C. Sinopoli, *University of California, Davis*

This collection of six essays was originally presented as lectures commemorating the bicentennial of the founding of Williams College. Its purpose, the editors tell us, is to explore the "obligations and character of contemporary citizenship" in the United States in light of the worldwide reemergence of national identity and its associated discontents in the post-Cold War era. Curiously, the editors attribute the heightened domestic interest in the character of citizenship to the U.S. status as a spectator of these international events. This underplays the extent to which multiculturalism and identity politics have raised questions about political membership in an increasingly diverse polity and would have done so regardless of events in the former Soviet Union and elsewhere.

The essays are quite accessible to a general readership. They also range widely—perhaps too widely to maintain the thematic integrity one might expect even from an edited volume. Consistent with the editors' view of the issues at hand, more attention is given to the citizenship than to the diversity side of the ledger. Only one piece, by Randall Kennedy, deals with a specific minority or ethnic group, and it focuses on the legal status of African American "freemen" prior to the Civil War. Kennedy's essay is, by the way, an effective rejoinder to what he calls the "Taney-Marshall"—Roger and Thurgood—view of this status, "in which anti-black, proslavery racism is uniform, pervasive, unlimited, and thus wholly triumphant" (p. 115). Kennedy paints a more complex and accurate portrait, following the lead found in the deservingly admired dissents to Taney's *Dred Scott* opinion. He argues that a "countertradition" including strains of racial egalitarianism precedes and helps explain the adoption of the Fourteenth Amendment.

Whatever its limitations, this volume does address important issues about U.S. citizenship historically and as it is understood today. Historian Pauline Maier examines colonial and local declarations of national independence circa 1776. She gives more attention than usual to federalism, a "masterpiece of eighteenth-century political engineering," as an essential aspect of U.S. nationhood (p. 54). The interests of newly founded states and of the United States were understood to be mutually dependent but by no means identical. The emerging federal system combined "communities with distinctive cultures, traditions, and interests in a larger whole," thereby "creating one out of many" (p. 54).

Political scientists Robert Dahl and Noah Pickus make similar arguments in separate essays. Both address U.S. citizenship as a virtue or disposition rather than a legal status. Dahl argues (though not with much originality) that a pluralist democracy cannot sustain classical republican "civic virtue," but neither can it survive based simply on egoism. He calls for a middle ground of sorts, described as "robust civility," where the public good is conceived "less as substance and more as process" (p. 13). Pickus argues in essence that it is just such a notion that Madison, as chief designer of the Constitution, tried to foster. The Constitution establishes a deliberative process through which diverse interests are brought together to define a common identity. Pickus even suggests that Madison opposed the initial inclusion of a bill of rights as it might curtail what is essential to politics: "deliberation over the relation between rights and powers" (p. 77). There is no direct evidence for this claim, and why Madison would presume what history demonstrates to be so clearly false is not explained. If not always convincing in the particulars, Pickus is more so in showing that Madison saw the political process as a means not merely to register exogenous interests but to mold and soften those interests through a process of accommodation, a point many critics of Madison, obsessed with *Federalist* No. 10, continue to miss.

Nathan Glazer raises several interesting paradoxes regarding contemporary views of U.S. citizenship and immigration. Among these is our propensity to celebrate our openness and diversity of origins while demanding a more thorough "change of heart and identity" than do more restrictive nations (p. 88). This observation speaks to the difficulty of attaining the purely procedural notion of U.S. identity that Dahl seeks. To be an American is seen by many as a matter of heart and soul, for example, a bearer of a certain character type, usually associated with the "frontier virtues" elegantly mythologized by Frederick Jackson Turner. Furthermore, as Glazer notes, many or most Americans are more likely to see people of European descent as "more legitimately American" than nonwhite immigrants (p. 88). As attractive as Dahl's vision of American identity may be, Glazer's observation leads us to question whether "Americanism" and proceduralism are now or will ever be quite so thoroughly unified as Dahl hopes.

Glazer also suggests that driving current moves toward restrictions on immigrants—legal and illegal—such as Proposition 187 in California, are fiscal concerns rather than "racist fears and antagonisms" (p. 97). Certainly, the costs of educating children of illegal immigrants and providing health care to undocumented aliens are substantial. One suspects, however, that Glazer's view of the motives behind these restrictionist positions is overly rosy. There is considerable evidence that citizens overestimate the economic harms of immigration and ignore its benefits. It appears that nativist and racist fears drive the economic assessments people make about immigrants as much or more than economic concerns determine their attitudes toward immigration.

In sum, one might have expected a book with this title to address some of the more perplexing issues of diversity and identity as they have been and are played out in U.S. politics and society. An essay on diversity and education would have been welcome, for example, given the occasion of these lectures. Nonetheless, this volume offers a good starting point for an educated general readership to begin thinking about our pluralist democracy and the forms of civic attachments and obligations it can sustain.

Midterm: The Elections of 1994 in Context. Edited by Philip A. Klinkner. Boulder, CO: Westview Press, 1996. 202p. $75.00 cloth, $19.95 paper.

Herbert F. Weisberg, *The Ohio State University*

The unexpected election of a Republican Congress in 1994 was the equivalent of a political earthquake. Therefore, it is appropriate that this election is the exception to the usual rule that midterms are not worthy of book-length attention. Clyde Wilcox's (1995) *The Latest American Revolution? The 1994 Elections and Their Implications for Governance* is a nice early summary volume that provides election interpretation along with a discussion of the resultant 104th Congress. Everett Ladd's (1995) *America at the Polls 1994* is a useful compendium of both polling and election results. The quick aggregate analysis by David Brady, John Cogan, and Douglas Rivers (1995) in *How the Republicans Captured the House* finds that the victory was due to voters in moderate and conservative districts rejecting Democratic House members who had supported President Clinton on key votes. Philip Klinkner's edited volume is a useful addition to this literature. The authors of the ten chapters provide detailed coverage of several aspects of the election.

Popular accounts of the 1994 election point to such factors as the angry white male, the Christian Right, the white South, and well-funded Republican candidates. Each of these factors is explored in *Midterm*, with results that often conflict with those popular accounts. Reeher and Cammarano tackle the topic of the angry white male, finding a turnout effect as well as dissatisfaction with Democratic policies affecting voting by men, but they do not provide easy estimates of the magnitudes of their probit effects. Soper explains the role of the Christian Right in terms of its pragmatism in not emphasizing social issues in 1994, a decision by leaders of the Christian Coalition that diluted their ability to push those issues in the 104th Congress. Frymer argues that voting change in the South is a case of slow realignment toward ideological consistency rather than dealignment, but whether dealignment continued across the rest of the nation is not addressed. Eismeier and Pollock show that the GOP victory was not due to a tidal wave of PAC money flowing to Republicans.

Inevitably, some other popular explanations of the 1994 election are not explored. In particular, the claimed lower turnout of liberal voters is not discussed, though that is an alternative interpretation for some of the results shown in these chapters. A family values effect is shown, but marriage and parenthood effects evident in 1992 voting could help put the angry white male argument in context. The lack of change in party identification prior to the election is not discussed, though that could temper the view that 1994 was a realigning election. It would also have been useful if different explanations of the election outcome, including those offered in this book, were posed against one another in more multivariate analyses of survey data.

The election outcome was dramatic enough to have potential implications for our usual theories about congressional elections. Gary Jacobson finds that the electoral swing was not uniform and that the electoral value of incumbency was still large, contradicting the usual view of increased nationalization of politics at the expense of incumbency in the election. Jacobson concludes that local variation remained important, with the extent to which national issues mattered depending on whether candidates could turn them into local issues. Maisel, Ivry, Ling, and Pennix report that several successful Republican amateur candidates funded their own campaign, a new pattern which could affect our theories of candidate recruitment and emergence. They also show, however, that the strategic politician hypothesis was not repealed. As would be expected in a year that looked good for Republicans, experienced GOP challengers had a high level of success, while few Republican incumbents faced quality challengers. Interestingly, both of these chapters find some support for standard views, regardless of the large shifts in the election results; there was consistency even amid the considerable change of the 1994 election.

While the major emphasis in *Midterm* is on the House side, Franco Mattei nicely details the Senate elections. A chapter on gubernatorial contests would have a been another useful addition to a book on the "Elections of 1994."

Three of the most thoughtful chapters provide interpretations of the election. David Mayhew puts 1994 in the context of earlier "innovative midterms" that led to new dominant policy coalitions (1810, 1866, 1910, and 1938), but he fails to identify a causal factor that the five hold in common, which could show that 1994 will not be seen as a major change when we look back at it thirty years from now. John Pitney and William Connelly analyze the newly achieved majority status of what was looking like a "permanent minority" party in terms of another turn of a Rubik's Cube of ideas (the Republican Contract with America), interests (especially the Christian Coalition), and institutions (united Republicans being able to channel voter dissatisfaction against a unified Democratic government), as shaped by individuals (principally Newt Gingrich). The most intriguing interpretation is that of editor Philip Klinkner, who argues that the dynamic of the election was not a referendum on President Clinton as much as a "Court-versus-Country" election. A replay of a classic battle between a pro-state Court party and a limited government Country party was made possible when Clinton aligned himself with congressional Democrats. It is not clear whether this represents a new enduring party divide, however.

This book was written before the consequences of the 1994 election for Congress and for future elections could be fully assessed. Until its final months, the 104th Congress produced more political gridlock and less policy change than originally seemed likely. The House passed most of the Contract with America in its first 100 days, the Republicans changed the standard operating procedures in the House, and Newt Gingrich moved the House speakership back to the center stage of U.S. politics, but the net policy effect of the election was less than expected when these chapters were written.

Secret and Sanctioned: Covert Operations and the American Presidency. By Stephen F. Knott. New York: Oxford University Press, 1996. 258p. $27.50.

Loch K. Johnson, *University of Georgia*

The chief purpose of this book is to convey an understanding of how the United States has often relied on secret activities to achieve policy objectives. Stephen Knott reminds us that some of the gods in the American pantheon—Washington, Jefferson, Lincoln—resorted to unsavory covert practices that today would be the subject of criticism.

The study has a second objective. The author argues that a reliance on executive discretion over intelligence activities has served the nation well in the past. These operations are frequently delicate and perishable; they rely on secrecy, flexibility, timeliness, and efficiency—all lost when Congress enters the picture.

Knott is impressed by precedents set in the nation's early history and rightly so, for the accomplishments of the founders are impressive. The enthusiasm, however, can be carried too far. During the Watergate scandal, defenders of the Nixon administration maintained that the president had done nothing more than earlier administrations. "I do not share this view," responded a House member during the impeachment proceedings, "or the view of those who hold that all presidents have lied, have broken the law, have compromised the Constitution. And if George Washington accepted bribes, it would not make bribery a virtue, nor would it be grounds for overlooking such acts by his successors." Similarly, Knott's finding that earlier presidents engaged in questionable intelligence operations does not condone the practice.

The author is a passionate critic of the post-Watergate rebellion against the imperial presidency. He laments the "myth of innocence" that befell reformers on Capitol Hill. Indeed, he views this book as a reminder of how our most venerated leaders were willing to engage in operations that today would send pantywaist legislators to the press room crying foul. He encourages America to don its covert fighting gloves again and to dismiss Congress as a ringside manager.

"The most important reform that should be made to the current system," he writes, "would be the elimination of the intelligence committees and the restoration of the system that existed from 1947 to 1974" (pp. 184–5). The system he prefers is one of overwhelming executive dominance in decisions about how the United States will fight secret wars, spend billions of dollars on spy machines, and develop clandestine relationships with U.S. journalists, scholars, and clergy. He recommends as well repeal of the "ludicrous" (p. 184) executive order prohibiting the assassination of foreign leaders, signed by President Gerald R. Ford in 1976.

Despite the commendable value of his historical research, Knott is on less firm ground when he turns to the modern era. (He cites but one interview, with a former CIA official from the 1950s.) His first misstep is to assume that congressional reformers in 1975 failed to appreciate the early evolution of intelligence activities; indeed, they "distorted" and "misrepresented" this history (p. 5). The author quotes a passage from the report released by Senate investigators stating that covert action had been used "for the past twenty-eight years" (p. 5). Yet, as even a cursory reading of the multivolume report indicates, this phrase was used in volume one simply because legislators elected to focus their remarks on the modern era (in a supplementary volume, they issued a history of early American intelligence).

The committee members were certainly aware of covert action precedents. Senator John Tower (R-Texas), the panel's vice chairman, regaled colleagues with stories (provided to him by the CIA) of covert action carried out by Washington, Jefferson, and even Benjamin Franklin. So did the intelligence director, William E. Colby. Moreover, the CIA sent to every member a slickly printed history of covert action. So the notion that legislators suffered "historical myopia" (p. 169) about earlier intelligence practices is the real myth.

What disturbed senators (who established the new intelligence oversight committee by a compelling vote of 72–22) was the extent to which the modern intelligence agencies had violated the law and their charters. Most legislators were shocked by the discovery of assassination plots (including the hiring of mob hitmen to dispatch Fidel Castro); the creation of more than a million intelligence files on U.S. citizens; illegal mail openings, wiretaps, and cable interceptions; drug experiments against unsuspecting citizens; illegal sequestering of chemical-biological materials; a master White House spy plan against U.S. citizens; an intelligence scheme to promote the suicide of Dr. Martin Luther King, Jr.; the infiltration of the media, universities, and church groups; the incitement of violence among African Americans; covert harassment against Vietnamese War dissenters and civil rights activists; and covert actions directed against not only autocracies but also democratic regimes (including efforts to overthrow the constitutionally elected president of Chile).

Despite this chilling record, Knott insists that the presidency is the office "best suited" to decide on covert operations (p. 6). On the contrary, the wisdom of the founders lies not in their use of covert action (although this instrument may be necessary from time to time) but in their understanding of the corrupting nature of power. Their enduring contribution to the art of governing was the establishment of a system that incorporates safeguards against the concentration of power.

Throughout most of the Republic's history, secret operations remained small and peripheral. Now the intelligence establishment has grown vast, beyond the capacity of the president alone to monitor. The Congress would have to help. Yet, in Knott's opinion, this legislative supervision ("micromanagement") has only stymied the secret agencies. Most of the intelligence directors since 1975 take a different view; they have welcomed the opportunity to share their heavy responsibilities with Congress. And no administration has sought to repeal the core set of laws and procedures that presently guide intelligence activities.

Nor do legislators want to see a recurrence of the Orwellian nightmares discovered in 1975. They understand, too, that the nation continues to possess, when needed, a viable covert action capability. According to the author, the new oversight has caused the CIA to shy away from this option; but, in fact, its most extensive use occurred during the Reagan years, well after the reforms were in place.

Knott does Congress a further disservice. Studies on the subject of leaks consistently trace the overwhelming majority back to the executive branch. If the author insists on charging the staff of the intelligence committees with the unauthorized disclosure of information (p. 177), he should provide evidence—not innuendo—to support the claim. The members and staff of these committees have been individuals of the highest integrity and deserve better than this.

Knott thinks the oversight exercised by Congress from 1947 to 1974 was sufficiently vigorous (p. 163). Every other study disagrees. The author also attempts to defend the relations between Congress and William J. Casey (President Reagan's intelligence director). The fact remains, however, that his standing on Capitol Hill reached rock bottom. He had nothing but disdain for the legislative branch and even managed, in a Herculean feat, to alienate the CIA's arch-defenders (including the Senate oversight chairman Barry Goldwater, R-Arizona).

The choice is, of course, not between executive or legislative supremacy over intelligence; the challenge is to tap into the best attributes of both branches. Congress brings to the table a strong sense of what the American people will support, plus a large amount of foreign policy expertise in its own right. It provides a second opinion, carefully tendered in executive session by a small group of legislators in each chamber.

During the Iran-Contra investigation, Vice Admiral John M. Poindexter (the president's national security adviser) conceded that he bypassed the intelligence oversight committees to avoid "outside interference." This extreme form of executive discretion is, evidently, what the author seeks as well; secrecy and efficiency are his ideals, not the preservation of safeguards against abuse.

What riles Knott the most are the "ill effects" democracy brings to intelligence decisions (p. 179). In this domain, debate must remain hidden largely behind closed doors. The new system of oversight, however, provides an opportunity for some degree of outside review. The alternative advocated in this book is covert operations by executive fiat. The unfortunate consequences of that approach, well documented by investigators in 1975 and again in the wake of Iran-Contra, remain seared in the memory of many observers, if not Knott's.

American Exceptionalism: A Double-Edged Sword. By Seymour Martin Lipset. New York: W. W. Norton and Company, 1996. 352p. $27.50.

Sidney Verba, *Harvard University*

Like the children of Lake Wobegon, all nations are exceptional, but the United States is more exceptional than most. Foreign visitors from the beginning of the Republic—Tocqueville and many others—found the United States to be different from what they knew in Europe. And that perception of difference has not disappeared. What U.S. academic traveling abroad has not been asked by a bemused foreigner to "explain" the United States? At issue may be something specific, like Watergate or Whitewater; something endemic, like our world-class crime rate; or something systemic, like our peculiar approach to the welfare state. Some may argue that our country is not that unusual, but most of us, I bet, would accept the premise that the United States is exceptional.

It would be interesting to survey political scientists on the question. What basic features would we mention first that make us different from other nations? Are they institutional (like our special way of dispersing power), or historical (like the sequencing of party and bureaucracy formation), or cultural (like our strong commitment to individualism), or social structural (like our diversity in racial and ethnic terms), or economic (like our industrial growth rates)? Or all of the above? These are big questions; they need big answers.

This book takes on the big question. The opening sentence begins: "This book attempts to explain contemporary America. . . ." No one is better equipped to provide that explanation than Seymour Martin Lipset. In his long and distinguished career, he has been the leading analyst of American exceptionalism. The present book is his latest and most complete attempt to describe and explain what is special about the United States.

Lipset knows that exceptionalism is a term of comparison, and essays comparing the United States to Canada and to Japan put "American" in context. Lipset also knows that being different from other places does not imply the absence of internal differences, and the chapters on African Americans, on Jews, and on intellectuals, while fascinating in themselves, help to highlight the general theme of the book.

Lipset locates American exceptionalism in various features of our society and polity. Central to our exceptionalism are the special nature of the welfare state and the special nature of party politics: Weak state control over the economy, low welfare spending, the absence of a strong working-class movement or party, and weak labor unions are among its main features. In addition, Lipset stresses the constitutional separation of powers and the divided and weak government that derives from this, as well as such related features as the important role of law and the courts.

If there is a key to explaining these many features, Lipset finds it in culture—in the values and beliefs of Americans. This country is what it is because of a syndrome of popular beliefs: liberalism (in the European laissez-faire sense), egalitarianism (which in its U.S. meaning of equality of opportunity is not that far from liberalism), individualism (which goes along with the first two), populism (which goes along with antistatism), voluntarism, and moralism. These values sometimes clash one with another but tend to form a syndrome that dominates U.S. politics. And though there have been changes in recent times, Lipset finds much continuity. The book does not contain a tight causal argument; it is not clear whether these patterns of belief are what is being explained or what is being used to explain other phenomena—probably more the latter. But it is clear that public beliefs are the key to what makes the United States special.

These beliefs form the American Creed. The creed, in turn, is a double-edged sword, producing some of the best and worst features of U.S. society. "It fosters a high sense of personal responsibility, individual initiative, and voluntarism even as it encourages self-serving behavior, atomism, and a disregard for communal good" (p. 268). Lipset's analysis of

American exceptionalism would be a good way to answer those puzzled observers from abroad who are taken by the freedom, opportunity, and openness of U.S. society and appalled by the apparent lawlessness and indiscipline. The creed explains both.

Is the explanation satisfactory? It certainly is compelling and is backed up by a very wide range of survey data and examples. A big question needs a big answer, and Lipset provides one supported by a great deal of evidence. But big questions also defy precise and definitive responses. Skeptics may remain unconvinced.

For one thing, Lipset pays little attention to alternative explanations. It is not that his argument is monocausal. He cites institutional factors such as those embodied in the constitution and adds them to the argument. But he does not address the large body of work—by people such as Theda Skocpol and Sven Steinmo—that is critical of cultural explanations.

An additional and more basic question has to do with the nature of evidence and inference in cultural explanations. I happen to be one who believes, with Lipset, that such explanations can be valid and powerful. But they pose very difficult problems of causal inference. The evidence for a cultural pattern is often hard to come by, and it is even more difficult to deploy such evidence in a causal argument—to show, for instance, that patterns of beliefs and values explain the particular form the welfare state takes in America. Lipset's main approach is to amass a vast array of survey results showing both continuity in attitudes among Americans and systematic differences from other nations. The data presented rarely go beyond marginals. There is little or no analysis of the data—à la James Stimson or Page and Shapiro—to try to tease out systematically the nature of continuity or change. Nor is there much discussion of how a cultural pattern (as evidenced by a pattern of response to a set of survey questions) links forward to public policy.

Nevertheless, the sheer magnitude and range of the evidence Lipset brings to bear is compelling. The case for the cultural explanation is not proven—it is not the kind of case that can be proven—but the case has been made in a most compelling manner. No one looking at the mass of evidence presented can doubt that America is, indeed, exceptional in the beliefs and values of its citizens and that this is a major reason our political practices and public policies differ from those of other nations.

In this book, Lipset tops off a lifetime quest to explain the contemporary United States. And all of us who have to answer the questions of what we are and why we are that way are in his debt.

Intensive Care: How Congress Shapes Health Policy. Edited by Thomas E. Mann and Norman J. Ornstein. Washington, DC: American Enterprise Institute and The Brookings Institution, 1995. 316p. $38.95 cloth, $18.95 paper.

Mark Carl Rom, *University of California, Berkeley, and Georgetown University*

Intensive care. The image is of a critically ill patient—probably on a respirator—who needs constant and perhaps aggressive treatment from a staff of skilled professionals. Whether the patient lives or dies, and whether the patient is healed or remains incapacitated, depends as much and perhaps more on the condition of the patient as on the skills and diligence of the doctors. This picture of intensive care has two apparent meanings for this book, although neither is made explicit. The first shows Congress caring for health policy, and the second has the authors attempting to heal Congress. In both cases the critical questions are: What is wrong with the patient? How competent are the doctors?

This much we already know: Health care reform does not always check out of Congress alive. The largest policy initiative of this decade—President Clinton's national health care reform—never even made it to a floor vote in either house, despite the vigor it seemed to show when Clinton first proposed reform. In 1989 Congress also Kevorkianed the Medicare catastrophic coverage to which it had given birth just the year before. Julie Rovner, who covered both issues for *Congressional Quarterly*, deftly relates the congressional (mis)treatment of these issues in two fascinating chapters. Notably, the more successful cases of health care reform, such as Medicaid expansion and Medicare regulation during the 1980s, receive much less attention.

Could Congress have saved Clinton's national health care proposal? Perhaps so, but as Allen Schick and others remind us, its prognosis was never all that good (despite some rosy reports to the contrary); moreover, lots of interests inside Congress and out desperately wanted to kill it. Health care reform failed in 1994, Schick concludes (actually, begins), not primarily because of congressional structure or process, however important these factors were, but because "it did not have the votes—not in Congress and not in the country" (p. 227). In other words, even the most skillful congressional doctors were unlikely to save this health care patient. Moreover, Schick writes, and the other authors imply, the failure of the Clinton plan "confirms much of the conventional understanding of how Congress operates.... Once the external and internal conditions [for Congress] are reviewed, the failure of health care reform does not appear to be surprising or anomalous" (pp. 228–9).

If Congress's internal and external conditions confirm the conventional political wisdom, then it should not be surprising that the authors say little new about how Congress might be cured. (Indeed, Mann and Ornstein aspire only to make a "modest contribution" [p. 2] to congressional scholarship.) The inability to heal Congress does not have much to do with the skill of these doctors, however. One would be hard pressed to find a team of scholars—in addition to Schick and Mann and Ornstein, it includes Lawrence Evans, Joseph White, Mark Peterson, and Mark Nadel—that is better trained and more experienced in congressional operations. *Intensive Care* itself grows out of the larger Renewing Congress Project sponsored by the American Enterprise Institute and the Brookings Institution. The project has already issued three reports and an edited volume addressing a wide variety of congressional concerns: leadership, partisanship, committee structure, budget processes, campaign finance, ethics, and public relations, among others.

The authors show that—despite the title's implication—Congress does not have the kind of problems curable by intensive care. A more appropriate analogy perhaps is that Congress needs chronic care because it suffers from painful and debilitating conditions that cannot be cured, only relieved. That jurisdiction over health policy is fragmented across multiple committees complicates policymaking, but as Evans points out, it would be unwise and impractical to consolidate health policy into a single "megacommittee." Budget procedures, especially "paygo" rules and mandatory CBO scoring, White argues, create a strong bias against new government commitments (as they should, White continues). Congressional oversight is neither systematic nor coordinated, Nadel suggests, but it has nonetheless increasingly become integrated into policymaking. Peterson shows that "policy-analytic" knowledge was ultimately dominated by ordinary (e.g., anecdotes) and distributional knowledge in the battle over the Clinton plan; still, he argues that this is as it should be, as "choices about ideological and partisan orientations are largely what elections are all about" (p. 123).

In general, then, the authors provide two kinds of care. One is to offer minor relief to nagging congressional symptoms. The other is to argue that the symptoms are actually characteristics; that is, they are features of U.S. politics rather than problems to be fixed. They neglect, by omission, consideration of more radical treatments (except for Peterson, who rejects term limits as lethal to Congress's analytical capacities). Those who believe that Congress is gravely ill are unlikely to be persuaded by either approach. Those who specialize in congressional studies will probably find them sensible yet conventional. The rest of us, however, will find this a solidly researched and written book that provides an excellent overview of congressional treatment of an important policy issue.

A final note: The book proclaims that it is about health policy, but it really is about insurance, not health. Its two major issues, Medicare catastrophic coverage and the 1994 health care reforms, are almost exclusively about who will pay for what medical care. These are, of course, important issues. But the fact remains that even guaranteeing everyone health insurance would do relatively little to improve the health of the nation. The major killers in our society are tobacco, rich diets and poor fitness, and alcohol. The most important causes of death among the young are accidents, violence, and AIDS. The rates of low birthweight babies and infant mortality are a national disgrace. The insurance reforms considered in this book barely even touch on these critically important health issues. Perhaps we can look forward to the day when a president calls for a *health* policy that helps Americans to be born healthy, grow strong, and live long. Maybe then scholars as talented as these can surprise us by showing how Congress made this possible.

Children in Court: Public Policymaking and Federal Court Decisions. By Susan Gluck Mezey. Albany: State University of New York Press, 1996. 212p. $59.50 cloth, $19.95 paper.

Susan E. Lawrence, *Rutgers University, New Brunswick*

Tuning in to the 1996 Republican and Democratic national conventions after a hiatus from national politics, one gets the impression that the American family has replaced the Red Menace as the number one public policy concern of the national government. And it strikes one as somewhat peculiar that so much campaign rhetoric is devoted to issues affecting the single largest group of nonfranchised U.S. citizens: our children. Equally odd is the fact that both parties have put what was once the nadir of the slippery slope of federal encroachment on state government power, the private domain of the family, at center stage in their campaigns for national office.

Of course, on the campaign trail, children are a lot like apple pie. It is hard to be against either one. But behind the prime-time campaign rhetoric of the national party conventions lies a mass of competing policy proposals that would have vastly different effects on children's lives if enacted. And children are shut out of the democratic electoral process that will determine which policies will be adopted. Will the federal courts, the presumed forum for the participation of the politically disadvantaged, represent and protect the interests of children—interests which may be in competition with those of parents and the state? As the locus of decision making for children's policy has moved from the state and local level to the national level over the last 35 years, how has federal court litigation shaped the contours of that policy? These are the questions Susan Gluck Mezey sets out to address in *Children in Court.*

The book presents a systematic description of federal court decisions in cases related to the legal, political, economic, and social status of children in the last half of the twentieth century. Including both statutory and constitutional claims, Mezey tells us that the book "is based on federal court decisions identified by computer searches in five major areas of federal policy making for children: public welfare assistance, preschool education, early childhood nutrition, child welfare, and child support" (p. 9). Mezey confines her analysis primarily to Supreme Court opinions, including all decisions "directly or indirectly involving adjudication of children's rights or interests in these five policy areas," turning to lower court decisions only "when there are no Supreme Court opinions" (p. 10). While Mezey gives us a complete listing in the appendixes of the 53 constitutional cases identified by her methodology, she does not provide us with such descriptive data on the statutory cases she includes.

Mezey arranges her analysis topically, with chapters on "Constitutional Rights Litigation," "Aid to Families with Dependent Children," "The Women, Infants, and Children Program and Head Start," "The Child Welfare System," and "Child Support Enforcement." Each begins with a summary history of federal government policy and then turns to federal court litigation. The book provides a concise primer on federal children's policy and is an important corrective to studies that ignore the role of the federal courts in the U.S. policy process. It would serve as a useful addition to any public policy course, particularly those that address entitlement programs and children's policy.

As Mezey tells us in the introduction, "this study focuses on the *outcome* of lawsuits affecting the status of children by examining the opinions of the courts" (p. 3), and this is precisely what she does. Her descriptions of the decisions are unadorned by discussions of how these controversies arrived in the courts. The questions of who claims to represent "children's interests" and how they define those interests are left open. Nor does Mezey move beyond the court decisions themselves to assess their direct or indirect policy effect, although it is clear that many of these decisions have played a key role in shifting the balance of power in various entitlement programs from the states to the national level and have introduced the symbolically powerful language of "rights" into the children's policy debate. (See R. Shep Melnick, *Between the Lines* [1994] and Michael W. McCann, *Rights at Work* [1994]).

What Mezey's analysis does reveal is that, in recent years, the Supreme Court's concern with federalism and state autonomy has permeated its decisions on children's rights and interests, leading to a refusal to constitutionalize a state obligation to protect children from physical and emotional harm or to require a more equitable distribution of those government resources and services that affect children, while generally upholding state court child support orders (pp. 172–3). Across children's policy issues, and particularly in respect to AFDC policy, "contrary to the tenets of the '[political] disadvantage theory,' the Court did not play a significant role in contravening the actions of the majoritarian branches of government" (p. 172). Rather, Mezey concludes that the three branches seem to work in harmonious confluence. Hence, the nation's largest, and most vulnerable, nonfranchised group—children—remains dependent on the good graces of those electoral processes that sing their name this election year. Be they lullabies or siren songs, Mezey's analysis suggests that the federal courts will simply join the chorus.

Public Lands and Private Rights: The Failure of Scientific Management. By Robert H. Nelson. Lanham, MD: Rowman & Littlefield Publishers, Inc., 1995. 373p. $61.50 cloth, $24.95 paper.

Paul J. Culhane, *Northern Illinois University*

Two federal public lands agencies, the Forest Service and Bureau of Land Management (BLM), are the principal targets of Robert Nelson's book. These agencies manage 41% of the land in the eleven states west of the 100th meridian, plus much of Alaska; the Forest Service also manages substantial land in the Appalachians, Ozarks, upper Midwest, and rural Northeast. Probably more important, the service has historically been the leading organization among the federal natural resources agencies.

As the book's subtitle suggests, the Forest Service's historical origins are the root of what Nelson sees as the failures of public land management. Gifford Pinchot, the service's founder, was a leading figure among the Progressive Republicans of the Theodore Roosevelt administration. Nelson asserts that the Forest Service's current operations still reflect its founding during this "scientific management" era. Both the political rhetoric of the Progressive era generally and Pinchot's founding of the Forest Service specifically were marked by an opposition to illegitimate expropriation of public resources by "special interests." Nelson argues that, ironically, "scientific management" by the Forest Service and BLM has shifted over the century into a system in which use of public lands is heavily influenced by interest group pressures.

As a result, Nelson sees this management as terribly inefficient. Forest Service timber management costs more than the federal government receives from timber sales. The BLM's range management costs more than the full economic value of public domain livestock grazing and five times the amount of the grazing fees BLM takes in; even minerals management is a loser, largely because the Congress gives such a high percentage of mineral receipts back to the states. As a solution to this bureaucracy problem, Nelson advocates a combination of giving some rangelands to the western states, deeding high-value timberlands to publicly owned timber corporations, leasing other rangelands to ranchers on a long-term basis (i.e., 50 years), and giving mineral lands to the states. In the penultimate chapter, he even proposes breaking up the Department of the Interior, including giving most of the National Park system to the states, local governments, historical societies, and other nonprofit groups.

The question is, what is *new* in this argument? Many of Nelson's prescriptions echo the calls of the "Sagebrush rebels" and privatization zealots during the first Reagan administration and Jim Watt's stint as secretary of the interior. Nelson served from 1975 to 1993 as a staff economist in the Office of Policy Analysis, Department of the Interior, which is BLM's home department, and he has been a well-known member of what was called in the 1980s the "new resource economics" community. This book is a collection of his writings from 1979 to 1989. In other words, most of the ideas in the book date from the Reagan-Watt regime. But from its very cover the book gives readers fair warning. In a dust-jacket quote, Marion Clawson, the dean of public lands scholars, tells us: "Whether one accepts or rejects Nelson's analysis and his conclusions, no one interested in public land management can afford to ignore this book." And Sally Fairfax's Foreword warns us: "Nobody will agree with all of Nelson's conclusions and recommendations.... But no one is likely to come away from ... these rich morsels with their own assumptions and biases about public resources completely unrevised" (p. ix).

There are, indeed, some interesting morsels in this collection. For example, Nelson notes how the states receive an extraordinary share of federal public land receipts, from 50% of mineral royalties to 25% of most Forest Service receipts; how the federal government receives no revenue from hunting and fishing on federal lands, largely because the states monopolize hunting and fishing license fees; and how state and local governments have greater formal and informal influence than other participants in public land decision making. As Nelson puts it, "if property is really a bundle of rights, many of the public land 'sticks' are already in state possession" (p. 240). So, Nelson argues, it is fine to give the states the rest of the property rights to federal lands, together with the management costs and responsibility. An interesting "morsel" indeed—but why not just as logically conclude that the states' extraordinary share of public land revenue and policy influence ought to be scaled back significantly? Of course, my conclusion is politically unfeasible, but it is probably less politically irrational than Nelson's proposed wholesale give-away of the National Park system.

A true book requires more than a few good morsels, however. The essential problem with this work is that it is a collection of inadequately revised pieces. Nine of the sixteen chapters in the book were previously published, several in good refereed journals or law reviews, though a few chapters were originally unpublished Department of the Interior policy memorandas. The original papers were written from 1979 to 1989, with the average chapter dating to 1985, and much of the material is obviously dated. Perhaps the most disappointing chapter in the collection (originally a 1980 departmental memorandum) deals with land use planning. I cannot disagree with Nelson's forecasts, as Paul Friesema and I made many of the same points in a 1979 article, as did others at the time. But a 1995 book should present some empirical evidence about the past decade's experience with, especially, the Forest Service's complex and difficult land and resource management planning process.

More important, throughout the book, Nelson repeats the charge that the misperformance of the Forest Service and BLM is due to the "failure of scientific management," but he never develops this charge into a coherent argument. Sometimes he associates scientific management with rational-comprehensive decision making, other times with the Wilson-Goodnow era of civil service reform, other times with Samuel Hays's argument that the Progressive conservation movement believed in management by public agencies staffed by "scientific experts," and still other times with Grant McConnell's argument that the Progressive movement was organized around rhetoric against "special interests." Yet, readers familiar with public administration and organization theory tend to associate scientific management with Frederick Taylor's time-and-motion-study task management and centrally planned and controlled factory system. The problem is that the modern Forest Service—as described, for example, in Herbert Kaufman's 1960 classic, *The Forest Ranger*—is a very highly professionalized agency with a very strong *esprit de corps*, not a Taylorist steel mill. (Similarly, the BLM is composed of professionals, though with a much higher proportion of range managers, whose scientific base Nelson questions, and without nearly as strong organizational *esprit* as the Forest Service.) So, Nelson's explanation by reference to historical roots seems very superficial; he just does not confront the full scope of the Forest Service's position astride its policy system.

Most important, because *Public Lands and Private Rights* is a collection, its contents are terribly repetitive. Most writers' thoughts on a subject do not change radically over time. But Nelson has taken a dozen pieces that make essentially the same argument in different venues, stacked them into four parts with an introduction and conclusion, and marketed it as a book. In chapter after chapter, we find a couple of interesting morsels buried in the same background description, the same mantra about the shortcomings of federal management, the same prescriptions to give the public lands to the states and industry, over and over again, ad nauseam. Instead of such repetition, Nelson could have written a thorough chapter on the case for transferring certain lands to the states, another thorough chapter on giving other lands to ranchers, a well-integrated chapter on mineral rights, and so forth. One should expect a self-respecting publisher who submits a manuscript to a legitimate review process to demand a work that is much better organized. In that case, the book might have offered readers a coherent argument in support of this noteworthy author's advocacy of a "property rights" destruction of our nation's system of public land management.

Classifying by Race. Edited by Paul E. Peterson. Princeton: Princeton University Press, 1995. 422p. $59.50 cloth, $19.95 paper.

Charles S. Bullock III, *University of Georgia*

In 1909 the Harvard Classics, a "five-foot shelf of books," was published. Classifying by Race might well be considered the Harvard book on race. Although published by Princeton University Press, the editor and most of the contributors have a current or past affiliation with Harvard. The research included here was originally presented over a two-year span at the Workshop on Race, Ethnicity, Governance, and Participation at Harvard's Center for American Studies.

The contributions vary in content and approach, although most of the articles explore a facet of elections. The approaches range from Lani Guinier's legal analysis in a piece initially published in the *Cardozo Law Review* (one of only two selections previously published) to hypothesis testing by David Metz and Katherine Tate, who examine the racial strategies of black and white mayoral candidates in major cities, to a contribution by Frederick Harris, who seeks to explain church-based political activism of Chicago survey respondents. Among the more descriptive efforts are the examination of Puerto Rican support for such values as economic individualism and political tolerance by Rodolfo de la Garza. A more historical approach is represented by Theda Skocpol, who examines the relationship between race and stigmatization of welfare beneficiaries from the Civil War to the present. Richard Valelly also focuses on the past as he explains turn-of-the-century minority disenfranchisement in terms of political parties. Once the GOP could muster a reliable majority in Congress by relying on newly mobilized votes in the North and West, it was no longer willing to confront adamant southern Democrats bent on white supremacy. Kenneth Benoit and Kenneth Shepsle turn to formal electoral theory in comparing the likelihood of electing African Americans from plurality, single-member districts versus alternative electoral structures.

Most of the articles focus on the U.S. experience, but a few introduce a comparative perspective. Benoit and Shepsle's examination of electoral systems includes information on the seat/votes relationship of European Green parties. Margaret Weir compares the increasing isolation of poor African Americans with the less exclusionary welfare approaches of Britain and France.

Notwithstanding the varied topics included, the primary concern of the Harvard effort is voting and office holding by African Americans. During much of the current decade, redistricting has been the most controversial facet of minority political efforts, and several contributors address the topic. David Lublin considers the partisan consequences of congressional plans for southern states and calculates the number of districts that racial gerrymandering cost Democrats in the 1992 and 1994 elections. He also considers the possibility that increasing the number of black legislators has reduced overall responsiveness to concerns of African Americans by reducing the number of white legislators who have a constituency that demands such attention.

Gary King, John Bruce, and Andrew Gelman seek a standard for measuring racial fairness in districting. In the early 1990s it often seemed as if civil rights leaders and Department of Justice attorneys sought at least proportional representation or even to maximize the number of districts likely to elect minorities. After noting that a single-member district approach is unlikely to give blacks a proportionate share of the seats, King and his colleagues reject proportionality as an appropriate standard because African American candidates tend to be more liberal than whites. Once black liberalism is factored in, the authors conclude that "black citizens are now fairly represented in the U.S. House of Representatives, since they have only one fewer member of Congress than our standard indicates" (p. 107). Benoit and Shepsle have no fairness benchmark but conclude that alternative voting systems, such as limited or cumulative voting, are likely to increase the number of minority legislators. They offer quantitative support for a position embraced by Guinier, who urges alternative electoral systems as an antidote for black underrepresentation.

The number of minorities in public office is not the only dependent variable of interest. Turnout and margin of victory are the concerns of an article co-authored by Lublin and Katherine Tate, who use data from mayoral contests in 26 cities over 22 years. Racial competition spurs participation and is associated with more hotly contested results. Registration, another measure of political participation, draws the interest of James Alt. Using data from the handful of states that maintain voter registration figures by race, Alt shows a symbiotic relationship between registration increases for the two races. Trends observed by Alt lead him to conclude that racial disparities in registration may be eliminated by the turn of the next century.

Metz and Tate use a data set similar to that analyzed by Lublin and Tate to study whether mayoral candidates make racial appeals. Black racial appeals are related to efforts to topple white incumbents, while whites make more racial appeals in close general elections. Race plays a greater role when black candidates believe they have reasonable prospects for winning the office. There is no evidence that white candidates use race when trying to displace a black incumbent.

The breadth of *Classifying by Race* and the skill and insights of the contributors will earn this volume a wide audience. It undertakes a broader set of questions than were covered by another recent edited volume from Princeton, *Quiet Revolution in the South*, and consequently will attract more than just those interested in rates of office holding among African Americans.

While *Classifying by Race* presents some of the most current research on race, the courts are outpacing both scholars and publishers. The *Hopwood* decision invalidating many affirmative-action efforts by colleges and universities has been decided since this volume was published. When the book was in production, the Supreme Court's *Miller* v. *Johnson* struck down Georgia's "Sherman District" (a congressional district that extended from Atlanta to the sea) for having been drawn primarily on the basis of race. The 1995–96 session of the Court invalidated congressional districts in two other states. *Shaw* v. *Reno* (1993), which opened the way for *Miller*, gets surprisingly little attention. Volume editor Paul Peterson, however, does a remarkable job of anticipating the *Miller* majority. Using language that the Court might have adopted, Peterson writes: "But making racial considerations the one and only factor to be considered in drawing election boundaries elevates race to the center stage of American politics. . . . The more obviously governmental actions are motivated by race, the more government intensifies the racial divisions in our society" (p. 16).

God at the Grassroots: The Christian Right in the 1994 Elections. Edited by Mark J. Rozell and Clyde Wilcox. Lanham, MD: Rowman and Littlefield, 1995. 274p. $22.95.

Lyman A. Kellstedt, *Wheaton College*

God at the Grassroots demonstrates once again the decentralized nature and the great diversity of the U.S. political system. This book shows that the Christian Right was active in the 1994 congressional elections and that its role and influence varied from state to state. This should come as no surprise; political parties, interest group configurations, and state political cultures vary from one state to another. This book examines the role of the Christian Right in eleven states: Florida, Georgia, Texas, Oklahoma, Virginia, and South Carolina in the South; Michigan, Minnesota, and Iowa in the Midwest; and California and Oregon on the West Coast. The well-written but highly diverse chapters will assist in understanding the important role played by Christian conservatives in the 1994 campaign. Conspicuous in their absence, however, are states from the Northeast and the Mountain region, or such midwestern electoral powerhouses as Illinois and Ohio. This is because the criterion for inclusion in this volume were states in which the Christian Right had a "dominant" influence in the state's Republican Party (according to a survey conducted by the magazine *Campaigns and Elections*). Hindsight suggests that including a few states with an insignificant Christian Right presence might have increased our understanding of the factors that mitigate or promote its influence. This means there is an even larger story to tell—about the Christian Right in states not included in this book.

The illuminating introduction by John C. Green is must reading for all political scientists concerned with understanding the Christian Right and its potential. Green debunks such labels as the "religious right" (too broad a term, the movement is mainly among evangelicals) and "pro-family" (too narrow, the agenda encompasses more than family issues) before settling on "Christian Right." "Evangelical Christian Right" might be an even more appropriate term to describe the activist and mass base components of the current movement. This terminological point is important, for the influence of the movement will be limited if it remains almost exclusively evangelical but will have much greater effect if it can be extended significantly to conservative mainline Protestants, traditional Roman Catholics, orthodox Jews, and Mormons. Green makes an even more important point by claiming that discussions of the Christian Right fail to distinguish among its three components: leadership, activists, and mass base. Despite all the publicity surrounding the movement, we know very little about its activist organizational cadres and mass base, which makes assessment of it difficult. For many evangelical Protestants, leading Christian Right organizations such as the Christian Coalition, Focus on the Family, and Concerned Women for America serve as classic intermediaries in the two-step flow of communications once performed by local party organizations. How well the Christian Right organizations mobilize and train a strong activist component and how receptive the potential mass base is to these activists will be key in determining the influence of the movement in electoral politics.

As for the eleven case studies, they are extremely diverse, using widely varying source materials and employing widely varying methods. Some rely on local newspapers, others on careful observation. Each provides insight into the politics of that state, and this reader found them uniformly interesting. Although the Christian Right had infiltrated the Republican Party in all eleven states covered in the book, their influence in the 1994 election varied considerably. It was powerful in Oklahoma and South Carolina, for example, and far less significant in Virginia and California. The case studies tend to fall back on idiosyncratic explanations and local considerations (the weaknesses of such candidates as Oliver North in Virginia and Allen Quist in Minnesota, the strength of such candidates as David Beasley in South Carolina, and the remarkable set of circumstances that came together in Oklahoma in 1994 and dramatically transformed a whole congressional delegation). What is lacking, for the most part, is an in-depth focus on the leadership of the Christian Right in each state, an assessment of the size and performance of the activist component of the movement, and a description of the size and electoral involvement of its mass base. In other words, the very things that Green suggests in his introduction should be the foci of attention to avoid confusion concerning the Christian Right are the missing ingredients in the case studies.

The short final chapter by the editors tries to tie the cases together to reach some general conclusions about the Christian Right and to look ahead to its potential role in the 1996 campaign. Ultimately, however, the conclusions cannot make up for what is missing in the cases, which do a better job in their focus on leadership than they do on activist followers and the mass base. Yet, the failure of the case studies to explore these crucial components of the Christian Right is easy to understand, for research time and costs to conduct the kind of work needed are difficult to come by. Answers to the questions posed above about the activists and mass base, however, are what future research must provide. The case studies in this collection take us a step in the right direction, however, by demonstrating the diversity of Christian Right influence across the country and by suggesting directions for future research. This is an important contribution, for the Christian Right should be an important force in U.S. politics for some time to come.

The Ironies of Affirmative Action: Politics, Culture and Justice in America. By John David Skrentny. Chicago: The University of Chicago Press, 1996. 326p. $48.00 cloth, $16.95 paper.

Kate Greene, *University of Southern Mississippi*

The Ironies of Affirmative Action is not just another polemic in the thirty-year affirmative action debate. Instead, it is fine scholarship which offers its readers a rich, nonideological cultural and political analysis of social policy. As promised by the author, this book does not take sides in the debate, and anyone hoping to find arguments for or against affirmative action will be disappointed. There is a much different value in this book, for it provides a new understanding of the history and politics of affirmative action and an interesting theoretical framework for analyzing and understanding other controversial social policies.

Skrentny explores two ironies. The first centers on the fact that when Congress moved to address the problem of employment discrimination against blacks, instead of using race *for* blacks instead of *against* them, the response was a call for color-blindness. According to Skrentny, the irony is that our society is full of exemptions to the difference-blindness rule in general (senior citizen discounts) and in employment (veteran preferences, nepotism), but when dealing with the question of race, preferences are absolutely taboo. The other irony is that affirmative action, which was unpalatable and out of bounds in 1964, soon became policy, and it did so without any organized lobbying for the policy and with a significant amount of public opposition.

Skrentny rejects the interest group framework for analyzing affirmative action, opting instead for a cultural interpretation of the politics, policy, and law of affirmative action. This cultural framework centers on the boundaries of legitimate action and discourse, the nature of the actors' interests, the moral expectations of politics, and the relevance of particular audiences; in his words, what the actors take for granted. Skrentny notes that each of these are socially constructed, and thus the rules of the game resulting from the interaction of these factors change over time.

Skrentny begins by examining the resistance to affirmative action. The color-blind employment model, which stands in contrast to affirmative action, is derived from the discourse of universal and abstract individualism of modern institutions, that is, the Lockean tradition. In the United States, as a society we believe there is a natural order and justice that will be achieved by giving people the freedom to pursue their own ends. Only a meritocratic, difference-blind employment model comports with our sense of justice. A color-blind employment model is thus consistent with our cultural and moral beliefs regarding justice and therefore is within the boundaries of legitimate political action and discourse. Yet, Skrentny notes and examines the fact that not all nonmeritocratic preferences in employment are rejected or considered unjust in our social policy—in particular, veteran's preferences and nepotism. The question for him is why we so unabashedly accept some preferences yet reject affirmative action. His answer is that, for affirmative action, it is not a matter of the strength of lobbying groups or voter strength but of a culturally determined question, the question of moral worthiness. That is, we do not view blackness as worthy of preference.

This is the one place where Skrentny makes an effort to be diplomatic when he should simply speak clearly and loudly. Attempting to lessen the blow of his point by speaking of "complexity" and "historically contingent meanings," Skrentny offers such observations as: "Race has been *special* in the model in that it was a legitimate basis of legal exclusion, but it is also *special* in that preference is expressly not considered just" (p. 62, emphasis supplied) and "Throughout American history some groups have been constructed as morally worthy, and other have not" (p. 63). Finally, however, he tells us what he means and what many of us do not want to face: "Americans who resist affirmative action are simply articulating the American model of justice as it relates to race and employment preference. Affirmative action is objected to because of its racial beneficiary" (p. 63). Still, what Skrentny shows with this point, whether he means to or not, is that there is no irony after all. As his framework would predict, within the boundaries of legitimate action and the moral expectation of politics in a racist society, it is not ironic that we accept preferences for veterans or family members but not for blacks. It is the logical progression of our racism.

The analysis of the second irony—that racial preferences progressed from being taboo in 1964 to being accepted within a few years—takes up more than half the text in four chapters and is the book's primary contribution. According to Skrentny, affirmative action became possible because the boundaries that made it unacceptable became blurred, and the context of certain political actors and actions made it seem "sensible and appropriate" (p. 223). One factor that made affirmative action possible was the crisis caused by urban riots. As the Cold War world watched human rights with a critical eye, jobs for blacks seemed the best nonrepressive response to the violence. In addition, administrators working with equal employment law found it was difficult to show results under the color-blind model, and administrative pragmatism led them to the results-oriented affirmative action as a tool for showing agency success. Thus, administrative pragmatism and the crisis of black rioting increased support for affirmative action with the political and business elites.

With these changes, the advocates of civil rights developed a discourse which suggested that affirmative action was consistent with the tradition of color-blindness and was nothing new. That is, color-blindness should eliminate the economic statistical disparities between blacks and whites. Affirmative action was intended to achieve this goal. This allowed civil rights advocates to attack opponents of affirmative action as the opponents of tradition. Also, Richard Nixon established affirmative action as an official policy because it allowed him to preempt the liberal agenda by splitting labor and the civil rights activists over the Philadelphia Plan. The Supreme Court, using the language of precedent in the same way the civil rights advocates used the discourse of tradition, weakly endorsed affirmative action as constitutional, and the policy became more acceptable, at least until the recent attack on it.

This brief summary does not do justice to Skrentny's argument, which is a solid explanation of how such an unpopular policy could become acceptable, though still controversial. Unlike most ideological tracts on affirmative action, this book puts some social, moral, political, and administrative context around the policy, and, like the first irony, the second irony disappears. Affirmative action is the incremental but logical progression of crisis management, administrative pragmatism, a discourse of tradition, and preemptive politics.

Skrentny refuses to rely solely on institutional approaches for explaining change because it cannot explain the institutionalization of affirmative action. He does not reject the institutional approach, however. Instead, he argues that it must recognize boundaries, discourses, audience, and the expectations of that audience, that is, the cultural and moral context. That is the contribution of this book to the affirmative action debate and social science in general. Skrentny asks us as political scientists to move beyond institutions and political actors and to jump into the *moral and cultural* fray when seeking explanations of political and policy change. Whether dealing with affirmative action, abortion, or lesbian and gay rights, the moral and cultural analysis of our discourses/debates has much explanatory power that should not be ignored. Yet, in the world of Ralph Reed, Bill Bennett, and "culture wars," we should be aware of the difference between using the moral and cultural context to explain public policy and using it to justify public policy.

Anyone with even the slightest interest in affirmative action should read this book. It is most worthwhile for the history it provides, but the analysis of that history suggests a new route for those studying other controversial social policies, such as abortion rights and welfare. Perhaps the best reason to read this book, however, is that it just may encourage all of us—scholars, politicians, and common women and men—to confront the moral and cultural principles that we take for granted and that shape and define what is and is not possible in our public policy.

State Trust Lands: History, Management, and Sustainable Use. By Jon A. Souder and Sally K. Fairfax. Lawrence: University of Kansas Press, 1996. 370p. $40.00.

Charles Davis, *Colorado State University*

Jon Souder and Sally Fairfax analyze a substantive policy area that has received surprisingly little attention from students of U.S. state politics or public lands policy. School grant lands were originally ceded to newly emerging states by the U.S. government to support public education. Currently, 22 states (mostly in the West) maintain and administer school or trust lands. Nationwide, these lands amount to approximately 132 million acres, more than the holdings of the National Park Service, and their economic and ecological value is substantial. State permanent trust funds currently hold $27 billion obtained from resource development activities, and administrators disperse $3 billion annually to beneficiaries. Moreover, these lands have many of the same ecological characteristics as adjacent national forests or rangelands.

The stated objectives of this study are to increase the visibility of state trust lands as a topic of scholarly inquiry and, more important, to analyze policy and management techniques in order to design more effective resource management programs in general. The authors emphasize the importance of the trust doctrine as a guide to good decisions; that is, the granted lands in combination with the revenues and permanent funds they produce create a legal obligation for trust managers "to approach their management responsibilities under the same array of rules and enforcement mechanisms that surround any trustee, such as a banker managing funds for a client's grandchild." Consequently, the trust approach produces management decisions that are based on four principles: clarity of trust goals, administrative accountability, enforceability, and perpetuity. Unlike the multiple-use framework that allows federal agency managers considerable discretion in making resource allocation decisions that may be self-serving, state trust decisions are explicitly aimed at producing long-term economic benefits.

To assess whether trust administrators are in fact making decisions that benefit schools rather than the agency or politically powerful lessees, such as timber companies or realtors, Souder and Fairfax discuss a number of analytic tools designed to evaluate performance. Attention is directed at specific indicators, such as revenues received per acre leased for a specific use and revenues received per full-time employee in each program. They also discuss in considerable detail applications of portfolio theory to the management of state permanent funds. This approach helps managers assess the risk associated with varying levels of resource development activities to ensure that current uses do not foreclose future and potentially more valuable uses of the land.

The authors then spend several chapters on an analysis of leasing arrangements linked to traditional commodity programs managed by the state, that is, cropland agriculture, livestock grazing, hardrock mining, and energy resources and timber harvesting. State management practices are scrutinized to produce answers to the following questions. To what extent do institutional arrangements for investment decisions and the management of natural resources vary across states? Are structural differences related to program efficiency? How do trust managers handle tension that inevitably arises between themselves and the lessee, between the need to serve the intended beneficiaries (the schools) and general public preferences for alternative land uses, and between current and future management priorities? How are these approaches different from the federal model presented in an array of federal land laws? One of the more interesting conclusions is that state agencies are better able to make decisions for beneficiaries despite opposition from commodity groups and their allies in the state legislature, largely because the clarity of the trust mandate has been consistently upheld by state and federal courts.

Next, Souder and Fairfax apply their framework to emerging management problems, such as compensation for recreation and hunting programs, determining how and when to develop properties located near rapidly growing population centers, and the control of water resources within trust lands. They conclude with a number of policy prescriptions designed to promote resource management decisions that are ecologically sustainable over time as well as efficient in generating income for trust beneficiaries. Perhaps the most critical recommendation is to consolidate responsibilities for managing the permanent fund and the allocation of trust resources within the same organization. Overall, the authors find that the trust approach has much to offer as a model for public resources administration that cannot be achieved under the multiple-use or land withdrawal approaches currently employed by federal land management agencies.

State Trust Lands is best described as applied policy analysis. The authors are less interested in developing a political explanation for between-state differences in land-use decisions than in providing an analytic framework that can be used by citizens and scholars alike to challenge prospective courses of action that are inefficient or short sighted. While they acknowledge that trust officials are not immune to the exercize of influence by traditional land-use constituencies, the relative emphasis on prescription is achieved at the expense of a more measured appreciation for political barriers to the attainment of desired policy reforms. But Souder and Fairfax have largely succeeded in their efforts to elevate the discussion of state trust lands to a more prominent position within the arenas of state politics and public lands policy. They also provide a wealth of data on state programs that will be quite useful for environmental or public land scholars wishing to explore substantive policy areas in greater detail. Others who will benefit from a careful reading of this book include students of policy design and regulation as well as policymakers at both the state and federal level of government.

The Theory and Practice of Political Communication Research. Edited by Mary E. Stuckey. Albany: State University of New York Press, 1996. 236p. $59.50 cloth, $19.95 paper.

Doris A. Graber, *University of Illinois at Chicago*

The moral of Mary Stuckey's collection of eleven disparate political communication essays is that there is more than one good way to skin the political communication research cat but that the mode of skinning matters. Different features of political language emerge depending on the theories that guide the investigation. The title of the book is a bit misleading because it promises more than is actually delivered. The collection encompasses neither a grand theory of political communication research nor a full array of the many forms that research has taken. Rather, the articles cover, in more or less detail, a limited array of important current theories and practices, drawn mostly from the realm of political rhetoric and linguistics. The focus is on the interface between the construction and interpretation of political messages and their effect on the conduct of politics.

The book begins with a densely written essay by political scientist David J. Lorenzo, who explains how literary theory and insights drawn from cultural anthropology can shed light on the motivations behind politicians' utterances. He thus reveals the systematic connection between the broader political culture and the rhetoric of political leaders. After reviewing the salient literature and models, Lorenzo demonstrates how analysis based on literary theory and cultural concerns can proceed. He uses as an example a speech by Ronald Reagan, who was then supporting the presidential candidacy of Barry Goldwater.

A brief survey of assorted verbal analysis theories follows. Speech communication expert Kenneth Hacker romps through various intellectual thickets, such as discourse processing, political and cultural linguistics, and semiotic analysis. He points out that language is an important instrument of control for the powerful that the powerless need to master. Students of the work of Murray Edelman will not be surprised. Somewhat later in the volume, Henry Kenski, whose credentials encompass both political science and speech communication, closes the methodological circle by recommending a multimethod approach to political communication research. He illustrates his recommendations with examples drawn from the study of policy issues using various perspectives to elucidate message framing and effect. The chapter contains explicit directions for a very useful content analysis design. If other scholars adopt it, it could bring some much-needed uniformity to the study of policy issue framing. The possibilities for comparative analyses would then be vastly enhanced.

Several essays focus on the particular verbal images that are created in various political contexts. Among my favorites is Bethami Dobkin's analysis of the clashing interpretations given to the widely publicized videotapes of the beating of Rodney King by Los Angeles police officers. Dobkin, a speech communication expert, explains why the framing given to the visuals by the news media aroused such widespread revulsion in most Americans, who interpreted the story as an inexcusable example of police brutality committed by white officers against a helpless black victim. This analysis is followed by an insightful account of the masterful verbal framing constructed by the police officers' legal defense team to structure the situation so that it convinced a jury the beating was justified and conformed to appropriate police procedures.

Another favorite essay is political scientist G.R. Boynton's account of the framing of the story of civil strife in Cambodia from November 1990 to September 1993. Boynton shows how the choice of various metaphors shaped the meaning of the story in the Senate Foreign Relations Committee and in *Washington Post* news accounts. Much of the history of the conflict was reported in the news stories, but in scattershot fashion, which made it difficult for average readers to piece the historical context together. While Boynton's account seems close to reality, an analysis by political scientists Francis Beer and Barry Balleck of the images used in the Gulf War debate seems farfetched. Beer and Balleck report on body metaphors contained in the Gulf War discussions. Such bodily concepts as balance or attraction, they contend, can be linked to such political notions as balance of power and alliances.

I was also intrigued by a down-to-earth account by political scientist Matthew Kerbel, who told of the trials and tribulations researchers face when they try to study how members of the press go about their business of producing news stories. It seems strange that professionals whose occupation calls for questioning people and intruding into their personal and professional lives are so reluctant to answer questions themselves or allow outsiders to observe their work. Given this Berlin Wall mentality, it is refreshing to receive pointers from Kerbel on how the wall might be scaled. Similarly, it is gratifying to learn from Roy Schwartzman's report about the usefulness of hermeneutics and critical theory to break through the mental barriers created by hate speeches—in his case Nazi racial rhetoric. Besides revealing the flaws of such rhetoric, the techniques can be used to prepare countervailing arguments.

Other essays in the book include a study of variations in the amount of time granted to candidates in presidential debates. Communication scholars Ronald Milafski and Jian-Hua Zhua report that various debate formats lead to substantial inequalities which may have significant political consequences. Two essays cover presidential rhetoric. The more context-oriented one, by political scientist Mary Stuckey, examines how rhetoric is influenced by the institutional characteristics of the presidency. She uses a model constructed from the work of Stephen Skovronek and Lloyd Bitzer to examine inaugural addresses. The other essay, by political theorist Andrew Valls, applies a social constructionist approach to political rhetoric. Valls reports on Ronald Reagan's depiction of African Americans and George Bush's description of the Gulf War to illustrate how language constructs reality, rather than merely reflecting it. While I found these contributions less interesting and useful than several essays mentioned earlier, this is undoubtedly a matter of personal preference. Any collection of essays is likely to evoke different reactions from various readers. All in all, editor Mary Stuckey deserves plaudits for assembling a diverse series of stimulating political communication studies that touch on numerous interesting issues and demonstrate the utility of various research traditions. Although the essays do not break much new ground, they widen existing paths and open up fresh vistas on the research landscape. That is a worthwhile contribution.

Constitutional Politics in the States: Contemporary Controversies and Historical Patterns. Edited by G. Alan Tarr. Westport, CT: Greenwood Press, 1996. 223p. $59.95.

Bradley C. Canon, *University of Kentucky*

Few people are interested in what state constitutions say, how they are amended, or how state supreme courts interpret them. This includes political scientists. Although my research focuses on the U.S. Supreme Court and I teach courses in constitutional law and civil liberties, I barely have a clue as to what my state constitution provides. I will bet that most persons who read this book review are in the same foggy state of mind about their own state constitution's provisions or state judicial decisions interpreting them.

Nonetheless, state constitutional politics (amending and interpreting the document) have received increasing interest in the past 25 years. One reason is the so-called New Judicial Federalism. As the Supreme Court lost its liberal majority in the Burger years and began holding the line or even retrenching on defendant's rights, state high courts with liberal majorities began interpreting their own constitutions to develop rights beyond those the Supreme Court found in the U.S. Constitution. Frequent reliance on state constitutions in some states to expand such rights led to serious law and order backlashes, most notably in California and Florida. California's chief justice, Rose Bird, suffered the electoral consequences, and Florida's chief justice, Ellen Barkett, almost did.

But more than criminal justice rulings have called attention to state constitutional politics. Since the 1970s almost half the state supreme courts have considered challenges to reliance on local property taxes as the primary funding source for the public schools. A dozen of them have ruled that the state constitution requires a major restructuring of school financing. (In 1972 the Supreme Court rejected such a challenge under the 14th Amendment's equal protection clause.) A half dozen or so supreme courts have struck down their states' sodomy laws as violations of state constitutional guarantees of liberty or privacy. (In 1986 the Supreme Court rejected an argument that sodomy laws violated such guarantees in the U.S. Constitution.) The latest rage in state constitutional politics is term limits. Since 1990 more than twenty states have amended their constitution to limit legislators' terms. (Many also tried to impose term limits on their members of Congress, but the Supreme Court held this unconstitutional in 1995.)

Thus, several books on various aspects of state constitutional politics have appeared in the last decade. Moreover, Greenwood Press (under Alan Tarr's editorship) is producing a book on each state's constitution—its background, its interpretation, and the controversies surrounding it. *Constitutional Politics in the States* is a nice addition to this growing literature. Among its eight chapters are three cross-state commentaries and five that focus on particular states.

The most comprehensive of the cross-state analyses is Donald Lutz's examination of the frequency of state constitutional amendment. He shows, among other things, that frequency is strongly related to length and negatively related to the duration of state charters. He also develops a scale of difficulty for amending constitutions and finds, not surprisingly, that amendments occur more often when constitutions are easier to amend (also see Donald S. Lutz, "Toward a Theory of Constitutional Amendment," American Political Science Review (June 1994):355–70). John David Rausch, Jr., describes the term limit phenomenon. He argues, not entirely convincingly, that this is not the mass movement it appears to be, but a movement run by elites. He focuses on campaign strategies in four states (Oklahoma, Washington, Michigan, and Florida) to support his thesis. The other cross-state chapter is an historical review by Alan Tarr of state constitution making and amendments. It is short, not intended to be comprehensive, and serves as an initial background chapter for the book.

Two of the particularly focused chapters examine California's "counterrevolution" (to use Barry Latzer's term), namely, the 1982 addition of Proposition 8, titled "Victims' Bill of Rights," to the state constitution by a 56% majority. This title is a misnomer, as Candace McCoy points out in her chapter analyzing the amendment. Its main provisions required the state courts to interpret the state constitutional guarantees against unreasonable search and seizure and compelled self-incrimination in the same way as the Supreme Court interprets the parallel provisions in the U.S. Constitution. No more judicial federalism here. Latzer's chapter discusses the California Supreme Court's liberal decisions that provoked the counterrevolution (and the 1986 defeat of Bird and two other liberal justices).

In 1978 Florida added a right of privacy to its constitution by a 60% margin after it was defeated by a 70% margin in 1978. Rebecca Mae Salokar discusses the pitfalls of the first campaign and the smooth operation of the successful effort two years later. Gerald Benjamin and Melissa Cusa analyze the politics of amending New York's constitution over the last three decades. Finally, and interestingly, Tarr and Russell Harrison apply Gerald Rosenberg's (*The Hollow Hope*, 1991) conditions of successful judicial implementation to two New Jersey Supreme Court school financing decisions and find that it only partly explains variance in the decisions' effectiveness.

One lesson several of these studies teach is that, because it is relatively easy to amend state constitutions (compared to the U.S. Constitution), state politics invite the frequent and often successful use of this tactic. This is particularly true in the 17 states where the constitution can be amended by initiative, bypassing the legislative process. Such amendments are more likely to alter the political or social status quo. McCoy and to a lesser extent Rausch argue that the initiative empowers special interests rather than the general public. Colorado's antihomosexual Amendment 2 may illustrate this. There are some recent instances of economic interests (unsuccessfully) using the initiative, especially in environmental policy. This, of course, is the opposite of what Hiram Johnson and other early-twentieth-century progenitors of the initiative intended. They wanted a device to get around legislatures whose members were being bought and sold by railroad magnates and other financiers. But McCoy in particular stretches the concept of special interest rather far. Widespread anger at liberal criminal justice rulings or desire to ensure greater legislative turnover are hardly the type of special interests Hiram Johnson had in mind. In fact, he may well have approved these goals.

By contrast, in the 25 states where legislative participation in the constitutional amendment process is mandated (in eight states, amendment is possible through a convention), amendments are unlikely to upset the status quo. Benjamin and Cusa show this to be the case in New York.

Constitutional Politics in the States is skewed toward the politics of state constitutional amendment. It is less focused on the role of state supreme court interpretations in state politics. I say this as an invitation, not a complaint; one book cannot cover the entire waterfront. But it seems to me that the greater personnel turnover (compared to the U.S. Supreme Court) of state court justices and their more direct answerability to the electorate should make constitutional politics qua politics more interesting at the state than national level. A complementary volume awaits us.

This book is a welcome addition to a thin literature. Except for Lutz's contribution, it is insufficient to support hypotheses or generalizations, but it is very suggestive of them and invites further and, I hope, broadly systematic work.

Rivals for Power: Presidential-Congressional Relations. Edited by James A. Thurber. Washington, DC: CQ Press, 1996. 267p. $24.95.

Robert J. Spitzer, *SUNY Cortland*

As a growing political science literature accustomed itself to a new, seemingly permanent partisan schism between the presidency and Congress, Bill Clinton came along and abruptly reintroduced the discipline, and the country, to united party control. Just as abruptly and perhaps more rudely, congressional Republicans spoiled this brief respite in partisan harmony only two years later by winning control of both houses for the first time in 40 years.

These two partisan gyrations helped prompt renewed interest in the all-important relationship between the first and second branches of government, including this new volume, edited by the veteran student of Congress, James Thurber. This book follows closely in style and contributors Thurber's earlier edited book, *Divided Democracy* (1991). Roughly half the contributors to the current book contributed to the last, although those who appear in both revised, in varying degrees, their new chapter offerings. The timing of this book suggests a close focus on the Clinton years or on Bush-Clinton shifts. The book's ambitions are greater, however, in that most chapters are looking for broader trends and the latest analytical insights.

In his introductory chapter, Thurber recounts seven bases for what he characterizes as the enduring rivalry between the branches. The list is familiar: constitutional design, constituency differences, varying terms of office, weak parties, divided party control, power competition, and pluralism. One of the more conceptually ambitious chapters is that of Roger H. Davidson, who adapts Stephen Skowronek's notion of presidents studied in political time to Congress (advancing on Davidson's similar chapter in Thurber's 1991 book). The virtue of Davidson's approach is twofold. First, it seeks to apply the concept to another governing institution; second, Davidson wants to rectify Skowronek's presidential centeredness. Both goals are laudable and warrant added work.

Walter J. Oleszek's chapter on congressional policymaking revolves around three developments that, he argues, will shape congressional policymaking into the next century: shifts in the national policy agenda, partisan instability, and divided government. Lester G. Seligman and Cary R. Covington offer a Neustadtian essay on the parameters of presidential leadership in Congress, in which they take the work of George Edwards, on the one hand (*At the Margins*, 1989), and Paul Brace and Barbara Hinckley (*Follow the Leader*, 1992), on the other, as the alpha and omega of executive leadership possibilities.

Leroy N. Rieselbach's chapter offers specific analysis of presidential coalition-building by examining Clinton's relations with the Democratic-controlled 103d Congress. Supporting the conventional wisdom, Rieselbach finds that Clinton could justly claim numerous victories (reflected in high CQ support scores, for example), while such prize achievements as health care reform slipped through his fingers. Partisanship was important, but so, too, were bipartisan coalitions for issues such as NAFTA. Partisan support does not neutralize deeper interbranch differences.

Jon R. Bond, Richard Fleisher, and Glen S. Krutz provide a useful overview of the current state of empirical work on presidential-congressional relations. While endorsing an analytically pluralistic approach, even including case studies, their goal is to cultivate the important empirical advances that they, Edwards, and other have provided. In the succeeding chapter, Scott R. Furlong applies the Bond/Fleisher empirical standard set out in their 1990 book, *The President in the Legislative Arena*, which found ideology and party to be the most important factors explaining congressional support of presidents. Furlong's examination of House voting patterns in 1989, 1992, and 1993 mostly supports this view.

One of the most interesting chapters, Christopher J. Deering's examination of the politics of military base closures, underscores the elegance of a well-done case study. Nowhere is the centrality of delegation and blame-avoidance more nicely illustrated. James P. Pfiffner takes a broader view of the 103d Congress, compared to Rieselbach's treatment, providing a synthetic assessment of Clinton's political and policy ups and downs. Alternately criticized for excesses of rigidity and compromise, Clinton demonstrated susceptibility to both.

Thurber's chapter analyzes six successive budget paroxysms, beginning with the 1974 budget act and concluding with Republican budget trimming in the 104th Congress. For all the budgetary furor of the last two decades, only the most recent two budget agreements have pushed decision makers closer to zero-sum decision making. Louis Fisher's superb analysis of Clinton's behavior as commander-in-chief underscores the executive branch's continued march toward sole authority over making war. While Clinton's military adventurism does not match the scale of his two predecessors, his sweeping justifications for unilateral military actions reach a new plateau. Fisher's treatment is the most concise account of Clinton's war powers philosophy to be found.

Michael L. Mezey's chapter applies key traits of the congressional system to draw out possible lessons for nations now struggling to reformulate their political structure. While Congress has contributed to the U.S. system's long-term political stability, Mezey concludes that a similar structure, transplanted to other nations, is unlikely to result in similar systemic stability for the simple reason that political culture and context must accompany the structure.

Students of presidential-congressional relations will find few surprises in this volume. Yet, it provides updates on recent research and insightful contemporary accounts of the Clinton era. Except for the Fisher and Mezey chapters, the book's primary focus is domestic policy.

A book of this sort is often time-bound, wedded to the particular political coalitions and leaders in power when it is written and published. To their credit, most of the contributors sense this trap and seek to draw out, or draw on, broader perspectives that give good books longer and more vital lives. Given the central importance of the presidential-congressional nexus, this volume should continue to find use regardless of the outcome of the next election.

Powersharing: White House–Cabinet Relations in the Modern American Presidency. By Shirley Anne Warshaw. Albany, NY: State University of New York Press, 1996. 380p. $24.95.

Thomas J. Weko, *University of Puget Sound*

Since its creation, the modern White House Office has intermittently found itself at the center of conflict in U.S. politics, most importantly during the Watergate and Iran-Contra affairs of the Nixon and Reagan presidencies. Among the indictments that emerged from these controversies, one of the most common is that the White House Office has tried to establish too much control over too many decisions that principally belong elsewhere, including decisions about the development of public policy proposals. The leaders of executive departments and senior civil servants (who possess institutional memory, programmatic expertise, and a welcome concern with the implementation of policy) properly deserve a large place in policy development but instead find themselves supplanted by the president's personal aides (men and women who are zealously dedicated to the president's own commitments and his short-term political fortunes but little else).

Shirley Anne Warshaw's *Powersharing* aims to shed light on this development, examining the changing role that cabinet departments and the White House staff have played in the development of domestic policy in the Nixon through Clinton administrations. "It is the central thesis of this book," writes the author in her introduction, "that the White House staff have indeed surplanted [sic] the cabinet as the president's chief policy advisors" (p. 4). Given that thesis, one might expect *Powersharing* to examine *why*. Exploring the forces that have led presidents to centralize policy initiation in the White House would allow the author to demonstrate how sweeping changes outside the executive branch—such as the multiplication of organized interests and vastly increased prominence of television—have reshaped the relationship between the White House and executive departments. Moreover, it would permit the book to be grounded in the most fruitful body of theorizing about the evolution of the modern presidency, the "rational choice institutionalism" of Terry Moe ("Presidents, Institutions, and Theory," in George C. Edwards et al., *Researching the Presidency*, 1993) and Samuel Kernell ("The Evolution of the White House Staff," in Can the Government Govern? ed. John Chubb and Paul Peterson, 1989). Instead, *Powersharing* ignores the forces fueling this development, save to say that "cabinet members establish strong relationships with players outside of the executive branch in order to fulfill their statutory mission, relationships that presidents have not always supported" (p. 2). That departmental executives serve many principals is a constant feature of executive politics in the United States, and one cannot explain change with a constant.

Alternatively, one might expect a study which posits that the White House staff has eclipsed the cabinet as "the president's chief policy advisors" to ask what the consequences have been—not only for presidents and presidential leadership but also for other institutions, such as bureaus and agencies, and for other values, such as administrative competence and continuity. Here, too, *Powersharing* is nearly silent. The book assumes that the subordination of executive departments to White House aides is beneficial to presidents and their prospects for leadership, and it lauds the Reagan administration for its success in harnessing executive branch policy development to Ronald Reagan's policy aims and political fortunes. Meanwhile, it ignores the possibility that the Reagan administration's assiduous efforts to "keep departmental initiatives in line with presidential objectives" helped badly damage the capabilities and organizational integrity of a host of agencies, such as the Environmental Protection Agency (See Richard W. Waterman, *Presidential Influence and the Administrative State*, 1989).

The thesis of *Powersharing* notwithstanding, the actual focus of the book turns out to be why some presidents do better (at some times) than others at getting control over policy development within their administration. Chapters describe how each president from Richard Nixon through Bill Clinton has conceived of the cabinet's proper role (and reconceived of it, in light of their experience); how presidents have selected their White House staff, cabinet, and subcabinet; what sort of formal structures presidents have adopted to link their White House aides and the cabinet departments in the development of domestic policy; and the success that presidents have enjoyed in marshaling executive branch policy development to their purposes. Surprisingly, the book does not present a theory linking ideas, staffing, and structure to one another, or to presidential success in marshaling policy formulation. Careful readers may weave together from the study the threads of a theoretical argument: Presidents control policy development when they select a cabinet and subcabinet marked by its fealty to the president's clearly articulated policy agenda and harnessed to presidential purposes through a formal structure of policy development that "keeps departmental initiatives in line with presidential objectives" (p. 149). This is a plausible argument, but it is the responsibility of the author to make it, not the burden of the reader, and its absence leaves the reader ill-equipped to organize and evaluate the description presented.

Even with a clearly stated theory, *Powersharing* would disappoint its readers, for it lacks the sort of evidence necessary to sustain its argument. Gathering systematic evidence about politics within the executive branch can be difficult, and most scholars, Warshaw included, rely heavily upon the published reports of participants and bystanders in Washington politics, that is, the memoirs of White House and cabinet veterans and reporting of journalists based in the capital. These sources present a host of problems: their explanations are deeply personalized, their evidence is often highly selective and anecdotal, and, in the case of memoirs, recollections can be notoriously self-serving. Presidency scholars who aim to surmount these problems (rather than repeat them, only more slowly) typically draw upon personal interviews and executive branch documents. To her credit, Warshaw does so in *Powersharing*. Unfortunately, the book's use of these sources is highly uneven.

Descriptions of some administrations, such as the Ford years, are drawn from an ample set of White House documents and interviews, while descriptions of many other administrations are drawn almost exclusively from secondary sources. Interviews with White House aides and White House documents are present, but documents and interviews from executive departments and the Office of Management and Budget are nearly absent. Some characteristics of these presidencies, such as the organization of the White House domestic policy staff, are capably documented, while other characteristics are not. The most important of these lacunae is the absence of systematic evidence about White House influence over policy development—a serious problem for a study which posits that White House aides have supplanted the cabinet as the president's chief policy advisors. Had *Powersharing* followed the lead of John Kingdon (*Agendas, Alternatives, and Public Policies*, 1984) and Paul Light (*The President's Agenda*, rev. ed., 1991) and drawn upon interviews to flesh out the role of White House staff and departmental executives in identifying problems, generating alternatives, and selecting the alternatives that make up the president's domestic program, it could have made an important contribution to our understanding of presidential leadership and the evolution of the modern White House Office. Instead, hampered both by problems of theory and evidence, *Powersharing* contributes only modestly to our understanding of the White House, the cabinet, and their relationship in the development of domestic policy.

Race Relations Litigation in an Age of Complexity. By Stephen L. Wasby. Charlottesville: University of Virginia Press, 1995. 421p. $65.00 cloth, $22.50 paper.

Susan M. Olson, *University of Utah*

This book pulls together Stephen Wasby's long-time research on civil rights litigation, some of which has appeared in articles and papers over the past decade and a half. Based on secondary sources and interviews with 41 lawyers, it is an ambitious and impressive work of scholarship that documents many facets of civil rights litigation from the late 1960s through the early 1980s. Although the focus is heavily on litigation on behalf of African Americans, Wasby makes frequent comparisons with similar litigation by women, other ethnic minorities, and persons with disabilities.

The book's central argument is that civil rights litigation since *Brown* v. *Board of Education* is not meticulously planned and unproblematically executed, but is complicated and has to respond to many contingencies. The complications include the proliferation of litigating interest groups and the consequent competition for resources, the spread of civil rights policymaking from courts to other branches, and the greater moral complexity of civil rights issues after the end of de jure segregation.

Wasby implies that his complexity argument is novel, but the exact focus and source of the counterhypothesis is unclear. Wasby suggests that Richard Kluger's *Simple Justice* (1976) overstated the rationality and neatness of *Brown*. Kluger's account did influence other works, including my own (*Clients and Lawyers* 1984, 4), but Mark Tushnet's *NAACP's Legal Strategy against Segregated Education, 1925–1950* (1987, 144) has already corrected oversimplified versions of the early history of civil rights litigation. Moreover, Wasby's own account is explicitly limited to the post-*Brown* period. Wasby implies that "others" have made Kluger's mistake about the more recent period (p. x), but he does not cite them. Indeed, Wasby does not question that much of the civil rights litigation he reports is planned (p. 336), so it seems to be primarily its execution that has become even more complicated since the days of *Brown*.

The reasons for choosing the study's time period are also somewhat unclear. The beginning is roughly the passage of the Civil Rights Act of 1964 and the Voting Rights Act of 1965, which complicated civil rights litigation by adding statutes and regulations to constitutional provisions as potential causes of action. Less indication is given for ending the study in the early 1980s. Wasby discusses the drastic change in the political environment for civil rights litigation under the Reagan administration as another source of complexity, but that situation extended well beyond the end of the study. Life, of course, is seamless in many ways, but the lack of a reason for Wasby ending the study when he does may also reflect his statement that "no particular social theory drives this study" (p. xii).

Without guiding theory, the scope of the book is so broad and the issues discussed so interrelated that the work could have been organized in a number of ways, as Wasby acknowledges (p. xix). After a brief discussion of the complexity argument, the book proceeds through the following topics: the changing political environment of civil rights litigation, the Supreme Court's effect on litigation, the major litigators and the proliferation of groups, the resources needed for litigation, the decision to use litigation as a strategy and whether it is successful, litigation complexity and choice of federal or state forum, the characteristics of "planned" litigation, groups' choice of area of law and particular cases, amicus participation as an alternative to case sponsorship or intervenor status, factors affecting the dynamics of litigation, staff and cooperating attorneys and their relationships with clients, and relationships among civil rights organizations. This last chapter describes in some detail the strained relationship between the NAACP and the NAACP Legal Defense Fund.

Within some of these topics, which have been explored by many scholars, such as the reasons groups choose to use litigation and whether litigation is successful, Wasby discusses and reports evidence in support of various competing explanations, but he rarely takes sides and argues that one conclusion is more valid than another. For example, with respect to Gerald Rosenberg's argument in *The Hollow Hope* (1991) that litigation did not accomplish much, Wasby claims, first, that this argument "may be beside the point because litigators believe they can make a difference" (p. 110), since they largely subscribe to the myth of rights. He then goes on to say, in a tone more critical than that in most of the book, that civil rights litigation may hurt itself by prompting the creation of other groups who compete for the same resources and by sticking with issues like school desegregation instead of more directly redistributive ones, which I take to support Rosenberg's argument that litigation tends to be "flypaper."

The book's topical arrangement means that Wasby tells just enough about a particular case or incident to illustrate the immediate point. Different aspects of the same case or incident may appear in several chapters, but readers who are not already familiar with, for example, the issues in *Moore* v. *Dempsey* or *Adams* v. *Richardson* may feel a little confused. Readers can, of course, pursue their questions from Wasby's carefully documented sources, but the book's reliance on endnotes alone without an alphabetical list of works cited means one often has to hunt back through prior notes to find full citations.

In short, Wasby's book is best suited for the serious scholar who either is already quite familiar with civil rights litigation and wants a synthesis of the subject or is new to the subject and wants an introduction to use as a point of entry to research that treats the individual subjects in more detail. The absence of complete "stories" (except for that of the NAACP-LDF conflict) would make the book difficult for the casual reader, and the usually noncritical reporting of the lawyers' perspectives leaves the development of theory to others. Nevertheless, the book makes an important contribution with the comprehensive range of its coverage of issues raised by black civil rights litigation, most of which are relevant to other cause litigation as well.

War Powers: The President, the Congress, and the Question of War. By Donald L. Westerfield. Westport, CT: Praeger Publishers, 1996. 245p. $55.00.

Louis Fisher, *Congressional Research Service*

This book focuses on the constitutional and political allocation of the war power between Congress and the president, concentrating particularly on the period from the Vietnam War to the Gulf War. Useful documents are reproduced throughout the chapters and, at the end, in an appendix. The author seems largely to favor independent presidential power. The book begins by quoting President George Bush, who in 1991 signed legislation authorizing military action against Iraq; his signing statement claimed that he could have acted without first receiving statutory authority. That theme carries throughout the book.

Westerfield states that the Vietnam War (and the War Powers Resolution that flowed from it) came from an "unrepresentative period . . . an anomaly by any standard of history." No previous war or foreign conflict, he claims, "had been so universally rejected by both the civilians and military as the Vietnam conflict." Never had there been "such a flagrant disregard for the constitutional division of powers and such a misuse of executive power as the extension of the war into Laos and the bombing of Cambodia" (p. 1). But what about the Korean War? At least Congress, through the Tonkin Gulf Resolution, authorized the initial actions by President Johnson against North Vietnam. President Truman never sought, or received, any authority from Congress for going to war against North Korea. Westerfield states: "Beginning, during, and after the Korean conflict, the public, the Congress, and the military, with few exceptions, supported President Truman's actions to defend South Korea, especially in light of the authorizations by the United Nations Security Council" (pp. 1–2).

First, authorization by Congress through the Tonkin Gulf Resolution was constitutionally more significant than the Korean resolutions passed by the Security Council. Second, the public initially supported military action in both Vietnam and Korea, but public opposition surfaced much quicker against the Korean War. Although the author says there was a "general feeling of pride to serve in the military" during the Korean War, veterans of that conflict returned home to an unappreciative public.

By labeling the Vietnam War as "unrepresentative," Westerfield can argue that its fruit—the War Powers Resolution—is suspect. It may be suspect and may be a good candidate for revision or repeal, but not because its triggering force was an "unrepresentative" Vietnam War. Executive aggrandizement of the war power has been a steady factor ever since the close of World War II. The first and most glaring example of presidential abuse was Korea, not Vietnam.

The author cites a paper by William H. Rehnquist in 1970, when he was serving with the Department of Justice. Rehnquist wrote that the nation had often acted militarily without a declaration of war by Congress, which is no doubt true. But the author then comments that Rehnquist "does not mince words in indicating that the president does have power to conduct 'use of force' operations without a formal 'declaration of war' by Congress. The implication that the president can 'make war' but cannot 'declare war' would seem to reduce the war powers of Congress to those few situations for which Congress felt the need to formally declare war" (p. 23).

Rehnquist did not say that congressional power was "reduced" to formal declarations. In the very quote selected by the author, Rehnquist notes that Congress can both declare and authorize war, and that Congress authorized President Adams to act against France in the Quasi-War from 1798 to 1800. A page later the author describes how President Jefferson, in the Barbary Wars, relied on "definite [statutory] authorizations of his commands" (p. 24). Congressional power is not limited to merely declaring war.

In a chapter on the UN Charter, the author says that UN military actions depend on "special agreements" that indicate the armed forces each nation will make available to the Security Council. Westerfield never discusses how the United States, in the UN Participation Act of 1945, chose to handle special agreements. Without the slightest ambiguity, this statute states that special agreements "shall be subject to the approval of the Congress by appropriate Act or joint resolution." Again, Congress authorizes, rather than merely declares, military action.

The author implies that Senate approval of the UN Charter enables the president to act militarily without first asking Congress for authority and that "Congress" authorized military actions in Vietnam through the Southeast Asia Treaty Organization (SEATO) (p. 79). But the Senate approved the treaty, not Congress. In ratifying such treaties as the UN Charter and SEATO, the Senate did not surrender the war power to the president. The UN Participation Act anticipated that both houses of Congress would have to authorize military action, and nothing in the legislative histories of SEATO, NATO, and other defense pacts signals an abandonment of legislative power. Otherwise, the president and the Senate could use the treaty process to eliminate a role for the House of Representatives. If SEATO was sufficient authority for President Johnson to conduct military operations against North Vietnam (p. 80), why did he come to Congress for the Tonkin Gulf Resolution?

The book misinterprets congressional action in 1990. The author says that a resolution passed by the Senate on October 2 "seems to give the president the 'constant' necessary to continue the buildup of forces and plans to repel the aggression." But the Senate action was merely a concurrent resolution, the very nature of which provides a nonbinding sense of attitudes, not legal authority. Second, the House never acted on this resolution, so at most it offers the Senate's view. Third, the concurrent resolution itself anticipated that Congress would have to support military action through the regular statutory process. The resolution supported President Bush in accordance with "United States Constitutional and statutory processes, including the authorization and appropriation of funds by the Congress" (p. 139).

The book states that in 1993 the Senate "settled on a 'sense of the Congress' resolution" to restrict military operations in Somalia, but the resolution was "made unnecessary" when President Clinton agreed to withdraw troops by March 31, 1994 (p. 171). In fact, it was binding law—not a sense-of-Congress resolution—that compelled Clinton to remove the troops (P.L. 103-139, sec. 8151).

Westerfield states that as of February 1994 President Clinton had not decided to use U.S. air power in Bosnia (p. 171), but four pages later the book is current as of June 1995 regarding the House attempt to repeal the War Powers Resolution. Judging by this latter date, the author could have mentioned the air strikes that began in April 1994, were repeated in August 1994, and continued throughout the first half of 1995.

Abortion Rates in the United States: The Influence of Opinion and Policy. By Matthew E. Wetstein. Albany: State University of New York Press, 1996. 153p. $49.50 cloth, $16.95 paper.

Ted G. Jelen, *Benedictine University*

Scholarly works which set the intellectual agenda for subsequent analyses are often termed "seminal." If I may be permitted to extend the reproductive metaphor, Wetstein's *Abortion Rates in the United States* might well be termed "conjugal." Although this will be an important book, rather than serve as the starting point for future research, its main contribution is to synthesize two diverse strands of research and to integrate them into a coherent whole.

In this brief volume, Wetstein combines and analyzes two distinct research literatures in the politics of abortion: analyses of public opinion, in which the dependent variables are typically public attitudes toward legal abortion, and accounts of abortion policy (typically performed at the state level) in which the dependent variables are state-level policy restrictions on abortion (or state abortion rates), and the independent variables are generally institutional in nature. Wetstein demonstrates a superlative command of both literatures and devotes most of the book to his own empirical analysis.

In the aftermath of the 1989 *Webster* decision, states have had increased discretion in the formulation and implementation of abortion policy. Furthermore, most state restrictions (with the exception of Medicaid funding) can be arrayed as part of a unidimensional continuum. Wetstein applies extensive dimensional analyses of various measures to state abortion policy to show that most measures used in the literature measure the same phenomenon. Thus, we can be reasonably clear about the meaning of abortion policy restrictions as dependent or intervening variables. Wetstein also uses the percentage of counties containing abortion providers within a state as an indicator of the availability of abortion services. Wetstein's ultimate dependent variable is (as his title would indicate) state abortion rates.

Wetstein's conclusions can be stated rather simply. Most important, public opinion matters a great deal in the explanation of abortion rates. Public opinion affects state-level policymaking (and, indeed, is more important than any set of institutional variables) and has a direct effect on the availability of abortion services. Policy and availability, in turn, have very strong effects on abortion rates. At the state level, then, public opinion has a major (if indirect) effect on the rates at which abortions are performed.

The importance of these results lies precisely in their pedestrian character. Analysts such as Laurence Tribe and Kristin Luker have characterized the abortion issue as a "clash of absolutes," in which incommensurable world views are pitted against one another. The incidence of violence against abortion providers is often taken as evidence that abortion is an issue which is dysfunctional for democratic civility. Wetstein's analysis, by contrast, suggests that public opinion on the abortion issue is quite comfortably accommodated by state policymaking institutions. Although the enormous symbolic importance of abortion cannot be gainsaid, the actual delivery (or nondelivery) of abortion services is quite responsive to public opinion. The day-to-day "resolution" of the abortion issue more closely resembles "normal" political discourse than any issue which democratic institutions cannot handle.

Wetstein has thus provided an empirically solid, methodologically sophisticated, and normatively important contribution to our understanding of the politics of abortion. This is an important book, which will be required reading for scholars seeking to understand abortion politics in the United States.

Given this generally positive evaluation, it seems appropriate to note certain "sins of omission" in Wetstein's treatment. Most important, Wetstein gives short shrift to the normative implications of his analysis. While he makes occasional references to "democratic theory," he fails to develop the systematic implications of his findings. While this is not a fatal flaw, the reader is left with a sense that Wetstein may not fully appreciate the importance of his analyses.

Wetstein also, in my view, expresses an excessively positivist view of "explanation," equating it with proportion of variation explained or goodness of (empirical) fit. While statistics such as R^2 or chi-square (for LISREL models) are certainly illuminating and provide a sense of the empirical adequacy of particular models, they do not suffice to put to rest the inquiring mind. Indeed, Wetstein's reliance on statistical measures of explanatory adequacy may lead him to gloss over certain anomalies in his results. For example, Wetstein shows that the proportion of Catholics in a state's population makes no direct contribution to state public opinion on the abortion issue (p. 121). Unlike Protestant fundamentalism, which has a strong influence on net abortion opinion, Catholicism operates directly on the propensity of states to enact restrictive abortion policies. Why is this?

A number of quantitative and qualitative studies have suggested that the presence of a large Catholic population in a given state occasions a pro-choice countermobilization among the state's non-Catholic citizens. That is, while individual Catholics are somewhat more pro-life than the average citizen, their non-Catholic neighbors are more pro-choice (even after controls for urbanization have been imposed). For reasons which remain to be investigated, there is no comparable countermobilization generated by evangelical Protestants. Wetstein's results indirectly confirm this set of findings and suggest that the Catholic Church may have its strongest effect on state abortion policies (and rates) through elite-level interest group activity rather than as an agent of political socialization. The different manners in which religious values influence public policy are fascinating and would seem to deserve comment, if not serious analysis. Again, the reader is left with the sense of a missed opportunity.

None of this detracts from the importance of Wetstein's contribution. Wetstein has provided a valuable resource, one which should be read by anyone interested in abortion politics or in the politics of state-level policymaking.

Comparative Politics

State, Labor, Capital: Democratizing Class Relations in the Southern Cone. By Paul G. Buchanan. Pittsburgh: University of Pittsburgh Press, 1995. 395p. $49.95.

Ben Ross Schneider, *Northwestern University*

If book jackets had warning labels, the one for this book might caution that the prose of the first three chapters is dyspeptic, but, for hardy digestions, the rest of the book offers a wholesome mix of historical analysis and original data. This review concentrates on the latter part, which offers an in-depth examination of the changes in national labor administrations (which comprise not only labor ministries but also policies that affect labor) during the transitions to democracy in the 1980s in Argentina, Brazil, and Uruguay. Drawing heavily on Adam Przeworski's analysis of class relations in postwar Europe, Buchanan argues that consolidation of democracy depends on managing class compromises, which are heavily mediated by "national labor administrations." Of the three cases, labor relations and administration improve or democratize the most in Uruguay, and these changes augur well for democracy there. The experiences in Brazil and Argentina are more mixed, and democratic consolidation is as yet "thwarted or incomplete" (p. xix).

The strengths of the book are several. For one, Buchanan moves beyond overly aggregate and usually vague discussions of the consolidation of democratic regimes to the analysis of what Philippe Schmitter calls partial regimes, such as legislative, judicial, and electoral regimes. One partial regime governs labor relations as mediated by government (strongly in Brazil and Argentina, weakly in Uruguay). Buchanan argues convincingly that the key to democracy in this partial regime lies in the "national labor administration." The analytic reaggregation, however, is less persuasive than the disaggregation, for Buchanan offers no single clear formulation of the causal connections between the partial regime of labor relations and the composite democracy. At some points in the book the connection is less causal and empirical than deductive and assumed: The book's "fundamental premise is that democratic capitalist regimes are founded on a class compromise between the organized working classes and capitalists that is mediated by the state" (p. xiii). The conclusion of the lengthy theoretical and conceptual discussion is that national labor administration is the key to class compromise, which is in turn essential to democratic consolidation. Yet, by the end of the book, Buchanan acknowledges that democracies can survive if not thrive without class compromise or democratic partial regimes in labor relations (p. 286). Such an admission is wise given the recent experience in Latin America, where democracies have achieved record life spans without the kinds of class compromises in postwar Europe that inspire so much of the book's theoretical agenda.

Another strength of the book is empirical. Buchanan spent a long time in Latin America combing through ministry archives, press clippings, and contemporary debates in the scholarly communities of each country. The research on labor relations is exhaustive in scope. It covers the full range of government policies as well as changes in the organization, jurisdictions, personnel, and budgets of labor ministries. Unfortunately, in-depth empirical coverage is limited to only a few years in the mid-1980s in each country and essentially ends in 1988. The mid-1980s were years of major change in labor relations in all three countries, but the empirical narrative of a book on institutions and consolidation needs to follow the changes on into the 1990s to establish which ones lasted. A plausible hypothesis, for example, would be that much stronger governments after 1990 in Argentina and Brazil had deeper and more lasting effects on labor relations than did the weak and tumultuous governments of José Sarney and Raúl Alfonsín, which are covered in the book.

A last strength of the book is the deep comparative analysis of three countries. Comparative work on Latin America can be superficial and rarely reports on original field work in more than one country. Buchanan deserves credit for conducting research in three very different countries (and partial absolution for the creative spelling of many Portuguese words). One common outcome worth emphasizing, in contrast to many other studies that cast doubt on whether regime makes a difference, is that the transition to civilian rule in the mid-1980s had profound consequences for labor administrations in all three countries (p. 279). That conclusion is not Buchanan's main focus, however. Buchanan concludes that much more progress has been made in reforming and democratizing labor relations in Uruguay than in the other two countries. He notes many of the differences among the countries but does not come down with a parsimonious explanation for Uruguayan exceptionalism. Doing so might have taken the book in more immediately profitable theoretical directions.

For example, one core hypothesis which emerges from the empirical chapters is that Uruguay democratizes labor relations more quickly because well-institutionalized unions, ministries, and procedures for collective bargaining were more democratic before the military took power in 1973. In contrast, state corporatist labor relations had prevailed in Argentina and Brazil since the 1940s and well before the military took power. Such an argument would have bolstered the institutionalist aspect of Buchanan's theoretical apparatus, though at the same time it would have raised doubts about his empirical strategy of examining only several years of change in national labor administrations. If the legacy of state corporatism weighs so heavily on Brazil and Argentina, it should take some time to dismantle it and replace it with more democratic labor administration. The other obvious hypothesis is that negotiated class compromise is far harder to achieve in dispersed, heterogeneous, continental countries like Brazil than in smaller, more homogeneous countries like Uruguay with a population smaller than Brazil's major cities. Buchanan acknowledges this argument in the conclusion (p. 277), but sustained engagement would have led the book down a different theoretical path along with similar work on Europe by Peter Katzenstein. As these extrapolations illustrate, there is a lot of provocative material in this dense book, which should be of great interest to students of labor in Latin America.

Divided Sun: MITI and the Breakdown of Japanese High-Tech Industrial Policy, 1975–1993. By Scott Callon. Stanford: Stanford University Press, 1995. 290p. $29.50.

Maurice Wright, *University of Manchester*

The role and influence of bureaucrats in Japanese policymaking have been debated for the past three decades. Progressing from oversimplified explanations of Japan, Inc., and a triadic power elite of the bureaucracy, big business, and the ruling Liberal Democratic Party, a narrower and sharper focus was provided by the "revisionist" school, which urged theories of bureaucratic dominance. This conventional wisdom derived mainly from Chalmers Johnson's 1982 historical study of MITI's role in industrial policymaking and the Japanese economic miracle. It was in turn challenged with the accumulating evidence of empirical analysis of institutions, processes, and participants of different industrial sectors to provide explanations of particular policy outputs, or changes in policy outputs, or why attempts to change such outputs succeed or fail. The counterrevisionists' critique of bureaucratic dominance provides a more nuanced and sensitive interpretation of the making and carrying out of different industrial policies.

Writing as an unabashed counterrevisionist, Callon makes the central argument that MITI's industrial policy regime collapsed as the Japanese economy was transformed from a catch-up follower to a caught-up economic superpower in the period 1975–85. The paradigm of coherence and cooperation that formerly marked the relationships among MITI, private companies, and other bureaucratic actors was displaced by one of competition and conflict. MITI's industrial policy for microelectronics was neither cooperative nor successful. The dominant position achieved by Japanese companies occurred despite not because of MITI.

From the 1970s MITI's main industrial policy was to promote the knowledge-based industries. A key instrument, developed in the 1960s, was the research consortium, through which MITI brought together the leading companies to undertake joint R&D. MITI provided leadership and organization, dictated the research agenda of projects and issues, and provided half the funding. This competitive collaboration appeared to work: Japan's rapid rise in microelectronics in the 1960s and 1970s was widely attributed to the conspiracy in the public interest organized by MITI. Building on that success, MITI set up four more consortia for high-technology projects, and these provide the empirical evidence to support the book's central argument.

The relationships between MITI and the participating companies as well as among the latter were marked by continual conflict over research aims, funding, and organization. Companies were reluctant to join the consortia, and their cooperation was minimal and grudgingly given. When participation in projects did not appear to serve their own interests, they tried to minimize their involvement with MITI. They refused to provide matching funds, to second key research staff, and to participate in joint labs. MITI was forced to make major concessions. In the supercomputer consortium the concept of a joint lab was abandoned; MITI was obliged to fund the whole project; the companies dictated the research agenda.

Only the VLSI consortium achieved the goals set by MITI, due more to the size and effectiveness of subsidies than to the cooperation of the participating companies in joint R&D. The key to success "was *not* the joint lab. It was the work done in the companies' private labs with MITI funds" (p. 119). Both the Supercomputer and Fifth Generation consortia "chased exotic technologies that did not pay off" (p. 115); the goals of the latter were impossibly ambitious and unfulfilled: "It produced specialized machines that no one wanted to use" (p. 145). The failure of the TRON consortium to install a standard educational PC in Japanese schools exposed the limits of MITI's powers. In a bitter and prolonged bureaucratic conflict over jurisdiction with the Ministry of Education (MOE), MITI lost. Bureaucratic turf wars characterized the operation of the other three consortia, with MITI's claim to industrial policy jurisdiction disputed, as R&D issues crossed organizational boundaries and policy jurisdictions. MITI's policy shift, away from the applied technology R&D conducted by its consortia in the 1960s into basic research, brought it into direct conflict with MOE, responsible for all research in national universities. Its policy initiatives in the Supercomputer, Fifth Generation, and TRON consortia were hamstrung by MOE's unwillingness to allow its academic researchers to work in MITI-financed labs or to contribute to MITI projects. There was conflict, too, between MITI and the companies as well as among the latter.

MITI's failure derived from the profound change in its policy environment. First, the transformation of the Japanese economy changed MITI's relationships with big companies. Once dependent upon the ministry to protect their domestic markets, the companies had grown in size, competitiveness, and independence. Second, as microelectronics companies moved to the edge of the technology frontier, catching up and then overtaking U.S. firms, they could no longer rely on U.S. patents, licenses, and know-how. They were forced to innovate, to increase their own R&D spending to create new technologies; they had less need of MITI seed money. Third, the deterioration in trade relations with the United States in the late 1970s, which developed with Japan's increasing market share in many high-tech industries, and the accumulating surplus on the trade account meant that MITI's former aggressive promotion of Japanese industry was no longer tolerated.

MITI's loss of power and prestige forced it to attempt to expand its policy jurisdiction into new areas, such as telecommunications and venture capital, as it searched for a new role. But here and elsewhere it has been repeatedly rebuffed by other bureaucracies.

The book provides useful additional evidence that cooperative business-government relationships are no longer (if, indeed, they ever were) distinctive features of Japanese (and East Asian) capitalism and that calls for positive industrial policies are being resisted. In his enthusiasm to roll back the revisionists, Callon extrapolates from limited case- and time-specific evidence to make the larger claim that the whole basis for Japan's postwar industrial policy has disintegrated since the 1970s. While it is true that a significant disjuncture apparently occurred in the making and carrying out of industrial policy between 1975 and 1985, it is still unclear what role MITI and industrial policy really played before that. The explanation of the economic miracle of the high-growth era now appears to be both more complex and historically determined than the revisionists have argued. Other institutions in both the public and private sectors as well as other processes, such as the formation of industrial credit, may have contributed as much or more to the miracle as did MITI and its industrial policies.

The Role of the State in Economic Change. Edited by Ha-Joon Chang and Robert Rowthorn. Oxford: Clarendon Press, 1995. 302p. $74.00.

Chung H. Lee, *University of Hawaii at Manoa*

This volume, containing an introductory chapter and a paper by Chang and Rowthorn as well as nine papers by eight other writers, addresses one of the most important questions in economics and political economy: What role should the state play in economic change? This has been a central issue in political economy ever since the publication of Adam Smith's *Wealth of Nations* more than two hundred years ago. As far as most neoclassical economists are concerned, however, the answer is Smith's laissez-faire and its modern reincarnation, neoliberalism.

According to neoliberalism, the role of the state in economic development and in the transition from a centrally planned socialist economy to a market economy is to be limited to maintaining macroeconomic stability and allowing prices to be freely determined in competitive markets. The papers in this volume, all by scholars based outside North America, are an attempt to challenge neoliberalism, which now dominates economics teaching in North America.

Part I contains two chapters on theoretical overview. The first, by Chang and Rowthorn, makes a cogent argument for bringing back the state as an agent of economic change. It is argued that in times of major change the state needs to play the dual role of entrepreneur and conflict manager. The first role is, in fact, related to industrial policy as practiced in Japan and Korea and, to a lesser extent, in Taiwan. It should be pointed out, however, that when the state carries out industrial policy it must have an organization that can effectively implement its entrepreneurial vision. In the case of the East Asian economies, such an organization has included some large private or quasi-private organizations besides the state bureaucracy, and what has made that organization effective in achieving developmental objectives is the way the state interacts with these subordinate organizations. In other words, the state is at the apex of a larger quasi-internal organization mobilized to achieve developmental objectives.

The second role of the state is necessary for a developing or transition economy, as the authors point out, because an economic change affects the values of assets held by various members of the society, making some of them winners and others losers. Creative destruction may be the essence of capitalistic development, but only if the losers are willingly destroyed. To the extent that the state smoothly manages this win-lose situation, it facilitates structural changes necessary for rapid economic growth.

To argue that the state should play the dual role as entrepreneur and conflict manager is one thing, but to create a state that is capable of playing such a role is another matter. As the authors recognize, such a state does not come cheap, and we must recognize that creating it, especially in poor developing countries, is a daunting task that will require resources well beyond those available to LDCs.

In the second chapter of Part I, Mica Panic argues that the slowdown of the world economy after 1973 was due to (1) a reduced scope for independent national policies and (2) a decline in international economic cooperation with the demise of U.S. hegemony. Although I agree that external factors have an important bearing on the economic condition of a country, I would put less weight on their importance than does Panic. First, the vigor of the East Asian economies continued throughout the 1970s and 1980s. Second, Korea, for example, when its economy was becoming increasingly integrated with the rest of the world in the 1970s, carried out its program of heavy and chemical industrialization with subsidies and protective measures. To put it differently, the globalization of the economy did not prevent Korea from carrying out an import-substitution program, which the World Bank was known to have opposed.

The world may have changed since the 1970s, becoming more interdependent, and it may be more difficult now than before for any nation to carry out independent policies. But this may be a double-edged sword. While greater interdependence may reduce the scope for independent national policies, it can also be used by the state to overcome distributional coalitions that oppose necessary changes in economic policy. That is, external pressures can be exploited by the state in its role as a conflict manager. Overall, however, I agree that the globalization of the world economy will eventually require a supranational form of political organization and economic management.

Part II of the volume consists of case studies on the role of the state in economic development in market economies—the United States, by Kozul-Wright; East Asia, by Wade; several late-industrializing countries in Asia and Europe, by Vartiainen; India, by Singh; and Africa, by Van Arkadie. The basic point is that in many of the late-industrializing countries the state has been an interventionist of one kind or another. In some countries the state's role was positive, while in others it was not, and in some countries industrial policy was carried out, while in others the state's role was limited to creating human and physical infrastructure. What these studies clearly demonstrate is that the debate on the role of the state in economic change should not focus on the dichotomous choice between an interventionist and a noninterventionist position but, rather, on the optimum point in the wide spectrum of state intervention.

The three chapters in Part III by Ellman, Nolan, and Van Arkadie analyze the transition experiences in Europe and Asia and relate to the recent debate on transition—the "big bang" versus an evolutionary approach. The big-bang or shock-treatment approach identified with neoliberalism and adopted, by and large, by the European transition economies has not been as successful as expected. But China, Vietnam, and Lao PDR, which have adopted the gradual approach, have fared much better than their European counterparts, the difference being attributed to the continuity of the political regime and other institutions in the former.

It seems that a major mistake of the shock-treatment approach was the neglect of institutional preconditions for its success. Creating new formal institutions takes time and cannot be based on some universal formula, since they must be congruent with already existing informal institutions, such as culture. By taking an evolutionary approach, the transition countries in Asia have been able to keep many of the existing institutions, however imperfect they may be, instead of starting the transition process in an institutional vacuum.

The message of this volume is clear: Bring back the state as an active agent in economic change. This message may not produce many converts in the neoliberalism camp, but the editors should not despair, as they have done a fine job of raising the level of discourse on the role of the state in economic development and transition.

Economic Transition and Political Legitimacy in Post-Mao China. By Feng Chen. Albany: State University of New York Press, 1995. 246p. $49.50 cloth, 16.95 paper.

Growing Out of the Plan: Chinese Economic Reform, 1978–1993. By Barry Naughton. Cambridge: Cambridge University Press, 1995. 379p. $49.95 cloth, $18.95 paper.

The Chinese Reassessment of Socialism, 1979–1992. By Yan Sun. Princeton: Princeton University Press, 1995. 352p. $45.00 cloth, $18.95 paper.

Marc Blecher, *Oberlin College*

These volumes approach the structural reform of China's political economy from radically divergent analytical perspectives. Moreover, for reasons not necessarily related to their approaches, they also offer sharply disparate evaluations of the structural reforms and of China's future. Barry Naughton, an economist, focuses on the macro- and microeconomics of reform, seeing policy and institutional change as their effects; as do too many economists, he sees politics as not particularly important at best, and downright distortive at worst. By contrast, political scientists Feng Chen and Yan Sun argue that Chinese theoretical and ideological debates about political economy have been crucial in shaping reform policy. But where Naughton and Sun, respectively, regard economic and ideological factors as having smoothed the potentially rocky pathway of structural change, setting it on a relatively firm foundation, Chen argues that ideology is in a profound crisis that threatens to spill over into the material world of political economy.

Naughton argues that the first fifteen years of structural economic reform comprise a coherent and effective whole, but that this was achieved incrementally as a result of fortuitous economic and historical circumstances rather than political design. The key was the piecemeal expansion of economic options. The collective and private sectors were permitted—nay, encouraged—to expand outside the reach of the state-planned economy and to compete with it. Enterprises of all kinds, including state-owned and -planned ones, could easily enter newly burgeoning sectors, such as consumer goods and foreign trade. Market allocation began to supplement and gradually replace planning in a number of arenas, including consumer goods, producer goods, and, to a much more limited extent, even finance and enterprise equity.

Naughton finds that all this was achieved not by politics and the state but despite them or because of the leadership's and the bureaucracy's basic willingness to stand aside. Efforts to shape structural economic reform by various politicians, including putative reformers such as Deng Xiaoping, and by their opponents more wedded to planning, as often as not ended up impeding reform. For example, Naughton associates Deng with the early efforts to pursue an overly bold ten-year plan. He is also critical of periodic efforts led by so-called conservatives to slow or limit structural reform. Yet, he finds a silver lining in these politically erected obstacles. The grandiose Ten Year Plan of 1977–78, which vastly increased the proportion of economic resources under central control, produced such a serious economic crisis, including excessive investment, drastic shortages, and ballooning foreign debt, that the leadership was left with little alternative but to embark on a broad program of structural economic reform in late 1978 at the famous Third Plenum. Likewise, the politically induced slowdown of reform in the early 1980s also had significant salutary effects on subsequent structural reforms, by decentralizing finance, encouraging marketization, and promoting the collective and petty private sectors.

In addition to the unintended favorable effects of political intervention, politics played an intended positive role by getting out of the way of the economy. The surprising willingness of the party leadership and the government bureaucracy to do so are, for Naughton, also the result of economic forces, such as fiscal crisis, not the product of ideological commitment or political wisdom or skill. Politics factors into its own self-limitation only historically and accidentally: fortuitously, the Great Leap Forward and the Cultural Revolution weakened central planning, undercutting the power of the state to run the economy.

Growing Out of the Plan makes several very important contributions. Whether or not Naughton is right in attributing the 1978 breakthrough of structural reform to economic necessity rather than politics, his analysis provides an important supplement and corrective to political scientists who have given short shrift to the severity of the short-term economic difficulties. The same is true of his fascinating argument about the unintended effect of the policies designed to block structural reform in the early 1980s. He also offers a provocative and well-grounded challenge to two articles of faith about the Chinese economy. First, Naughton argues that the state-owned sector is not nearly as moribund or as dead a weight as is commonly taken for granted. Using a novel approach focusing on equalization of profit rates, as well as much other statistical and qualitative evidence, he contends that state enterprises have actually responded very seriously and effectively to the competitive forces that have come to surround them. They also account for a much larger share of China's stunning economic growth than is commonly credited by analyses focusing on rural and urban collective enterprises and the private sector. Second, he takes a positive view of the fact that the Chinese economy has been only partially reformed, in contrast to many others (including Feng Chen, Yan Sun, and myself) who have focused on the contradictions, tensions, and crises of an economy with one foot in the plan and another in the market. Central to his argument is the uniquely Chinese phenomenon of dual-track pricing, according to which state enterprises produce partly for the plan, buying their inputs and selling their outputs inexpensively through the state-planning apparatus, while also producing for the market with its higher prices not only for outputs but also inputs. Whereas most analysts have emphasized the irresistible opportunities for corruption and for abuse of state resources created by this mixed system, Naughton emphasizes the way it eases enterprises' transition to the market, giving them time to adjust, while also permitting a political compromise between leaders more committed to planning and those urging market reform. In comparative terms, he articulates a convincing case against the big bang theory, put forward by many economists and implemented by many policymakers concerned with Eastern Europe and the former USSR, according to which structural economic reform must take place quickly, so that the necessary pain may be endured before the euphoria occasioned by the overthrow of state socialism has worn off. In China, Naughton reminds us, structural economic reform was gradual, relatively painless, and extremely effective.

But if *Growing Out of the Plan* makes contributions to our understanding of China's economic restructuring that are critical (in both senses), its analysis of the politics of reform is less than satisfying. The book does not problematize the relationship of economics and politics; instead, it sees policy as a relatively unmediated function of economic phenomena. This is not an entirely fair criticism, since Naughton's central objective is economic analysis, but inasmuch as he often makes a point of downplaying the role of politics, it is not entirely unfair. In rehearsing the old canard that politics distorts sensible economic policy (p. 20), he fails to appreciate what he surely knows: Politics—in this case, conflict between leaders more and less committed to structural reform—produced policy oscillations that, over the medium term, steered a middle course between stagnation, on the one hand, and inflation, mushrooming unemployment, and overheated development, on the other. In characterizing China's political institutions as "primitive" (without really explaining what he means [p. 20]), he elides a potentially fascinating debate with recent contributions by political scientists who trace the success of China's economic restructuring to a definite degree of institutional and political flexibility (e.g., Susan Shirk, *The Political Logic of Economic Reform in China*, 1993; Gordon White, *Riding the Tiger: The Politics of Reform in Post-Mao China*, 1993). Thus, he argues that overconcentration of power prevented key policy adaptations and compromises (p. 194), without asking himself why political centralization could not have precisely the opposite effect (as, for example, Shirk and White argue).

In sharp contrast, Yan Sun and Feng Chen believe that ideological and theoretical factors have been central to the restructuring of China's political economy. Sun offers a carefully wrought account of the Chinese debates over fascinating structural economic issues, such as socialist organization, management, ownership, distribution, and politics. Her overarching argument is that, because in China the conceptions of socialism and Marxism were stretched but never abandoned, structural reform has been less destabilizing, has had fewer pernicious side effects, and has had many more benefits than it did in Eastern Europe or the former USSR, where the break with socialism and Marxism was complete. In a book that is, as I shall soon argue, almost excessively objective in its approach, this passage—both stunning in its form and directness but rather typical of many serious Chinese intellectuals and social scientists—stands out:

> The plight of the Russian economy and of the masses after the collapse of the CPSU [Communist Party of the Soviet Union], in sharp contrast to the vibrancy of the Chinese economy and the increasing prosperity of Chinese citizens, has shown that China may not be better off, after all, with the dissolution of the CCP [Chinese Communist Party]. Even China's corruption looks mild compared with the mafia-ridden economy of post-Soviet Russia, because of the capacity of China's stronger state to control the scope and nature of corruption. . . . *Has the freedom of a few politicians to engage in open power rivalry and of a few intellectuals to engage in open discussions really signified the end of alienation and the restoration of human integrity in post-Communist Russia?* (pp. 268–9, emphasis added).

The primacy Sun gives to ideological and theoretical debates has two shortcomings, though. In a mirror image of Naughton, she inadequately problematizes the place and role of ideology within China's wider state, politics, and political economy. Her impressively filigreed expositions of the debates over fascinating issues refer to their economic and political contexts, but close analysis of the ways in which the debates were actually shaped by political conflicts and economic factors, or of their concrete effects on policy, are largely missing. Thus, China's ideological and theoretical reassessment of socialism appears important only because of the limits it set on restructuring, but no more. Perhaps, though, that is precisely her point. Second, Sun eschews taking a position on the fascinating and controversial issues she analyzes, in the way that a Marxist or an economic institutionalist would. The nature of socialist ownership or distribution, or the questions of exploitation and economic democracy under socialism, remain one cutting edge of theory of political economy in China and, as John Roemer has again recently shown (*A Future for Socialism*, 1994), the capitalist West as well. A potentially rich debate about them barely began during the Cultural Revolution before it was drowned under extremist politics. Sun misses a chance to reopen and engage it, contenting herself with a richly detailed but frustratingly dry exposition.

Like Sun, Feng Chen focuses on what he regards as the neglected role of ideology in the Chinese structural reforms. But his argument and its implications are poles apart from Sun's. Reviving a shopworn, simplistic distinction between what he terms fundamental and instrumental ideology, Chen argues that the Chinese leadership has clung to orthodox basic principles while departing progressively farther from them in practice. Where Sun sees the virtues of flexibility, Chen sees an untenable ideological gap that threatens to undermine the legitimacy and, therefore, the survival of the Chinese state.

Chen does a serviceable job of linking ideological debates to their economic contexts and their policy effects (though Gordon White's *Riding the Tiger* and Joseph Fewsmith's *Dilemmas of Reform in China*, 1994, offer more analytically and theoretically textured treatments). But *Economic Reform and Political Legitimacy* has serious conceptual, theoretical, and analytical problems. First, the concept of ideology is crude and unexamined; at one point it is even conflated with ideas, in the strange formulation "ideas/ideology" (p. 2). Second, Chen makes two functionalist assumptions: Ideology must be consistent internally and also with practice. Third, his argument pivots on the idealist assumption that legitimacy—another unexamined concept—depends on a "credible and functioning ideology" (p. 209). It is as if no one had ever produced the rich theoretical literatures on ideology (summarized so elegantly by Eagleton, *Ideology*, 1991), the anti-functionalist critiques of legitimacy (e.g., Therborn, *The Ideology of Power and the Power of Ideology*, 1980), or the arguments about hegemony as inherently conflictual (e.g., Laitin, *Hegemony and Culture: Politics and Religious Change among the Yoruba*, 1986). Even if legitimacy survives as a useful concept, it is not at all clear from Chen why it depends mainly on ideology rather than material factors. This is especially so for the Chinese case in the present period, when ideologies of all sorts have been discredited by both the political crises of the Maoist period and the Dengist period critique of theory and ideology. Even Sun, who believes in the importance of ideology, notes its declining importance in recent years (p. 258).

This quaintly idealist and functionalist theoretical terrain is bound to lead to serious analytical problems. For example, if Chen is right that "a total change of fundamental principles would shake the very foundations of the system" (p. 209), then how can it be that the Chinese Communist Party has continued to preside over the People's Republic of China almost two decades after it abandoned such basics as class struggle, while also subjecting Marxism to searching critique? It is far more likely that, as Sun would argue, the answer lies precisely in the fact that the state did part with some of its thornier fundamentals. The gap between them and practical principles may well have done more to resolve than to deepen the crisis of the state. But because Chen's analysis assumes what it would do better to demonstrate, it cannot raise this problem, which lies at the heart of his argument.

The Bosnian Muslims: Denial of a Nation. By Francine Friedman. Boulder, CO: Westview Press, 1996. 288p. $75.00 cloth, $21.95 paper.

Zachary Irwin, *Pennsylvania State University at Erie*

This is one of many recent works that places the current Yugoslav tragedy in a broader context. Friedman's account of the Bosnian Muslims is exceptionally valuable because it provides a careful history graced by an excellent grasp of Western sources. A corresponding command of Ottoman sources perhaps might settle the "artificial debate over the ethnogenesis of the Bosnian Muslims" (p. 160), for it is the obscurity of Bosnia's pre-Islamic past and the problem of religious conversion that have shaped a pretext for mutually exclusive Serbo-Croat claims in the area. Typically, Friedman's cautious presentation summarizes one aspect of the debate that has identified Islamic conversion with former adherents of the persecuted Bogomil heresy. She correctly observes that Islamicization in Bosnia took place slowly in the absence of countervailing Christian organization.

The narrative makes a good case for the profound effect of Ottoman rule. The *millet* system of self-governing religious communities reinforced religious, class, and urban-rural cleavages. Distinctions between non-Muslim serfs and free Muslim peasants seem to have been scarcely less than those among Muslim peasants, landlords, and urban dwellers. Landlords opposed efforts by the Porte to implement the modest protection of the peasants through the *tanzimat* reforms of Sultan Mahmud II (1808–39). After the 1878 occupation, Austrian efforts to promote a common "Bosnian" identity to counter outside Serbian influence came to little. What might have been a "united front" (p. 66) of Muslims to petition Austrian officials in 1900 concerned Muslim religious rights. Again, religion formed the leitmotif for activities of the interwar Yugoslav Muslim Organization (JMO) in resisting Serbo-Croat pressures for assimilation.

At crucial stages, Bosnian Muslim assertiveness remains a reaction to the serial conflicts of others, such as Serbs and Croats, Serbs and Austrians, and četniks and the Partisans. Friedman uses as evidence an abundance of census data to show increasing Muslim resistance to declare themselves as Serbs or Croats. The process appears fairly straightforward, yet whether Bosnian Muslims responded defensively or emulated the nationalism of others, they wished to be Muslims exclusively. It is disconcerting to read Friedman's observation that it is impossible to determine "precisely" when Muslims "began to consider themselves a nation rather than merely [!] a religious group" (p. 165). In view of the fact that a majority of Muslims remained faithful, if not fully observant, would most not scoff at the notion of nationality as a superior identity to religion? Is there any indication that any Muslims except communist intellectuals believed it so?

Friedman is arguably correct in asserting the centrality of the 1968 decision to recognize the "Moslems" as a "constituent people" (*narod*) of socialist Yugoslavia. As a *narod*, Muslim intellectuals took enthusiastically to the task of distinguishing their tradition, even to the point of elevating the distinctive attributes of their use of Serbo-Croatian. Yet, fieldwork in the early 1970s among Muslims by the U.S. anthropologist William Lockwood revealed the persistent use of the word *nacija* to identify peoples, and one's *nacija* was a question of religious identification (*European Moslems: Economy and Ethnicity in Western Bosnia*, 1975). Ineluctably, Friedman recognizes the "difficult" task of distinguishing the "growth of national identity" from the rise of transnational "religiosity." Deeply etched categories of thought defining the "religious" and "national" are difficult to shed.

Confusion about the Bosnian Muslims, which one seldom finds in former Yugoslavia, begs certain questions of ourselves. If we simply recognize the Muslims as a religious community, then do we fear the implication they are somehow less democratic, stable, or tolerant than if they are national? Are religious groups less worthy of sympathy or protection? Friedman's conclusions about "ethnonationalism" challenge us to confront the "fear throughout Europe that the conflict in Bosnia is the first salvo in the battle between Christians and Muslims" (p. 248), yet her observations about the implications of multiculturalism and its demise in Bosnia preserve the image of religion as an antithetical force to functional politics. A view of Islam as politics no less than as religion is an inevitable component of Bosnia's future. The defection of the popular Muslim leader Haris Siladžić from Alija Izetbegović's Party of Democratic Action (SDA) may test whether an Islamic state may be Islamic without being monolithic or intolerant.

Friedman has made a signal contribution to the discussion of Bosnia's Muslims, yet we cannot avoid continuing reflection on some basic questions about the potential of Islamic politics. By entertaining an inverted relation between what we traditionally recognize as religious and political, it is easier to accept Bosnia's Muslims as an "imagined community" in Benedict Anderson's phrase, "personified before it can be seen, symbolized before it can be loved, imagined before it can be conceived" (*Imagined Communities: Reflections on the Origin and Spread of Nationalism*, 1991, p. 7). The wealth of such work should help us better discuss the past and the future of this unique Balkan people.

Hitler's Willing Executioners: Ordinary Germans and the Holocaust. By Daniel Jonah Goldhagen. New York: Alfred A. Knopf. 1996. 622p. $30.00.

Kristen R. Monroe, *University of California at Irvine*

The Holocaust, more than other political phenomena, should be approached with great care. Incalculable suffering and loss, too much blame to go around, and a legacy of grief all produce an outrage that slowly gives way to unquenchable tears. Grasping the full extent of the Holocaust requires a sensitivity beyond the range of normal human experience, and comment from those who did not live through it can seem foolhardy, even invasive. Indeed, as someone neither Jewish nor German, I am uneasy reviewing this book; I do so only because of a sense of obligation to the Germans I interviewed for *The Heart of Altruism* (1996), people who risked their lives to save Jews and who are ignored and condemned by Goldhagen's blanket indictment of the German people.

Goldhagen's central thesis is that the Germans possessed a world view so permeated by anti-Semitism that they were willing executioners for Hitler, not civilized people temporarily seduced by Hitler's rhetoric or coerced by Nazi terror, as is often claimed.

Goldhagen's historical analysis begins with a discussion of German anti-Semitism before Hitler, to demonstrate that virulent anti-Semitism was so embedded in the German psyche that Hitler's plan to eliminate the Jews was readily accepted, even relished, by the populace. His investigation is designed to demonstrate that the nineteenth-century extensions of freedoms to Jews were window dressing, a fragile facade masking deep-seated, continual, eliminationist anti-Semitism.

This analysis has three problems. First, Goldhagen provides no historical or comparative context by which his claims concerning a distinctly German world view can be evaluated. Second, he fails to convey the full spectrum of pre-Nazi German attitudes toward Jews. Historians have amply faulted Goldhagen for this, noting that our assessments must include, for example, both Treitschke's anti-Jewish vitriol *and* the public repudiation of Treitschke by Mommsen and other German historians. Third, describing the cognitive framework of an entire nation is difficult to justify conceptually and even harder to do. Goldhagen would need to consider the kind of mass data used by Siegfried Kracauer (*From Caligari to Hitler*, 1947) to trace the decline of bourgeois values through German films. Such data are not presented.

Goldhagen's empirical analysis focuses on Reserve Order police battalions, work camps, and death marches. Here, Goldhagen advances a two-part argument which, if substantiated, would indeed revise our thinking on the Holocaust: (1) these perpetrators were ordinary Germans, not Nazis, and (2) they are representative of German society as a whole.

Does Goldhagen make his case? Unfortunately, not. His quotations from survivors' records, legal testimony, personal letters, and official documents present extensive illustrations of the perpetrators' sadism, numbing in their detail and enormity. These fully support Goldhagen's claim that the perpetrators were not only cold-blooded technocrats removed from the carnage but also included genocidal madmen who enjoyed inflicting suffering on people they considered subhuman. But Goldhagen is less attentive to more systematic consideration of material critical to his central argument. There is some fragmentary presentation of data on how many of the Reserve Order policemen were members of the Nazi party, for example, but little or no systematic examination of any data that might attest to the extent to which work camps or death marches were German, as opposed to Nazi, efforts. (There is discussion of one death march in which participants were killed despite Himmler's order not to do so, an order motivated not by mercy but by political expedience.)

The empirical data presented here do not justify the retributive conclusion that "a demonological anti-Semitism, of the virulent racial variety, was the common structure of the perpetrators' cognition and of German society in general" (p. 392). And even if we grant that Goldhagen has proven that the perpetrators were anti-Semitic, this is not new information. Both scholarly and popular wisdom have long acknowledged the central role played by anti-Semitism in the Holocaust. What is crucial to Goldhagen's argument is establishing the link between perpetrators and German society as a whole.

Goldhagen fails to make this link because he fails to put forth clearly established criteria of evidence. To consider seriously his charge that it was German, not just Nazi, anti-Semitism that caused the Holocaust, what kinds of data should we examine? Goldhagen argues that the conclusions drawn from the acts of ordinary Germans in the police battalions "can, indeed must be, generalized to the German people *in general*. What these *ordinary* Germans did also could have been expected of other *ordinary* Germans" (p. 402, emphasis in original). But do instances of sadism constitute the relevant data? Might one not just as easily make the counterargument that we should generalize from those Germans who risked their lives to rescue Jews? Why is one group more illustrative than others of the entire society? In fact, probably neither rescuers nor perpetrators constitute the most representative sample of the German people. The failure to present a convincing argument for generalizing from one, as opposed to another, group of individuals is the fatal flaw in Goldhagen's study.

The polemical aspect of Goldhagen's work is illustrated by its strange silence on instances of humane treatment by Germans. On pages 348–9, for example, Goldhagen describes the Helmbrechts death march and cites as three instances of German cruelty the guards' refusal to let the Jews have food or shelter offered them by villagers. But according to the map Goldhagen provides (p. 347), these villagers appear to be in Germany. The origin of the villagers, or their kindness, is not commented on.

Goldhagen ignores such "data" that do not fit his thesis. In his final chapter, he argues: "At no point during the Nazi period did significant portions, or even identifiable minorities, of the German people express either dissent from the dominant elaboration of the nature of Jews or principled disapproval of the eliminationist goals and measures that the German government and so many Germans pursued" (p. 430). Goldhagen continues with an impassioned plea to know "where is the evidence" that anyone spoke up for the Jews. There is a great deal of this evidence; Goldhagen simply ignores it.

Goldhagen also ignores different interpretations of the same data analyzed previously by other scholars. Christopher Browning's important work (*Ordinary Men: Reserve Police Battalion 101 and the Final Solution in Poland*, 1992), for example, is relegated to a few footnotes, even though Browning considered many of the same data and offered a quite different and, to my mind, more comprehensive explanation stressing the interaction of wartime brutalization, anti-Semitism, German fear of encirclement by foreigners, and Nazi manipulation of these fears.

Goldhagen's outrage at what was done to the Jews and his chilling illustrations of sadistic cruelty that served no purpose other than the Jews' humiliation and death are perhaps the book's most striking feature and invoke precisely the kind of disgust and revulsion they should. This is a valuable contribution, reminding us exactly what genocide is: the killing of innocent people, not for acts they have committed but simply because of their accident of birth. It is not rational. It serves no purpose. It is morally reprehensible, wanton cruelty at its very worst. That the Nazis engaged in genocide is abominable and shameful, all the things that Goldhagen wants us to feel through his extensive use of detailed descriptions and documentation. But it is not news.

Had Goldhagen presented more systematic evidence that genocide was engaged in by ordinary Germans, not just Nazis or people intimidated or terrorized by the Nazis—and Goldhagen vastly underestimates the effect of Nazi brutality and the climate of fear instilled in the populace—had he suggested why the perpetrators best reflect the German people, or asked whether Germany was more pervasively anti-Semitic than other societies in Europe, he would have made a stronger case for his argument. Sadly, ironically, by condemning the German people en masse, Goldhagen comes perilously close to the same kind of cultural and ethnic stereotyping that still leads to anti-Semitism, and which Goldhagen so passionately and rightly deplores.

Democratization in Russia: The Development of Legislative Institutions. Edited by Jeffrey W. Hahn. Armonk, NY: M.E. Sharpe, Inc., 1996. 303p. $63.95 cloth, $24.95 paper.

Terry D. Clark, *Creighton University*

Numerous appeals have been made for scholars of Russia to break out of their isolation from political science by thinking theoretically in the post-Soviet era (e.g., see Alexander J. Motyl, *Thinking Theoretically about Soviet Nationalities*, 1992; Frederic J. Fleron, Jr., and Erik P. Hoffmann, eds., *Methodology and Empirical Theory in Sovietology*, 1993). *Democratization in Russia* is an effort to respond to these calls. Starting with the premise that legislatures are central to democracies, the book's essays focus on whether the experience of the first Russian legislature in the post-Soviet period demonstrates that the country was becoming more democratic. In the volume's first chapter, Hahn derives several criteria from the theoretical literature on democratic consolidation and the new institutionalism by which to judge the degree to which Russian legislatures (at both the national and local level) reflected growing democratization. Among them are the rules by which representatives are chosen, linkages between representatives and those they represent, the policy role of the legislature, the legislature's degree of autonomy, the complexity of internal organization, and continuity of legislative norms.

The book is divided into three parts. Part I focuses on the two-tiered national legislature from summer 1990 to its forceful dissolution by Boris Yeltsin in October 1993. Given the violent end of the "First Republic," Hahn contends that a study of the period is useful, as it presents us with an opportunity to consider how a pathological case may contribute to theory. Instead, however, the essays analyze how theory illuminates our understanding of the pathological case itself. Nonetheless, the strength of the contributions in this section is the implicit, if not explicit, link with theories of democratic consolidation.

Two of the essays in Part I focus on executive-legislative relations. Thomas F. Remington argues that the creation of the presidency led to an exodus of legislators from the parliament to the executive, thereby upsetting the balance of forces in the former and leading to irreconcilable conflict between the two. Lilia Shevtsova similarly identifies the inability of political elites to engage in negotiating pacts as a major characteristic of the First Republic, the most obvious case being Boris Yeltsin's violent abolition of the first legislature and its replacement with a constitutionally weaker legislature in late 1993. Peculiarly, however, she concludes that democracy nonetheless may not be in as great a danger as theory might suggest, owing to the emergence of a civil society and the growing devolution of power to the localities and private capital interests. Such a conclusion presents an opportunity to consider the implications of the Russian case for theory, particularly given the theoretical premise that democracy requires a factious and relatively evenly divided political elite (e.g., see Adam Przeworski, *Democracy and the Market*, 1991).

The remaining two essays in Part I shift the focus from executive-legislative relations to the representative function of the legislature. Both argue that the First Republic (1990 to 1993) was characterized by weak ties between legislators and those they represented. The theoretical implication, as developed by Hahn in the introductory chapter, is that democratic consolidation lagged. Timothy J. Colton locates the problem in the relatively little importance Russian deputies attached to the representative function in comparison to policymaking. Jerry F. Hough attributes the problem to structural causes. Noting the weak Russian party system, he identifies legislative rules encouraging multiple factions as the primary cause of the phenomenon.

Part II turns from a consideration of the national legislature during the First Republic to analysis of local governments during the same period. If the contributions in Part I possess a clear tie with theory (implicitly or explicitly), those in Part II generally do not. With the exception of Hahn's essay analyzing the experience of the local legislature of the City of Yaroslavl, the pieces do not directly address the theoretical propositions developed in the introductory chapter. Blair A. Ruble contributes an explicitly descriptive case study of the effect of central government policies on urban planning in Yaroslavl. From a comparison of local government in two Russian cities, Kathryn Stoner-Weiss argues that economic context influenced the degree to which consensual government was achievable at the local level. Where economies were more highly diversified, local elites were prone to splinter into conflicting factions.

This is not to say that the essays have no theoretical relevance. Indeed, quite the opposite is true. Stoner-Weiss's piece, for instance, seemingly argues against the theoretical proposition that, to the degree that democracy requires divided elites, conflictual governance is to be preferred to consensual. It would be interesting to know why the opposite holds in the Russian case. Under what circumstances is one preferable to the other?

Virtually all edited volumes suffer from the tendency of the essays to stray from the theoretical framework established by the editor. The preferred solution for most is to conclude with an essay that seeks to place each contribution more explicitly into a theoretical perspective. Hahn adopts this approach. In the last section he attempts to fit each contribution into the original democratic consolidation and institutional framework. Furthermore, he addresses how the First Republic as a pathological case contributes to our understanding of the role of legislatures in consolidating democracies.

In the concluding section's other essay, Michael L. Mezey appeals to both the reader and the volume's contributors not only to be guided by theory but also, and just as important, to be comparative. For some, this volume will undoubtedly fail on both counts. The preference for contextual richness over theory building and for lists of propositions over clear and coherent discussions of theory will seem sufficient reason not to give the book serious consideration.

I would argue that, despite the inevitable shortcomings in any book, this one has much to offer. While individual essays largely consider how theory illuminates our understanding of the Russian case or only indirectly address theory, taken as a whole the volume makes a clear contribution to our theoretical understanding of legislatures, not only in the post-Soviet context but also in transitioning and consolidating political systems. Indeed, the book's essays (and Hahn's concluding summary in particular) make the case that the legislative experience of the First Republic confirms much of theory. Even in the pathological Russian case, institutional arrangements constrained human volition and limited outcomes. Indeed, the Russian failure to strengthen the representative function of the legislature and balance executive-legislative relations greatly detracted from the assembly's ability to contribute to the consolidation of democracy. Finally, within the constraints provided by rules and institutions, decisions made by strategic actors were critical to outcomes. The unwillingness of salient political elites to engage in negotiated "pacts" is particularly evident in this regard.

The Legacy of the French Revolution. Edited by Ralph C. Hancock and L. Gary Lambert. Lanham, MD: Rowman and Littlefield Publishers, Inc., 1996. 308p. $67.50 cloth, $24.95 paper.

John A. Rohr, *Virginia Polytechnic Institute*

A certain uneasiness takes hold of me as I review this book, for which a far more extensive and searching review is already in print. It appears as the tenth and final essay in this very volume, under the title "Conclusion: Two Revolutions and the Problems of Modern Prudence," by Ralph C. Hancock, one of the editors. In his conclusion, Hancock compares, contrasts, and critiques the preceding nine essays with earnest rigor and impressive insight.

Unlike most edited volumes, this one adheres faithfully to the theme announced in its title. All the essays, save one, compare various aspects of the French Revolution to its American predecessor, an element of the book that will make it of particular interest to readers in the United States.

The book's coherence is remarkable because the essays come from various times and places as well as from sundry books and journals. An extensive introduction explains the plausible connections the editors find among the essays and helps to shape the disparate emphases of the several authors into a meaningful whole. This coherence is reinforced by Hancock's concluding essay, which, as suggested above, is more a summary and evaluation than a genuine conclusion.

The three parts of the book deal, first, with the idea of revolution as such, then with specific philosophical aspects of the French and American revolutions, and finally with the legal and constitutional consequences of these two great events. Thus, the overall movement of the essays from general to specific offers something of interest to a broad range of professional readers, including theorists, historians, comparativists, and jurists.

For this reviewer, the high points of the book are (1) Jeremy Rabkin's ambitious effort to link the salience of the judiciary in the United States and administrative centralization in France to the revolutions in the two countries and ultimately to the political philosophies of Locke and Rousseau; (2) Terence Marshall's spirited defense of the classical virtues of prudence, deliberation, and moderate politics in the face of the uncritical contemporary acceptance of Rousseau's notion of compassion and Kant's separation of principled judgment from prudence; and (3) the fascinating tale told by C. Bradley Thompson of the competing influences of John Adams and Benjamin Franklin on the French revolutionaries as they debated the relative merits of unicameral and bicameral legislatures.

As noted above, all the essays but one have something to say about the American Revolution. The lone holdout is Philippe Bénéton, whose essay is surely the most intriguing of all. He squarely addresses the familiar but still haunting question of how the French Revolution, which began by "proclaiming the first principles of liberal democracy" (p. 175), collapsed within a few short years into the Reign of Terror. For Bénéton, "the troubling issue is this: Are liberalism or democracy or both implicated in the crimes of the Terror" (p. 175)? He tries to save liberal democracy by centering his essay on a complex argument rooted in a distinction between *universalist* and *ideological* principles, the former alone being authentically liberal and democratic, and the latter subverting both liberalism and democracy. In the concluding essay, Hancock offers some disturbingly strong arguments to challenge Bénéton's efforts and thereby reinstates the unsettling urgency of the original question.

The book's most serious flaw is its insufficient attention to the effect of the institutions of the Fifth Republic on the legacy of the French Revolution. Not surprisingly, nearly all the authors mention the origins and the unhappy results of parliamentary sovereignty, but Philippe Reynaud alone touches upon (albeit rather lightly) the limitations placed on this sovereignty by such important Fifth Republic innovations as the 1962 constitutional amendment that led to the direct election of the president, the increasingly important role played by the Constitutional Council, and the *enumeration* (and therefore the limitation) of legislative power, with all residual powers reserved to the regulatory domain of the government. Clearly, the tradition of parliamentary sovereignty cannot be reconciled with the Constitution of the Fifth Republic. This incompatibility should have played a far more prominent role in a book that examines the légacy of the French Revolution, especially since the contributors to it showed no hesitation in applying the lessons of that revolution to contemporary problems in France and the United States. The constitutional origins and development of the Fifth Republic cry out for a serious analysis of their effect upon the legacy of the French Revolution. Do they reinforce, ignore, or betray it?

Votes and Budgets: Comparative Studies in Accountable Governance in the South. Edited by John Healey and William Tordoff. New York: St. Martin's Press; London: MacMillan Press, 1995. 270p. $59.95.

Goran Hyden, *University of Florida*

As the title indicates, this book is concerned with the relationship between democratic politics, on the one hand, and accountable governance, on the other. The broader research question that the contributors address is whether the presence of free and competitive politics makes a difference in public-expenditure management in developing countries. The dependent variable is conceived as having two competing dimensions: political responsiveness and public responsibility. The political leadership must respond to the needs and demands of voters, but it also must guard the national interest, which often may call for a different policy than that preferred by the public. Accountable governance, therefore, amounts to the proper balancing of these two dimensions.

This project was conducted under the auspices of the Overseas Development Institute in London, a quasi-private think tank devoted to research and consulting work in developing countries. John Healey is a research fellow at the institute, and William Tordoff is a professor emeritus of government at the University of Manchester, an institution from which a few other contributors also come. The empirical data were collected in four British Commonwealth countries: Botswana, Jamaica, Sri Lanka, and Zambia. Additional comparative material used in the volume comes from Ghana and Tanzania and from South Korea and Taiwan. There is no control group of countries in the study, but the political evolution of Sri Lanka and Zambia, where autocratic rule has prevailed for part of the last 35 years, provides scope for some comparison of what difference democracy makes to governance. From examining the four main cases, and considering the material gathered from the other four, the book arrives at two principal conclusions. The first is that accountable governance is met more effectively in periods when democracy rather than autocracy prevails. The second is that fair, multiparty electoral conditions enhance political responsibility more than it does public accountability. In short, public sector management is not necessarily strengthened by the conditions of free and competitive politics, a finding that Healey in his concluding chapter explains with reference to the absence of institutionalized norms of democratic behavior.

Against the background of a worldwide trend toward democracy in recent years, there understandably has been a growing interest in questions relating to regime transition. The majority of studies focusing on this set of issues treat democracy as the dependent variable and concentrate on finding explanations for success or failure in such transitions. This volume is of interest to scholars because it treats conditions of democracy, notably fair and competitive politics, as explanatory variables. It is also valuable because it is a serious attempt to operationalize the concept of governance in ways that make it measurable. Yet another positive aspect of this volume is that it is comparative not only within one of the established areas, such as Africa or Asia, but also across these boundaries, demonstrating that meaningful and interesting comparisons can be made between countries in different regions of the world.

The question still arises of how well the editors and contributors have carried out their comparative study. To start with, this volume suffers from the usual shortcomings associated with edited, multiperson book projects. It is always difficult to design such a study in ways that make the project as satisfactory as those done by a single person. The design in this project is inductive and generally quite loose. This should not overshadow the fact that the contributors largely stay on course, providing the expected data and addressing a similar set of analytical concerns. A real strength of the volume is the quality of the four case studies. Even though they rely more on secondary than primary data, they provide it in a systematic fashion and often with considerable attention to detail Tordoff's longitudinal comparisons of accountable governance in both Africa and Asia, which follows these cases studies, is informative and a joy to read, but unfortunately it stands too much on its own. Its value to the overall objective of the volume, therefore, is largely lost. Another weakness of the book is that it is not clear on what grounds the four principal cases were chosen. The reader is left to guess whether they were selected because they are basically similar—all Commonwealth countries with a legacy of British colonial rule—or because they are different. If the former, the variables that can be held constant are still relatively few, and the extent to which the democracy variables really explain accountable governance is in doubt. If the latter, the reader has trouble knowing how the similarities, for example, in political responsiveness, can be explained.

The overall impression is that the book brings to attention in a systematic fashion a set of issues which remain to be more fully explored. As such, it is an original contribution that will prove valuable to students of democratization and its implications for better governance. The conclusions rest on rather shaky grounds, although they largely confirm what seems to be intuitively right. For this reason, the volume certainly is worth reading with a view to understanding what requires further study in this area.

Constitutional Policy and Change in Europe. Edited by Joachim Jens Hesse and Nevil Johnson. Oxford: Oxford University Press, 1995. 403p. $59.00.

Michel Rosenfeld, *Yeshiva University*

Europe is at the center of a veritable proliferation of major constitutional developments which began after World War II and have vastly accelerated since the collapse of the communist regimes of Eastern Europe. Moreover, because of the wide variety of problems it has confronted and the great diversity of constitutional solutions it has implemented, Europe has become the most promising living laboratory for evaluating the recent worldwide trend toward constitutionalism—broadly defined as commitment to limited but effective government, adherence to the rule of law, and adequate protection of fundamental rights. In particular, focus on Europe affords a privileged vantage point for examining the nature and function of contemporary constitutions, assessing the extent to which core constitutional norms may be deemed universal, and evaluating the potential for transplantations of constitutional arrangements from one country to another and for their successful spread beyond the nation-state.

Constitutional Policy and Change in Europe, a collection of essays that originated at an international conference held at the Center for European Studies, Nuffield College, Oxford, is a welcome addition to the vast recent literature on contemporary constitutional developments and trends. Other collections also combine contributions by prominent legal and political theorists, and other works focus more thoroughly on broad theoretical issues or detailed historical narratives. The present collection nevertheless deserves special praise for the sharpness with which it zeroes in on the pressing and troubling constitutional issues of the day. By concentrating on the influence of Western European constitutional practices on Eastern and Central Europe, and on often less than apparent parallels and points of contact between Eastern and Western Europe, the collection affords a generally crisp insight into the evolution and adaptability of constitutional concepts and practices as well as into their suitability for purposes of supranational, national, and regional integration.

The contributions in the collection variously address changes in the conception of constitutionalism, the evolution of different constitutional systems or traditions, the major constitutional trends and shifts in significant Western and Eastern European countries, and the conceptual and practical difficulties surrounding efforts to constitutionalize certain relationships in the context of the European Union.

It is remarkable how little direct influence U.S. constitutionalism has had on recent developments in Europe. This is due, in part, to European rejection of the liberal procedural conception of constitutionalism prevalent in the United States and, in part, to the contrast between the status and functioning of common law judges and those of their civil law counterparts. As clearly outlined in the essay by Donald Kommer and W. J. Thompson, the liberal procedural conception of constitutionalism evinces a strong preference for the state as guarantor of fundamental rights rather than as principal engine for realization of the common good. Accordingly, one might expect the liberal tradition to predominate in European countries bent on distancing themselves from an authoritarian past. This has not occurred, however, for, as Nevil Johnson stresses in his essay, Europeans embrace a more teleological conception of constitutionalism owing to their stronger commitment to community and deeper commitment to state-promoted welfare.

Yet, America's lack of influence regarding judicial review is paradoxically ultimately attributable to a convergence between common law and civil law adjudication rather than the obvious divergences among them. This conclusion emerges from various essays in the collection especially from Cheryl Saunder's succinct and incisive account of the evolution of the British constitutional system. Indeed, although mistrust of judges and lack of judicial independence run deep in the traditions of many European countries, recent disenchantment with politicians and the emergence of strong, specialized, and politically independent constitutional courts have strongly boosted judicial enforcement of constitutional norms. As a matter of fact, some European constitutional courts, such as the German and the Hungarian, enjoy sweeping powers that go well beyond those of the U.S. Supreme Court.

The various essays in the collection range from primarily descriptive accounts of particular constitutional changes to rather ambitious explorations of larger theoretical issues. Among the latter, two essays deserve special mention because of the breadth and depth of their insights. The first is Ulrich Preuss's essay on constitutional evolution in Eastern Europe, in which he successfully blends his consummate skills as a constitutional lawyer and political theorist to pinpoint the central issues arising out of the recent constitutional journey undertaken in Eastern Europe and to draw out their larger implications for constitutionalism in general. Preuss uses those recent experiences to stress the close relation between constitutional politics and the consolidation of national identity in multiethnic polities. He also provides an illuminating account of certain East European countries' resort to constitutionalization of particular conceptions of citizenship to deprive certain ethnic groups within their borders of full political membership.

The second essay noteworthy because of its theoretical scope is that of Dieter Grimm on constitutional reform in Germany. Grimm, a judge on Germany's Constitutional Court, not only provides an acute analysis of the constitutional dimensions of German unification but also draws on recent events in Germany to map out a persuasive account of the relationship among constitutionalism, politics, and democracy. Furthermore, Grimm sheds interesting new light on the prospects for the success of constitutionalism within the ambit of the European Union, given the "democratic deficit" associated with its current governing structure. Contrary to the prevailing belief that the problem could be solved by a shift in power toward the European Parliament, Grimm concludes that the union's lack of a common language—and hence of a unified media capable of molding a commonly grounded public opinion—makes it unlikely that it will reach a level of democracy commensurate with genuine constitutionalism.

Two other essays, each in its own way, serve to complement Grimm's. Dusan Hendrych describes the use of constitutional means to dissolve Czechoslovakia into two separate constitutional republics and addresses issues arising out of such dissolution. While his essay lacks Grimm's theoretical scope, it makes for interesting comparisons between constitutional integration and disaggregation. For its part, the essay by Gunnar Folke Schuppert on the prospects for a European constitution furnishes a useful summary of various positions on the issue and thus further situates Grimm's views within the ongoing debate.

The remaining essays are generally informative, but a couple of them leave something to be desired. This is either because of a failure to convey the import of the constitutional changes discussed to a reader unfamiliar with the tradition involved or because of an overly schematic narrative with unclear theoretical implications. Moreover, although the discussion of individual Eastern European countries, which concentrates on former Czechoslovakia, Hungary, and Poland, is quite good, the collection undoubtedly could have benefited from a discussion of other countries with quite different constitutional problems, such as Russia, Bulgaria, and Romania. In the last analysis, however, these shortcomings are relatively minor, and the collection should prove useful to both legal scholars and political theorists.

Courts, Law, and Politics in Comparative Perspective. By Herbert Jacob, Erhard Blankenburg, Herbert M. Kritzer, Doris Marie Provine, and Joseph Sanders. New Haven and London: Yale University Press, 1996. 408p. $45.00 cloth, $20.00 paper.

Martin Shapiro, *University of California, Berkeley*

Published shortly before Herbert Jacob's death, this book could well serve as a memorial to his enormous achievements. It will allow those who have offered a general course on U.S. courts based on Jacob's *Justice in America* (1992) or comparable materials to add a comparative dimension or a companion course. In this volume there is an introduction, conclusion, and a chapter summarizing his own work and that of others on U.S. Courts by Jacob. Chapters on England, France, Germany, and Japan by well-known authorities do not slight constitutional law and courts but also provide basic coverage of criminal, public law, and private law litigation, including careful considerations of legal professions and legal cultures. Clearly, great efforts have been made to maintain a parallelism across the several national presentations. The approach is appropriate for either a straight political science or a "law and society" course.

This book is basically a text but not a dumbed-down one. It incorporates the latest research concerns. It wears its sophistication lightly, but it is not a series of mere static descriptions of court organizations, personnel, and procedures. Both law and politics are taken seriously. Courts and law are treated neither as wholly autonomous phenomena nor as only the creatures of broader political forces. In the current jargon, law and courts are treated as both constituted and constituting. Teachers who want to use the book as a kind of institutional database for their own analysis may do so, but it can also be used as a relatively free-standing view of the relations among law, courts, and politics.

Although excellent at the level of description and innovative in the range of subject matter considered, the book does not offer many original insights, but there are some. Kritzer provides a particularly sensitive treatment of the vexing question of whether the British courts are becoming more activist vis-à-vis administrative agencies and tribunals. The whole volume usefully spotlights judicial review of the lawfulness of administrative action alongside our traditional concern for constitutional judicial review, treating both as central to judicial policymaking. The heavy emphasis on lawyers will help push political science in a useful direction, although all the contributors are much better about viewing lawyers in the criminal trial process than about assessing their crucial functions in judicial policymaking. While valiantly sticking to the parallel and essentially descriptive tasks of the other chapters, Blankenburg cannot resist some of the generally social democratic critical perspectives on German law and courts of which he is a past master and which are particularly relevant in the context of reunification. Provine is excellent at tracking both the pro- and antijudicial activism themes that vie with one another in contemporary France.

The chapter on Japan is peculiar. Following the current literature, which he fully commands, Sanders gets beyond the simplism that the Japanese do not litigate because of a deep cultural commitment to harmony and takes the position that courts do little as a result of deliberately avoiding litigation regarding government policies. Yet, ultimately the chapter is "reorientalized" as a hymn to a Japanese culture and structure of compromise, consensus, trust, and stability. Like the old community study of Tijuana that missed the whore houses, Sanders gives us a low crime rate without the Yakusa, a nonlitigational auto insurance claims processing system without the cartelized insurance industry colluding to undercompensate, and industrial relations without the raw government-backed economic coercion of the dozen dominant conglomerates. If this is a law and politics study, it is politics without power. For once a touch of critical legal studies would have done some good. Interesting things about Japanese law and politics will be written when we learn to begin from the premise that Japan is a rightist, corporatist state.

The importance of the European Court of Human Rights and the European Court of Justice is a leitmotif running through all the European chapters. By hindsight we can see that an additional chapter on those courts would have been a good thing, and course instructors would be well advised to construct one on their own. Indeed, because of the distinctive post–World War II growth of national and transnational constitutional judicial review in Western (and now Eastern) Europe, law and courts and comparative politics teachers ought to consider a comparative courts or comparative constitutional law of Europe course as an alternative to one with a worldwide scope. Besides this book, a number of recent journal symposia, books, and book chapters have recently appeared (and a number of others will appear in the next couple of years) that, for the first time, allow for a richly detailed and relatively integrated European course concentrating on constitutional and administrative judicial review.

In my view, all the contributors pay too much attention to criminal law and too little to government regulation, but all the chapters do restore regulation to a central place in political science, a place that it surely should regain in an age so fascinated with "free" (i.e., regulated) markets. The volume's inclination to treat civil litigation essentially as litigation by small parties, and then to lump together litigation in which either government or corporations or both are parties, has its up and down sides. Instructors will have to pay special attention to sorting out such issues as repeat versus one-time players as well as the conventional European boundaries between public and private law, which tend to be obscured or confused by this kind of lumping. The absence of a bibliography is no doubt due to cost cutting, but one is awfully useful in a course text. The footnotes make up in part.

Gendering the Middle East: Emerging Perspectives. Edited by Deniz Kandiyoti. Syracuse, NY: Syracuse University Press, 1996. 177p. $39.95 cloth, $16.95 paper.

As'ad AbuKhalil, *California State University, Stanislaus*

In the products of the culture industry in the West, Middle Eastern women remain passive, subservient, and changeless. Their oppression and exclusion are assumed, not researched or documented. They sometimes serve as a backdrop to the stereotypical depiction of the region, but their voices, like those of the Persian-speaking women of the movie *Not Without My Daughter* are considered irrelevant. Fortunately, women of the Middle East are no longer invisible in the scholarly production of the West, although their stories are often told indirectly, mostly through the accounts of anthropologists who interpret their worlds and words to Western readers. Many of the studies of Middle East gender issues have been conducted by anthropologists, sociologists, and historians. Political scientists of the Middle East, with the notable exception of Mirvat Hatem of Howard University, have ignored issues of gender, perhaps believing that their field is too serious to tackle a subject which only recently has been partially incorporated into political science curricula.

This book contains a variety of articles with only the issues of Middle East gender in common. The book, however, should be titled "English Language Gendering of Parts of the Middle East," because the rich literature on gender in Arabic, Persian, Turkish, French, and Hebrew is largely ignored. That is the major weakness of this otherwise informative and useful book. Nowhere is this weakness more apparent than in the fine introduction by the editor, an anthropologist by training. Kandiyoti attempts to draw a general and thematic picture of the state of gender in Middle East studies but fails to incorporate the significant body of literature in "local languages." She merely mentions this limitation in an endnote (p. 23) without realizing the effect of this shortcoming. She, for example, maintains that there has been "little emphasis on sexuality" (p. 14) in gender studies of the region, which is only true of English-language works. People such as Bouhdiba, Al-'Adhm, Zay'ur, Tarabishi, and Yasin all dealt with issues of sexuality in their studies of gender in French and Arabic.

Kandiyoti also maintains that the debate about the "compatibility of Islam with women's emancipation" (p. 9) remains on the agenda without paying attention to Lebanese feminist writings in Arabic which have been ignoring this question altogether. Yet, the author succeeds in categorizing the various phases in the relatively recent effort of Western writing on gender in the Middle East. She is correct in characterizing the incorporation of feminist scholarship into Middle Eastern studies as "partial" and "selective." With the exception of the field of anthropology, gender issues remain confined to their respective experts, mostly women. This is reinforced by demand in the academic market, as limited as it is, which emphasizes the issues of empires and wars. Kandiyoti also succeeds in producing a volume free of the burdens of the classical Orientalist paradigm without presenting a polemical endorsement of Middle Eastern actualities.

The book contains two full chapters on feminism and Iranian studies. Paidar writes a superb piece, "Feminism and Islam in Iran," and weaves into a brief chapter a full century of events and movements. One learns of the ways in which the state, Pahlavi or Khomeinist, still mainpulates and exploits issues of women. Fortunately, she does not satisfy the appetite of Western readers for stereotypes about veiling and segregation. This volume prefers social, economic, and political explanations instead of the exhaustively overused method of Islamic reductionism, or what Maxime Rodinson calls "theologocentrism."

The rest of the book deals exclusively with Israeli and Palestinian women, thereby excluding most of the people and countries of the region. This is perhaps due to the wide interest in anything dealing with the Arab-Israeli conflict. The chapter by Moors, "Gender Relations and Inheritance," is informative for the study of inheritance in general. She, however, wrongly assumes that inheritance laws are universal among all Muslims (p. 70). In reality, Shi'ite laws on inheritance are more favorable to daughters than are Sunni laws, especially when the father has no sons. To her credit, however, she qualifies her discussion of inheritance with references to class differences.

Katz's chapter offers an interesting deconstruction of Palestinian and Israeli national discourses. She persuasively shows how the nation often is used to subjugate women and how notions of masculinity—crudely strong among warring groups and nations—are defined in terms of distancing the self from "feminine" essentialism. Katz, however, tries to maintain an impartial stand on the highly charged emotional issues of the Arab-Israeli conflict, but she mentions Arab violent "attacks" (p. 99) as if the Zionist struggle has been pacifist in practice. She draws uniquely on Arabic and Hebrew works but manages to portray Palestinian women as passive. No mention is made of early political struggle, including demands for emancipation, by Palestinian women. The tone in general does not serve her apparent aim of dealing sensitively and even-handedly with the subject. She fails in this regard while all the other contributors avoided making political choices among combatants.

Sharoni's contribution is a rare feminist critique of the practice of Middle Eastern politics. She presents a devastating picture of Rabin and uses his own words to illustrate the sexism of international relations. She approvingly cites the wise words of Israeli feminist Erella Shadmi, who—almost as a wake-up call to political scientists—defines her interests as follows: "Oppression *per se* is my concern—whether Palestinians, women, children, Sephardic Jews, blacks or any other creature on earth. Moreover, I am also very much concerned with fundamentalism, environmental degradation, capitalism, compulsory heterosexuality and other ideologies which attempt to limit my liberty and free choice" (pp. 117–8).

Finally, Sayigh, widely known from her two books on Palestinians in Lebanon, makes an important contribution to the study of intersubjectivity, "understood as the problem of the relationship between the researcher and the research community" (p. 145).

With these obviously important and widely diverse topics and interests, how could the exclusion of gender issues from Middle Eastern political science continue?

Actively Seeking Work? The Politics of Unemployment and Welfare Policy in the United States and Great Britain.

By Desmond King. Chicago: The University of Chicago Press, 1995. 331p. $50.00 cloth, $17.95 paper.

Sanford F. Schram, *University of Hawaii at Manoa*

For years, many welfare policy analysts bemoaned the inability of the United States to develop a European welfare state. Countries such as England were held up as models that the United States should emulate. European welfare states were more universalistic in their coverage of the population and more comprehensive in the protection afforded. The United States had a fragmented system that divided federal entitlements into (1) federal insurance for retirees; (2) substantially federally supported state-run insurance for unemployed workers; and (3) weakly federally supported state public assistance for those whose lack of a relationship to the labor market disqualified them for insurance programs. The federal entitlement to welfare was limited to Aid to Families with Dependent Children (AFDC), which allowed states to set their own benefit levels. In recent years, some scholars have theorized that the major differences among state structures was the main explanation for the disjuncture between the United States and European countries. "Historical institutionalism," as it has been dubbed, as articulated by Theda Skocpol and others, has helped us understand why the U.S. state system has not developed into a European welfare state.

It seems that the gap between the United States and Europe is wider than ever, especially since the fragmented U.S. system has become even more tiered than before. In 1996 President Clinton was forced to keep his promise to "end welfare as we know it" and sign historic legislation ending the 61-year-old federal entitlement to public assistance. AFDC was turned into a block grant program, Temporary Assistance to Needy Families (TANF), administered by the states under a variety of restrictions, including not providing aid for more than five years in a person's lifetime and for no more than two years before the recipient goes to work.

Desmond King's richly detailed, theoretically nuanced account of the historical development of what he calls "work-welfare" programs in both Great Britain and the United States was completed just before the push to end welfare. Nonetheless, its prescience in focusing on work-welfare is matched by its compelling perspective, which helps make these dramatic developments understandable. King's historical account is sandwiched between short introductory and concluding chapters. The introduction articulates the book's provocative thesis, and the conclusion raises important theoretical issues for the study of the welfare state. King's focus is on work-welfare, but his conclusions have much to say about questioning the "historical institutionalist" perspective. It provides much evidence, in particular, for understanding why conservative reform of welfare under both Reagan and Thatcher succeeded, whereas liberal reforms that preceded these efforts failed. In the process, *Actively Seeking Work*? convincingly demonstrates its main thesis, namely, that the differences between the United States and at least Great Britain have been exaggerated.

This book provocatively suggests that previous scholarship needs to be addended in other ways to give greater emphasis to the role of politicians and political parties in influencing the development of social policy. It soberly reminds us that, regardless of state structures, political actors can only do so much when operating under pressure from other, often more powerful, people who are primarily interested in perpetuating the logic of their market economies and the values embedded in their cultural practices. If politicians succumb to placating those who, among other things, give priority to profits and prejudice, then they will not build an inclusive welfare state, whether their state structure is more or less open to such revision.

King defines work-welfare programs as denoting "three sorts of government policy for the unemployed: first, placement policies to marry jobseekers with vacancies; second, training schemes, intended to augment jobseekers' skills; and, third, workfare programs sometimes imposed upon job seekers as a condition of receiving benefits." His inclusion of the third could not be more timely. His meticulous narrative documenting the machinations of politicians, administrators, policy experts, and interest groups over the development of work-welfare in the course of the twentieth century is simply fascinating. It convinces this reader that the development of these programs in both countries was haunted by political constraints ensuring each country could only go so far in helping those who needed assistance before the imperatives of power associated with capital and culture, economics and value, imposed their limitations by way of wary political actors. As King documents, pressures for cheap labor and prejudice against African Americans undermined the development of serious training schemes in the United States. In Britain, the resistance was more class based. Yet, in both countries labor market policy could never be effectively joined to economic policy, and welfare guarantees became increasingly at risk when seen in the 1980s as undermining the "clearing price for wages" in a softening economy. By the 1990s, the logic of retrenchment had taken hold, and Britain's more developed set of protections underwent serious revision, making its system more like that of the United States, at least until the "end of welfare" became an American reality.

This book stresses detail at the expense of analysis. King makes a claim for his perspective that is too stark, and his differences with historical institutionalism begin to seem precious. While the detailed chapters end with passages which help build the momentum to the anticipated conclusion, the conclusion itself needs less nuance and more thorough extrapolation. King is right to emphasize similarities over differences. The differences, while perhaps explainable in terms of historical institutionalism, are small in comparison to the similarities, but these cry out for a political-economic explanatory perspective. Yet, King ends by only suggesting an addendum to historical institutionalism when his evidence suggests scraping it for a more critical reading of the limits of welfare in what passes for capitalist democracy in either England or the United States. The specter that haunts this book is the one that Jacques Derrida says haunts all of capitalism. It is the ghost of a universal welfare state now more evanescent than at any time in the post–World War II era.

To Dream of Dreams: Religious Freedom and Constitutional Politics in Postwar Japan. By David M. O'Brien, with Yasuo Ohkoshi. Honolulu: University of Hawaii Press, 1996. 271p. $50.00 cloth, $29.95 paper.

Daniel A. Metraux, *Mary Baldwin College*

On June 8, 1789, James Madison fulfilled the Federalists' promise to constitutional critics by proposing several amending articles to the Constitution. The unsettled separation of church and state in the United States was legally effected on December 15, 1791, with the ratification of the Bill of Rights. That, to be sure, is but a glimpse into the unfinished story of our nation's attempt to structure the religion-politics dynamic. And the past two centuries, of course, have repeatedly demonstrated that constitutional mandate is a far cry from solution.

David O'Brien's new book, *To Dream of Dreams*, gives us a rare and instructive look inside the troubling ambiguities of constituting political being. His is the story of Japan's postwar constitutional founding, specifically, modern Japan's confrontation with its own religion-politics tensions. O'Brien's aptly titled study should prove to be doubly delicious to serious students of law, religion, and politics: The church-state distinction does not exist in the traditional Japanese cultural mind. This is a sophisticated work that challenges categories of naïve historical, cultural, and political thinking. Fine scholarship voices a subtly questioning self-consciousness. O'Brien's vigorous intellectuality and nuanced inquiry lend his observations and findings a profoundly textured ambiance that makes for a good read and an enjoyable learning experience.

The analytical core of O'Brien's study consists of a number of complex lawsuits involving *jichinsai* and *chukonhi*. A jichinsai is a traditional ceremony held at a construction site before breaking ground in order to worship the spirits (*kami*) of the locality and pray that construction may proceed without incident. A chukonhi is a monument to honor the souls of fallen soldiers loyal to the emperor. Whether the frequently practiced jichinsai or chukonhi are to be regarded as deliberately religious ceremonies or merely as social customs or conventions is difficult to ascertain in a society of religious accommodation that does not draw such clear boundaries.

O'Brien focuses on a legal action prompted by the City of Minoo's participation in the removal and reconstruction of a chukonhi on publicly donated land in 1975. Several housewives living near the relocated monument filed a lawsuit in February 1976 charging that the chukonhi is a religious facility for ennobling the spirits of slain soldiers, which resonates with the militarism of the state religion, Shinto, and that Shinto ceremony accompanying reconstruction of the monument and attended by municipal authorities was a religious activity that violated the Constitution. Although a lower court upheld the plaintiffs' claims, an appeals court overturned the decision, and the Supreme Court concurred, ruling that the chukonhi is nothing more than a secular memorial stone to commemorate fallen soldiers and that the Shinto ceremony was a social formality, neither of which violates the constitution.

O'Brien also discusses a lawsuit filed in 1965 by a Communist assemblyman from the City of Tsu, who charged that the mayor had violated the principal of the separation of religion and state when he used public funds to pay for a Shinto jichinsai on the occasion of constructing a municipal gym. The Supreme Court eventually ruled in favor of the mayor, determining that a jichinsai is not an unequivocally religious action but a social custom of ambiguous religious significance.

Based on these and other lawsuits examined in his work, O'Brien concludes that for the past several decades liberal and conservative forces have used the courts to advance their respective political-cultural agendas. Progressive elements seek strict adherence to constitutional provisions separating all religion from affairs of state; the conservative establishment desires the reinstitution of traditional values and norms, which weave religion into the fabric of political life.

O'Brien notes that whereas the United States innovated an independent judiciary as an essential component of democracy, the Japanese judicial system lacks a tradition of substantive independence: It has never overturned any law, policy, or practice supported by the conservative elite that has governed since 1948. Indeed, the judiciary consistently has shown a conspicuous propensity "for deferring to the government and reinforcing traditional cultural values and norms" (p. 23). Moreover, "the Supreme Court and the lower courts almost invariably reinforce the government's position out of seemingly purblind deference." The courts, O'Brien observes, do not "function as an independent judiciary, and merely follow the dictates of the administrative judgement" (p. 135). Since conservative forces invariably win the cases, progressives have taken to initiating suits to garner publicity and new supporters. Here, O'Brien deftly identifies a crucial dynamic in, and a looming challenge to, Japan's constitutional order.

Japan's failure to develop a system of independent judicial review, O'Brien holds, stems not only from its unitary tradition in government but also from a highly centralized form of judicial appointment controlled by the conservative political ideology that has held sway with rare interruption for five decades. The pressure on judges to acquiesce is enormous; lower court judges who exercise independence are rarely promoted. Typically, the Supreme Court only hears cases permitting it to reverse the occasional antiestablishment decisions of lower courts. Hence, the Supreme Court is the conservative establishment's legal bastion thwarting political change. This important book suggests that the power of cultural conservatives, the waning appeal of aging liberals, and the political indifference of Japan's younger generation mean the conservative preference for tradition will predominate, and the liberal tenets and sentiments found in the postwar Constitution will fade.

O'Brien's lack of a comprehensive training in Japanese studies, however, leads to a number of minor errors. He states, for example, that the Liberal Democratic Party formed a coalition government in 1994 that included the New Frontier Party (NFP), whereas the NFP is in fact the chief opposition party today. He also suggests that the number of religious adherents is decreasing, whereas the opposite is true, and he notes that the Showa emperor only took a decisive stand during his reign when he demanded that Japan surrender in August 1945. He neglects the fact that Hirohito also moved to end the military revolt in Tokyo in February 1936. These minor flaws, however, in no way negate what is manifestly one of the finest recent books on Japanese political culture and law.

To Dream of Dreams is an eloquent and meticulously researched study of Japan's contemporary political and legal system. It is also a fascinating read which students and specialists will enjoy. O'Brien's obvious command of the subtle structures of constitutional law and political order, together with his ability to dissect and analyze Japanese cultural values and politics, make for unique insights into the rapidly changing political scene in Japan. This is an important work in comparative constitutional law and should prove to be of particular interest to students of the postwar Japanese cultural mind.

Human Rights in the West Bank and Gaza: Legacy and Politics. By Ilan Peleg. Syracuse: Syracuse University Press, 1995. 191p. $34.95.

David P. Forsythe, *University of Nebraska, Lincoln*

In this slim volume of 129 text pages, Ilan Peleg of Lafayette College takes on the daunting task of evaluating the status of human rights in the West Bank and Gaza between 1967 and the early 1990s. The subject has long been controversial. Various parties do not agree on what legal regime or regimes apply to the area. As was inevitable, since human rights behavior is one source of governmental legitimacy, Israel's human rights record in the territories has been used widely as part of the broader power struggle in the region. And there has been considerable misrepresentation on the subject, both by Israel and its historical adversaries. Peleg adopts a nonemotional, almost dissertation-like tone in his approach. On the basis of surveying many publications and reports, he does come to clear conclusions.

Peleg argues that Israel's record on human rights in the West Bank and Gaza has not been basically liberal; rather, it has entailed fundamental and systematic violations of many internationally recognized human rights. A primary source of the violations in his view has been the introduction of large numbers of Israeli citizens into the territories, first under the Labor coalition and than under Likud. This led to predictable conflict between the settlers and the Arab inhabitants, with both the military authorities and the Israeli government favoring the settlers at the expense of Arab rights. Not only did the settler movement violate such international standards as the Fourth Geneva Convention of August 12, 1949, for protection of civilians in armed conflict and occupied territory, but also it led to a dual legal system in the territories that favored the settlers and discriminated against the Arabs in numerous ways. Among other sources, Peleg notes the Karp report by Israeli authorities, documenting lack of vigorous investigation and prosecution when claims were presented about settler vigilante attacks on Arabs. Peleg believes that Arab property rights have been much violated in various ways. This section retains its importance since the Netanyahu government, as of mid-1996, was talking about a renewed settler movement.

If anyone still doubts, Peleg also makes clear that rights of personal integrity have been violated by Israel concerning Arab detainees. Citing various sources, including the Landau report by Israeli authorities, Peleg shows a policy not only of coercion of detainees but also of systematic perjury by Israeli interrogators, who lied in Israeli courts about the confessions they had obtained by coercion. Israel, while confronting the issue of interrogation in times of threats to national security, not only failed to prosecute those responsible for ill treatment but also publicly endorsed certain types of ill treatment in its efforts to suppress the Intifada from December 1987 onward. Here is further proof that armed conflict and threats to national security normally erode protection of human rights, even by liberal democracies.

Peleg believes that military occupation is by its nature injurious to the rights of the local inhabitants. He also argues that long-term occupation should be judged by the terms of the Universal Declaration of Human Rights of 1948, not just by the terms of the Hague and Geneva regulations pertaining to military occupation. Long-term occupation, he says, resembles regular government and therefore should be evaluated by standards in peacetime as well as in armed conflict. Finally, the author believes that solutions to human rights problems in the West Bank and Gaza must be fundamentally political and not legal. While the Israeli high court has had a modest effect on peripheral issues in the territories, Peleg does not think it can do much more. He is of the opinion that only two options can substantially improve the human rights situation: a two-state solution or a Palestinian federation with Jordan. In other words, only a political agreement to end the occupation will improve the human rights situation.

No one study on this subject is likely to satisfy all, but Peleg has written a carefully researched and balanced account. He uses both Arab and Israeli sources, and he gives Israeli parties—public and private—credit for several positive steps while documenting serious violations. He does not, unfortunately, fully use the published accounts by the International Committee of the Red Cross (ICRC), which has had systematic access to Arab detainees, including eventually those undergoing interrogation. ICRC statements, generally respected, would confirm further Peleg's evidence about systematic ill treatment. There are places in Peleg's account (e.g., p. 93) where one could wish for clearer thinking about what is mistreatment and what is torture and what might be a type of pressure falling below the level of at least torture. Also, after all the evidence, one could wish that in his conclusion Peleg had taken a clear stand instead of writing that military courts "have not been able to prevent *what some have perceived as* an epidemic of violence and human rights violations in the territories" (p. 117, emphasis added). By his own evidence, it is not a question of perception but of fact.

Despite systematic and serious violation by Israelis of numerous internationally recognized human rights in the territories, after the Oslo Agreement several Palestinians claimed that human rights were better protected under Israeli occupation than under local rule by Yasir Arafat and his Palestinian Authority (*New York Times*, August 5, 1996, p. A3). Ironically, one of Israel's legacies in the West Bank and Gaza may be increased interest in human rights by the Palestinian population.

Socialism After Communism: The New Market Socialism. By Christopher Pierson. University Park: Pennsylvania State University Press, 1995. 249p. $45.00 cloth, $16.95 paper.

David Schweickart, *Loyola University Chicago*

Is there socialism after communism? Christopher Pierson, a reader in politics at the University of Stirling, centers his book on this question. Curiously, one puts down this on-the-whole careful and intelligent work not altogether certain as to his answer.

Pierson's basic theses may be summarized as follows: (1) Both the Soviet model of socialism and the social democratic model of the Keynesian welfare state are dead, but the generalization of the death certificate to all forms of socialism is unwarranted. (2) Market socialism may well be an economically viable, morally desirable alternative to capitalism. (3) Unfortunately, market socialism is not feasible. Well, then, is there some other model of socialism that *is* feasible? The reader expects Pierson to suggest such a model, or if not, to reconsider (1). Curiously, he does neither.

Before examining this conundrum, let me sketch more fully Pierson's three theses. Part I of his book takes up (1). Pierson accepts the standard criticisms of Soviet-style central planning. He also agrees that social democracy is in serious trouble, since changes in the global economy have made "Keynesianism in one country" impossible. But it is not true, he argues, that the working class is disappearing, that socialist and social democratic parties have been uniformly routed, or that the basic values associated with socialism have been overwhelmingly rejected. What the globalization of the economy and the demise of the Soviet Union have called into question, he thinks, is not so much the *ideals* of socialism as the ability of socialist or social democratic parties to *deliver* on these ideals.

Part II sets out a rather general model of the "new market socialism," drawing on the work of, among others, Alec Nove, John Roemer, David Miller, Robert Dahl, James Yunker, and Leland Stauber. During the past two decades there has developed a significant body of theoretical reflection concerning models of socialism that incorporate market mechanisms. Theorists of market socialism reject the traditional identification of capitalism with markets and socialism with planning. They maintain that, although it is not possible to have an economically viable modern economy without markets, it *is* possible to have an economically viable modern economy without capitalists. Their models, so they argue, promise to be not only economically efficient but also much superior to capitalism in terms of equality, democracy, and various other widely accepted ethical values.

Part III assesses the "feasibility" of this project. Pierson distinguishes two senses of feasibility: (1) If implemented, would market socialism work as its proponents claim? (2) Might such a socialism serve as a core element of successful socialist strategy for the transformation of a contemporary advanced industrial society? After a careful consideration of the first question, Pierson finds the case for market socialism "contested but defensible" (p. 190). He is particularly attracted to the form that gives workers in an enterprise the authority to manage the enterprise. He finds there to be "good grounds for endorsing the claim that, if feasible, workers' self-management would bring about a major extension of democracy" (p. 165).

But alas, according to Pierson, neither worker self-management nor any other form of market socialism is feasible in the second sense. Why not? In a word, it is too *radical*. As Pierson correctly observes, market socialism takes aim at the heart of the contemporary economic order. It aims to replace capitalist ownership of the means of production with some form of social ownership. Although most models allow *some* space for capitalist enterprises, for the most part the expropriators will be expropriated. This, says Pierson, is not going to happen. Any feasible strategy for social change, he argues, must take place incrementally and within the framework of liberal democratic political institutions. But market socialism as a political project will never command sufficient support among the electorate. It is too much tied to workers, at a time when nonworkers—students, retirees, welfare recipients—are increasing as a percentage of the electorate. Moreover, market socialism does not address the specific concerns of women, minorities, or ecologists, groups that would have to be mobilized in support of a socialist agenda.

Pierson clearly has a point. Market socialism is not, in itself, very "sexy." But why should it not be a part of a larger socialist project? At one point he seems to say as much: "Self-management under social ownership might be one plank in a revivified social democratic programme" (p. 208). Perhaps this is Pierson's considered view, but it is not repeated in the book's concluding chapter, which summarizes his reasons for believing that "market socialism cannot form the basis of a revised strategy of the left" (p. 213).

In fact, Pierson's concluding chapter seems to contradict the analysis that precedes it. "What makes market socialism seem feasible is its attachment to the *market*, but that which makes it *socialist* is the very thing that renders it infeasible" (p. 211). That is, market socialism is infeasible because it is too radical. But now market socialists are taken to task for being "*insufficiently* radical in their rethinking of the socialist idea" (p. 218). Pierson charges them with paying insufficient attention to ways in which both the state and civil society may be more thoroughly democratized and with not addressing the question of ownership in a radical enough way. (What this more radical questioning of ownership might be is left rather vague.)

The source of this logical muddle is easy to locate. Pierson cannot bring himself to say that the fundamental reason for the infeasibility of market socialism is the sheer power of the capitalist class. This class—the one percent or so of the population which, in the United States, owns one-third or more of all wealth—has rigged the electoral game so as to make certain there will be no effective challenge to its power, at least not in the foreseeable future. References to this class, to its lock on the mass media, campaign financing, and so forth, are virtually absent from Pierson's book. Instead, he blames the infeasibility on market socialism itself, on its excessive/insufficient radicalism.

It would have been better, I think, to have taken Plato's tack. In constructing his ideal Republic, Plato insists that it is worth knowing what a truly just society might look like, even though one cannot be sanguine about its coming into existence. Pierson's decent book would have been a better book if he had addressed the question of class power straight on.

Avenues of Participation: Family, Politics, and Networks in Urban Quarters of Cairo. By Diane Singerman. Princeton: Princeton University Press, 1995. 335p. $39.50.

Robert Bianchi, *Attorney-at-Law, Chicago, Illinois*

Avenues of Participation argues that many residents of Cairo's older neighborhoods rely on informal networks to advance economic and political interests in a manner that partially compensates for their limited influence in Egypt's authoritarian regime. The author presents her arguments in a series of narratives of her experiences during 1985 and 1986, when she lived with an Egyptian family. She examines several types of activity, such as finding marriage partners, making personal loans, private tutoring, trading subsidized foods, holding multiple jobs, paying and avoiding taxes, working abroad, petitioning deputies, using day-care centers, and running family businesses and small workshops.

The book's primary contribution to scholarship is description rather than analysis. Readers will enjoy Singerman's willingness to share the fun of her fieldwork, but they may wish it had been accompanied by a bit more theoretical work. As are many imaginative studies, this one is still a work in progress: The author thinks out loud about the possible meanings of her stories without coming to any solid conclusions. Anyone who has lived in Egypt recognizes that these networks abound and operate as useful survival mechanisms. Nonetheless, it is hard to draw implications from her work about the country in general.

One problem is trying to understand who and what this book is about. The author refers to her subjects as "the people" or "the popular sectors," but in Egypt as in many other countries these are loaded expressions that include many overlapping categories. Although Singerman frequently uses Arabic equivalents for these terms, saying them in another language does not help to narrow the choices. Like most political slogans, they are shorthand phrases used in exhortations, jokes, and put downs, depending on the context and audience. Whether in English or in Arabic, however, they do not define clear social science concepts. Similarly, the "informal networks" surveyed here cover too many different kinds of relationships to tell us much about power. If influence were measured by one's skill in forming and manipulating interpersonal ties, then most Egyptians would be political superstars.

It is difficult to connect this work with the larger body of social science scholarship on Egypt. The author continues the efforts of a legion of investigators who have explored every imaginable form of political participation in Egypt, including many that are informal, illegal, and implausible. Singerman tells us frequently that her work breaks with previous political scientists, whose approaches to Egypt she describes as elitist and ethnocentric, but she does not support these claims with a critical discussion of any particular study or writer. Specialists on Egypt may be grateful to be mercifully dismissed as "the usual suspects," but Singerman's readers will not learn much about the extended intellectual family whose debates nurtured her research interests.

The author avoids taking a stand on the question of how her subjects and their groups fit into the political system as a whole. No one doubts that informal networks of everyday people are part of the Egyptian landscape. But where do they fit in and how important are they? This book reminds us that such groups exist, that they perform important functions, and that we should take them seriously. No model of Egyptian politics is threatened by this author's findings. Singerman has chosen a risk-free approach that leaves conventional wisdom undisturbed while appearing to challenge it. Anecdotally, we have moved a step forward; theoretically, a step sideways.

Social Revolutions in the Modern World. By Theda Skocpol. Cambridge: Cambridge University Press, 1994. 366p. $54.95 cloth, $17.95 paper.

John Foran, *University of California, Santa Barbara*

Along with Charles Tilly, Theda Skocpol is the doyen(ne) of scholars of revolution in the English-speaking world. While Tilly's name has been associated with the resource mobilization perspective and thus quite influential in social movements theory, Skocpol is probably the major figure for those interested in social revolutions. Whereas Tilly has pioneered such fruitful concepts as political opportunity and cultural repertoires, Skocpol's main contribution has been to center attention on the state and its relations with classes and other states, a focus that has been productively deepened and debated by other scholars ever since the 1979 appearance of her classic statement in *States and Social Revolutions.*

The present volume is a collection of eleven of Skocpol's major essays from 1973 to 1989 (three are co-authored with Margaret Somers, Ellen Kay Trimberger, and Jeff Goodwin, and there is an additional essay by William H. Sewell, Jr., to which Skocpol replies). These are framed by an introduction and a substantial concluding essay written especially for this volume, which offers Skocpol's most recent thinking on the field of revolutions. All the pieces should be of interest to scholars in this field. We find, for example, critically appreciative reviews of Barrington Moore, Jr., and Immanuel Wallerstein's classics, in which Skocpol begins to trace out her own approach to issues of macroscopic social change. These are followed by a programmatic essay on the uses of comparative history and by three essays that pre-date *States and Social Revolutions* and introduce its structural, institutional, and international emphases.

The debate with Sewell over the role of culture and ideology in the study of revolutions occupies a separate part of the volume. Here, Sewell argues for the indispensability of such ideologies as Enlightenment thought in understanding the French Revolution, and Skocpol counters with a deft distinction between popular cultural idioms and more explicit revolutionary ideologies. Together, these two 1985 pieces set in motion a research agenda on how to bring culture into theories of revolution that a number of scholars are presently trying to advance.

Four other essays from the 1980s constitute Skocpol's leads on how to apply the insights of her study of the "classical" revolutions of France, Russia, and China to more contemporary cases. These pieces both extend and defend the general principles of *States and Social Revolutions* in the study of more recent Third World cases. A structural, state-centered approach is retained, with some new accents on the importance of culture and ideology and clarification of how agency finds its place within Skocpol's framework of analysis. The essay on Iran, for example, forthrightly addresses the urban and religious aspects of this revolution as posing new theoretical challenges. She and I disagree on whether Iran is an exception among Third World revolutions (her position), or suggests a new framework for studying such cases (my own).

All twelve of these reprinted essays are worth reading and rereading. Of further interest to scholars will be the newly commissioned introduction and especially the conclusion to the volume. In the former, entitled "Explaining social revolutions: First and further thoughts," Skocpol clearly and soberly introduces the themes of her work through a look at the genesis and significance of each piece in this volume, situating them in terms of both the work available at the time each was written and the evolution of her ideas as new realities and new theoretical avenues opened up in the 1980s.

The concluding essay, "Reflections on recent scholarship about social revolutions and how to study them," represents Skocpol's latest thinking on the subject and, given her changing research interests in the direction of the origins of welfare states, possibly her final word for some time to come. Here, some sparks fly as she offers sometimes acerbic replies to her toughest critics as well as hands out praise and criticism of the current generation of scholars of revolution (I must acknowledge that my own work is positively assessed). The essay opens with justly appreciative accounts of the work of Jeff Goodwin and Timothy Wickham-Crowley on twentieth-century Third World social revolutions as the best recent comparative-historical works on the subject. Both start close to Skocpol's own orientation, and each adds significant new elements to the literature. Goodwin's ambitious study of armed insurrections in Southeast Asia and Central America shows how the logic of the Sandinistas' success against Somoza's neopatrimonial dictatorship in Nicaragua is similar to revolutions in directly ruled colonies such as Algeria, Vietnam, and Portuguese-ruled Africa, all of this contrasted with cases in which revolutionaries have failed (Malaya, the Philippines, and El Salvador). These arguments will soon be tied together in a forthcoming book. Wickham-Crowley's *Guerrillas and Revolutions in Latin America* (1992) is also singled out as "remarkable" (and it is): A sophisticated research design using Boolean qualitative comparative analysis is used to examine more than two dozen instances of attempted armed insurrection in Latin America since 1956 to account for successes in Cuba and Nicaragua and failures everywhere else. Skocpol uses these works to assess what has been learned about social revolutions to date, notably how neopatrimonial regimes prove vulnerable, the utility of conjunctural causal models, and the importance of transnational and international processes.

About half of the forty-page conclusion is devoted to a spirited rebuttal of the critiques of Skocpol's work by Michael Burawoy, Michael Hechter and Edgar Kiser, and William Sewell, Jr., representing Marxist, rational choice and cultural/narrativist alternative approaches, respectively. Skocpol ably defends herself in these pages, although the work of Burawoy and Sewell in particular does provide directions worthy of more consideration than she accords them. Most remarkable is the tone of the debate: Although warned that her account will be "personal" and "opinionated" (p. 302) to make reading it as "informative and entertaining as possible" (p. 303), the reader is still unprepared for the bite of her dismissals of these critics as "overweening" (p. 324), "bombastic" (p. 325), "grotesque" (p. 329), or "utterly banal" (p. 332), among many other turns of phrase. The result is indeed entertaining but distracts from the substantive issues at stake. The essay and the book end with a glance at the horizons of the study of revolution, judiciously seen in terms of the current preoccupation with folding culture and ideology into the long-standing concerns put on the map by Skocpol's oeuvre.

This is a well-chosen set of Skocpol's writings on revolution over three decades, and it can be read profitably in whole or in part by all who would follow in her footsteps.

What Was Socialism, and What Comes Next? By Katherine Verdery. Princeton, NJ: Princeton University Press, 1996. 298p. $49.50 cloth, $17.95 paper.

Sabrina P. Ramet, *University of Washington*

As with all too many books being published today, this one is mistitled. Why is anyone's guess, but the mislabelling inevitably has consequences for how one reads the book. Had it been titled "Aspects of the Post-Communist Transformation in Romania" there would be no difficulty, since the book clearly builds on the foundation of analysis of various aspects of life in Romania since 1989. The sundry references to Russia, Poland, and other European states that have abandoned communism indicate that the author is aware of the applicability of some of the lessons that may be drawn from the Romanian case to other cases as of some areas of comparability. Alternatively, had the book been titled "Reflections on Post-Communist Romania and Its Lessons for Eastern Europe Generally" (granted, a nineteenth-century sounding title), the reader would likewise know what to expect. As it is, the subject matter comes as a bit of a surprise.

Although I may complain about the title, I have only praise for the contents. The author clearly knows Romania and tells many an interesting story, including that of the ill-fated "Caritas" financial pyramid scheme of 1992–94. Other subjects explored in this book include the "etatization" of time in Ceauşescu's Romania, nationalism in postsocialist Romania, and property restitution in Transylvania. Drawing together previously published essays of Verdery's, the book will have a broad interest for Romania specialists and will surely make stimulating reading for anyone interested in contemporary Eastern Europe.

Verdery is at her best when she describes the exigencies of life in socialist-era and postsocialist Romania and the frustrations endured by many East Europeans. She should also be commended for generously citing the works of many other scholars from whose work she has benefited. We all stand on the shoulders of those whose work we read, but writers pretend all too often that they found their way without the assistance of any other literature than their own; it is refreshing to find so fine an exception to this regrettable trend. Verdery's semiautobiographical Introduction makes an effective prologue to the book.

Verdery argues in the Introduction that it is a mistake to construe the changes in Eastern Europe as constituting a transition of any kind; she prefers the term *transformation*. But in chapter 8 she uses *transition* without apologies. *Webster's New World Dictionary* defines *transition* as "a. a passing from one condition, form, stage, activity, place, etc. to another; b. the period of such passing." As far as I can see, this definition neither fails to represent what has been occurring in Eastern Europe since 1989 (or, rather, before 1989) nor excludes processes of "transformation" from playing a vital part in such "transition." Be that as it may, Verdery has produced a stimulating and well-researched book, one which offers some useful insights into the economics, social dimensions, and nationalist components of both socialism and the systems that have succeeded socialism across Eastern Europe.

Conflicts of Divided Nations: The Cases of China and Korea. By Gu Weiqun. Westport, CT: Praeger, 1995. 263p. $59.95.

George T. Crane, *Williams College*

Writing on Chinese politics is a tricky business. Events have a way of changing just in time to shatter the argument of a new book. The military exercises in the Taiwan Straits by the People's Republic of China (PRC) in December 1995 and March 1996 have not completely undermined Gu Weiqun's detailed comparative analysis of Chinese and Korean struggles over divided nationhood, but they certainly demand a reconsideration of the observation that "the level of conflict in the Chinese case has decreased over time in a continuous linear fashion" (p. 13). Gu is not the first to face this sort of frustration; indeed, he joins a long line (this reviewer included) of analysts whose best understandings have been challenged by surprising political turns in China. The good news is that he will, no doubt, be able to revise deftly his analysis based upon the command of sources and data amply demonstrated in this book.

The volume is well organized. Chapter 1 speaks generally to conflicts of divided nations, suggesting that tension is expressed in three ways: military competition, national legitimation, and international legitimation. Chapters 2–4 describe how the PRC and the Republic of China (ROC) on Taiwan have squared off in each of these areas of conflict; chapters 5–7 cover the analogous experiences of North and South Korea. Chapter 8 makes comparative observations.

The greatest strength of the book is the detailed coverage of the PRC-ROC face-off since 1949, especially the story of military confrontation. Gu's use of primary sources and confidential interviews allows for a thorough presentation of various schemes for invasion and counterattack. He mentions the rather obscure Mount Alishan Landing Plan of 1962, in which PRC personnel were sent by sea and air to Taiwan to establish guerrilla bases (pp. 26–7), itself a response to the Sea Prowess Plan launched by the ROC to infiltrate military agents into the mainland (pp. 46–7). These are important reminders that active measures were employed by both sides after the second Taiwan Straits crisis of 1958, a point not fully appreciated in most accounts of the PRC-ROC rivalry. The book is somewhat weaker on Korea. Lacking the depth of research and sources he marshaled for the Chinese case, Gu provides a useful overview of North-South relations but with fewer political insights. He does, however, make some good points regarding the differences in the pattern of competition in the Chinese and Korean cases, demonstrating that military rivalry between the two Koreas has been more intense but their diplomatic accommodation more extensive.

Conceptually, the argument is underdeveloped. A promising start is made when, in chapter 1, Gu posits the notion of "transpolital" to describe "relations between states (polities) within divided nations that have characteristics of both international as well as domestic relations" (p. 5). Unfortunately, the conflation of state and polity is not corrected as the analysis unfolds, nor is the transpolital fully articulated. While it may not have been his intention, Gu could have done well to link this idea to the literature on nationalism and national identity, which might have provided some help in understanding the power of intranational struggles riven by the interstate system. Instead, the new concept is more or less ignored after its introduction, and Gu falls back on a descriptive device of fight-standoff-disengagement derived from a 1978 article by Russell Leng, "Structure and Action in Militarized Disputes." The rationale for adopting this categorization is not fully elaborated in the text, nor is any discernible contribution made to this line of work. Moreover, the use of this framework adds little to Gu's historical narrative. When he actually gets to the work of explanation—why, for example, it seems that fights among Koreans have been bloodier than those among Chinese—he relies more on particularistic factors than comparative categories.

This is not to suggest that particularistic explanations are somehow inferior. Quite to the contrary, Gu is probably right when he argues that the PRC's aggressiveness toward the ROC waned after 1962 because the reconstruction after the Great Leap Forward and the destruction of the Cultural Revolution turned political life inward, while North Korea increased pressure against the South in 1965 when it appeared that the United States was distracted in Vietnam. These sorts of contextualized accounts ring true. Had Gu continued in this vein and looked more carefully at the rise of nationalism evident in the domestic politics and foreign policy of the PRC in the post-1989 period, he might have foreseen (as much as any of us can) some of the trouble that exploded in the Taiwan Straits in 1995 and 1996. Although countervailing trends of economic interdependence are also at work, suggesting possibilities for more peaceful compromise, nationalism has frustrated the most optimistic predictions and has confounded Gu's view that PRC-ROC relations had entered a period of military disengagement. Perhaps circumstances will shift yet again and Gu's interpretation will look better a year or so from now. In the meantime, a more persuasive explanation is needed.

The New Struggle for Democracy in Africa. By John A. Wiseman. Aldershot, UK: Avebury Press, 1996. 200p. $59.95.

Matthew J. Costello, *Saint Xavier University*

John Wiseman attempts to analyze the processes of democratization in African states from a comparative perspective (p. 15), an ambitious endeavor. Wiseman admits that the process is ongoing and thus presents his analysis as an interim report, the "story so far" (p. 11). His study has two main objectives. The first is to demonstrate that democratization in

Africa is a product of events and processes internal to Africa, that it is through an understanding of African domestic politics rather than the effect of global politics on Africa that the democratization process is best understood. The second is to show that the current move to multiparty systems is the product of increased mass mobilization, constituting a new phase in African politics. While the first point is probably already well understood by both Africanists and non-Africanists alike, the second point, that the push for multiparty democracy is not simply an elite conflict, clearly deserves attention.

Wiseman begins by identifying the elements he considers essential to understand the current wave of democratization in Africa. While not organized as a coherent model, these elements do comprise a framework that would permit some systematic comparisons. His treatment of democratization as a new stage in African politics rests on fundamental changes he sees in three areas: (1) the environment, particularly the end of bipolarity, the delegitimation of Marxist ideologies, and the demonstration effects of the "Third Wave" of democratization; (2) increased levels of mass participation; and (3) the outcome of the struggle for democracy, which has been a rapid move to multiparty systems. As his principal concern is to demonstrate that this wave of democratization is a product of the dynamics of mass politics, his emphasis falls primarily on participation and democratic outcomes. The analysis thus focuses on the conflicts (struggle) between masses and elites and among various groups in the prodemocracy camp in achieving an opening to multiparty elections (democracy).

The first chapter outlines this framework; the second classifies African states according to whether there has been a democratic opening. The remainder of the book is organized chronologically, breaking the process of democratization into four stages: mounting challenges to state authority, national conferences and referenda, elections, and postelectoral outcomes. The method of analysis is to present brief case studies of the various processes in selected countries, compiled primarily from local newspaper accounts and secondary sources. The cases, drawn widely from sub-Saharan Africa, include all major regions. The detailed discussions of the role of various groups in mass mobilization and of the debates and conflicts between these groups and ruling elites offers significant support for his claims concerning the mass base of this "new struggle" for democracy. The third chapter, which describes the roles of various groups in mobilizing prodemocracy forces, is particularly successful in this regard.

Wiseman is very sensitive to the differences among African states, as is demonstrated by his case methodology. This sensitivity is clear in both the introduction and conclusion. In the first chapter, he asserts that understanding the current wave of democracy requires "paying particular attention to the very different experiences of the different African states" (p. 3). In the last chapter he rejects arguments that attempt to make systematic comparisons, referring to "dedicated structuralists" (pp. 165–72) and rejecting the explanatory value of environmental variables. Wiseman's emphases on human agency in forcing authoritarian regimes to open themselves to competition and the role of domestic politics in African democratization are well taken, and sufficient information is presented in the cases to support the main arguments. Nevertheless, this method limits the extent to which the process of democratization in Africa is understood and explained because it precludes systematic comparisons and offers a chronological description rather than analysis. The possibility of comparisons suggested by the framework developed in the first chapter is not realized, as the framework is not used to structure the case discussions, which raise many more questions than they answer. For instance, Wiseman defines 1989 as the key year in the new struggle for democracy but offers no explanation for its significance; the choice of 1989 is justified empirically (p. 16). While he asserts that the success or failure of national conferences and referenda were the product of the extent of the "resources of real power" (p. 92) held by rulers, he offers no evidence to compare the degree to which these rulers were powerful. There is no analysis of why a referendum or a national conference was chosen, save to note the demonstration effect of the conference in Benin for other Francophone nations (pp. 84–5). Some of these questions might have been addressed if a more balanced account of environmental pressures and mobilizational politics had been offered. The longest chapter on emerging pressures for democratization, offers only a brief discussion of the global economic crisis and its effect on state capacity and legitimacy in the 1980s.

The New Struggle provides a useful chronology of democratization in Africa and clearly demonstrates that this process has been driven by domestic mass mobilization rather than by external forces or simply elite competition; Wiseman is thus successful in supporting his main argument. As an analysis of democratization, however, the book is less successful. Emphasizing the particularity of African democratization, Wiseman eschews systematic comparison, raising many more questions than he answers. His "interim report" does provide a wide-ranging descriptive account that could serve as a basis for developing a research agenda by raising these questions. Still, the agency focus of the study and the rejection of "dedicated structuralist" arguments, while serving the polemical goals of the author, fail to provide an analysis that explains why these events happened at this time and in this manner.

Mobilizing the Masses: Building Revolution in Henan. By Odoric Y.K. Wou. Stanford, CA: Stanford University Press, 1994. 447p. $49.50.

Tang Tsou, *University of Chicago*

In retrospect, the article by Roy Hofheinz, Jr., "The Ecology of Chinese Communist Success: Rural Influence Patterns, 1923–45," stands out as a landmark in the study of the Chinese revolution, or more specifically the communist movement in the rural areas. He came to the conclusion that structural conditions such as unequal land distribution, cultural backwardness, progress toward modernity, and "peasant nationalism" had either no correlation or only a very weak one with his constructed measures of CCP successes when the Chinese revolution in various periods and in all parts of China was taken as a whole. He suggested that "what is perhaps the most important and least examined of all possible explanations of Chinese Communist success [is] the behavior of the Chinese Communists themselves" (p. 77 in A. Doak Barnett, ed., *Chinese Communist Politics in Action*).

As the microscopic examination of the "behavior" of the Chinese communists can only be done in case studies, Hofheinz's seminal article reinforced the focus of Chalmers Johnson's pioneering study of the Chinese communist movement behind the lines and Mark Selden's work on the Shen-Kan-Ning border region. More important, it brought a new level of interest in Chinese communist policies, actions, and behavior in various base areas, with a workshop at Harvard as a signal of this development. Many important books and articles ensued. Odoric Y.K. Wou's book is among the best of these studies.

Following Elizabeth Perry's lead in her stress on the interaction of different ecological environments with the social organization and actions of the peasants, Wou selects for his case studies four base areas on the borders of the three provinces of Henan, Habei, and Anhui during the period 1927–49. For the purpose of comparison, he includes one base area on the Taihang Mountain range, in a hilly and very poor region, as his fifth case. The most noteworthy feature of the book is the thorough use of the internal party documents acquired during his field trips to China. Wou's detailed and meticulous description and analysis clarify some of the much discussed issues in explaining the CCP's success.

One of the first and most hotly debated issues is Chalmers Johnson's thesis that peasant nationalism during the Sino-Japanese war was the fundamental source of communist power and its successes. He based his study on Japanese Army archives, the best available source materials at the time. Those documents accurately described the phenomena as perceived by Japanese officers and soldiers. He was severely criticized by Chen Yung-fa in his massive 690-page tome for "romanticizing" the peasants and for offering a "simplistic interpretation of peasant motivation and commitment," which was based on the "limited scope of his evidence" (Chen, *Making Revolutions*, p. 13). But *pace* Chen and other critics, Johnson's conclusion is not wrong, insofar as it describes the phenomenon of the peasants' activism in the war of resistance against Japan, which in turn explains the CCP's successes. It still stands the test of time. What it fails to do is describe and analyze the long chain of mechanisms and processes that produced the outcome, namely, peasant nationalism, which the Japanese soldiers and officers confronted. These processes and mechanisms can be ascertained only through a detailed and careful examination of internal party documents.

By focusing on cases, more specific than those in Chen's book, and offering more detailed examination of each, Wou succeeds in pinpointing these processes and mechanisms more accurately. This specificity and historicity distinguish Wou's work from the more topical treatment offered by Chen, although Chen did give homogenized factual materials under each topic.

Space limitation forces me to outline in the following formula those mechanisms and processes which transformed the peasants' parochialism into a nascent nationalism. This oversimplified formula, of which the various elements had many combinations and permutations in different base areas, is as follows: Japanese invasion of China→the defeat of the Guomindang (GMD) forces and the collapse of the GMD's authority in the countryside→the militarization of the villages and subcountry units for self-protection against the Japanese as well as the defeated and disorganized GMD soldiers and the bandits→ the proliferation, fragmentation, and conflicts among these local militarized groups→the CCP's use of these "local political power crises" by offering its organizational and military skills and power for the protection of the locality→the formation of united fronts not only on the top levels with the local gentry and GMD officials remaining in the localities but also the penetration of village society to the grassroots level with the local sectarians→the mobilization of the peasants with appeals to their social, economic, and political interests→the justification of all these actions in terms of nationalism→the actual benefits received or perceived by the peasants and those in the United Front→ the supplementation of peasants' parochialism by nationalist appeals→the emergence of peasant nationalism. It is the outcome of this long chain of complicated mechanisms and processes—peasant nationalism—which is the basis of Johnson's still correct although telescoped conclusion for this period.

The CCP's success in taking full advantage of the "local political power crisis" stood in sharp contrast to the reported instances of the limited effectiveness of its rent-reduction program in the base areas where tenancy rates were very low and cultivated land was scarce and not very productive. By comparison, a "local political power crisis" occurred in almost all localities where the CCP found success in spite of widely different socioeconomic circumstances. This contrast suggests that in the total crisis which confronted or was perceived by the Chinese in the twentieth century, political crisis resulting, first, from warlordism, then civil wars, and finally the Sino-Japanese War loomed much larger than economic crises, stressed by Marxist or left-wing historians. Chinese communist success was based on its skill in organization and leadership in building a structure of political power, in organizing military units, in mediating or controlling local military disputes, and in adjusting policies and actions to the widely different socioeconomic conditions in different localities. At least during the Sino-Japanese War, the redistributive program was more of a means to achieve political power than an end in itself. Some of these mechanisms or processes were innovations, and others were practices borrowed from traditions. In all cases, they were the result of political choices and strategic interactions with their enemies, allies, friends, and supporters. In most places, they were systematized into a unified program to make a revolution and to lay the foundation of a communist state.

In his book, long considered authoritative in its analysis and effective in refuting the interpretations found in all prior works, Chan Yung-fa also put his description and analysis of the CCP actions in terms of making a revolution while building a state at all levels. His book was also based on internal party documents, although they were those found in the GMD's archives of the Bureau of Investigation and Bureau of Intelligence on Taiwan. But the interpretation and analysis of Wou's book emphasize aspects of the same mechanisms and processes that are very different from those of Chen. Chen underscored class struggle and coercion, that is, the CCP's manipulation of the peasants in struggling against the landed elites. In comparison, Wou shows how the United Front and the exigencies of the times converged to make accommodation with the local gentry, at least a large part of them, an adopted policy as well as a necessity. Wou writes that "it was a policy of patriotism, moderation, and constant consultation that ultimately won the support of rural elites for the revolution" (p. 270). Chen coined the term "controlled polarization" to characterize CCP policy, with the emphasis on the word "polarization." In contrast, Wou underscores "coalition politics" and "the dual tactics of struggle and compromise." He makes the point that the party adopted programs which "brought these two rival classes [the peasants and the elites] together to participate collectively in rural revolutionary reconstructions" (p. 271). He writes that the party did not simply act as a mediator between the various rural classes and that its economic programs served to integrate communities (p. 283). One of the innovations was a hierarchy of "multi-class committees" which played a part in making decisions from the county level downward. He notes that "the Communists were less successful in community-divisive programs and had better success in community-cohesive and production programs" (p. 383).

The two different emphases of Chen and Wou could be considered complementary. Obviously, there were both coercion and persuasion, both class struggle and class accommodation, both confrontation and compromises, both conflicts and integration. It was natural for some scholars from Taiwan to underscore the former element in the foregoing set of contrasts to rationalize the loss of the mainland by Jiang Jieshi. But if one takes Chen's analysis and interpretation exclusively, one cannot explain the success of the CCP, as the GMD's coercive power was much more formidable and was used more brutally; and it practiced *realpolitik* much more astutely than did the CCP. What happened was that the CCP mobilized the hitherto passive and submissive peasantry and at the same time won over a large part of the local elite, thus creating a large coalition of social forces which overcame the militarily vastly superior GMD coalition of upper classes. It thus forever changed the scope of mobilization and participation in the Chinese political system. Wou's book re-creates for us the processes and mechanisms, particularly between 1927 and 1949, that brought about this historic change in China. He enables us to project ourselves back into the history of that period. This is the contribution of Wou's book, and it is related to the different methodological outlooks between Chen and Wou.

Chen attempted to discredit all other interpretations, but he made no explicit effort to test or use the various general theories in the social sciences that deal with peasant movements. In contrast, Wou seeks to subject two of the most relevant theories to the empirical test of his case studies. He found no "moral outrage" among the peasants in the village, as predicted by the theory of moral economy, and thus discredited it entirely. The social exchange theory fares better. Wou merely suggests that "it is not sufficient to use a behavioral approach (such as exchange theory or social reaction theory) alone to analyze peasant mobilizations" and that "one has to combine both the behavioral approach and a social-structural approach" (p. 32).

But in his narrative, Wou is actually using, without full awareness, one form of the theory of rational and strategic choice which takes a social-structural approach into account. He writes: "Communist revolutionary success varied from program to program and from places to places" (p. 327). Like Hofheinz, he suggests that in the study of the Chinese revolution, "one should focus on the creative and adaptive roles of the Chinese Communist Party and the complexity of rural social mobilization" (p. 326). This approach, combining an appropriate theory with hard work on Chinese documents and data, heralds a bright future for the study of the Chinese revolution, its successes before 1949, and its failures and setbacks afterward.

International Relations

Indigenous Peoples in International Law. By S. James Anaya. New York: Oxford University Press, 1996. 267p. $45.00.

Alison Brysk, *Stanford University*

Like politics itself, the evolution of political science proceeds in repeating cycles rather than as a linear progression. While the study of international law served as one of the foundations of our discipline, it was widely abandoned as an idealist exercise in wishful thinking during the realist-dominated Cold War years. Now that world politics has become increasingly transnational and norm driven, it behooves political scientists once again to consider the role of prescriptive international law in attempts to regulate political interaction at all levels. Students of ethnic politics and human rights have noted the softening of state sovereignty as a definitive parameter for the exercise of individual and collective autonomy, inspiring a renewed interest in international norms as an alternative guideline motivating and predicting the behavior of challengers as well as state response.

This book makes a solid but narrowly framed contribution to the political science literature on international law. Transcending its descriptive institutionalist predecessors, the volume makes a clear, compelling, and historically grounded argument: Although international law developed to legitimate colonialism, it has been transformed by the norm of self-determination to an important resource for indigenous peoples in struggles for human rights. The author does not spell out the implications of this argument, but it provides an important case study in the normative and participatory transformation of international institutions, one which lends support to both neoidealist and constructivist approaches to international relations. The emergence of a set of international institutions linked by principle with a growing influence on state behavior and expectations may even qualify as a nascent international regime. Yet, Anaya carefully notes the inherent limitations in the jurisdiction and implementation of international law, as well as persistent shortcomings in its treatment of indigenous peoples.

Within the terms of its own project, the book's strengths include a long historical view showing the relative recency of the post-Westphalian state-centricity and the ongoing normative struggle over the proper subjects of international law. The review of debates surrounding decolonization, aboriginal rights, and the concept of *terra nullius* is also useful. The discussion of collective human rights and indigenous groups' status as "peoples" contributes a broadened interpretation of community identity and forms of self-determination.

The project's internal weaknesses are a heavy reliance on indications of customary international law and some questionable assertions regarding self-determination and domestic politics. Anaya simply states one of the most hotly contested issues in international human rights: that self-determination serves as a standard of legitimacy for governments (p. 77). While chapter 4 offers a helpful elucidation of norms linked to self-determination, the linkages asserted are tenuous. Similarly, the account of domestic politics glosses over fundamental issues: "Governments usually want to appear to the world as being in compliance with their international obligations, in order to avoid public condemnation or more serious consequences for violating those obligations. Motivated to appear in compliance with their international obligations when under the scrutiny of international bodies, states are more likely to actually be in compliance" (p. 153).

While the book offers a well-written, accessible, and comprehensive synthesis of legal developments, it fails to provide several further elements of critical importance to political scientists. Anaya's theoretical contribution is not situated in a larger account of how and why laws and institutions change (slighting legal debates on positivism and realism as well as the international relations frameworks cited above). Anaya devotes one-third of the volume to implementation of international law regarding indigenous peoples, but he largely lists available institutional mechanisms along with a few examples of their use and potential—rather than documenting the political process and domestic barriers. Finally, this work fails to extrapolate from its own findings to suggest policy remedies or implications for similarly situated groups. Scholars of international relations will thus find it useful to supplement Anaya's treatment with Franke Wilmer's *The Indigenous Voice in World Politics* (1993), which frames a similar argument regarding self-determination in a world order perspective stressing moral suasion and systematic challenges to state sovereignty, and which provides a broader picture of international law and organizations as agents in development conflicts.

Because Anaya concentrates on reviewing legal cases, he does not engage the growing literature on indigenous rights, which could inform and enrich the analysis. He does not cite or discuss the pioneering (if polemical) work of Roxanne Dunbar Ortiz (*Indians of the Americas: Human Rights and Self-Determination*, 1984). Similarly, he does not incorporate fellow international attorney Russel Barsh's political critique of the United Nations system's treatment of indigenous peoples ("A 'New Partnership' for Indigenous Peoples: Can the United Nations Make a Difference?" *American Indian Culture and Research Journal* 1 (1), 1993), a work which specifically treats the domestic and economic obstacles Anaya leaves out. The work under review ignores a series of relevant articles in the *Human Rights Quarterly* that explore the limits of the UN Working Group and the self-determination norm so central to Anaya's argument (see Corntassel and Primeau May 1995, Stamatopoulou February 1994, Tennant February 1994).

The final omission of this internally praiseworthy book is one common to its field: a vision of action without actors. Anaya provides scant coverage of the activities of an extensive social movement and associated network seeking to reform international law from the bottom up. Along these lines, he fails to discuss his own professional involvement in some of the cases he covers.

Nevertheless, the scope, detail, and documentary rigor of *Indigenous Peoples in International Law* make it an essential reference for future work in the field. Anaya reminds us that there are principles which supercede the state and that global norms may assist those most marginalized by state power.

A Genealogy of Sovereignty. By Jens Bartelson. Cambridge: Cambridge University Press, 1995. 317p. $59.95 cloth, $19.95 paper.

Cynthia Weber, *Purdue University*

Jens Bartelson's book is an intellectual tour de force through numerous political and philosophical representations of sovereignty and the configurations of knowledge that make them possible. His main thesis, "that sovereignty and knowledge implicate each other logically and produce each other historically" (p. 5), is traced through three periods, the Renaissance, the classical age, and modernity. For each period, Bartelson asks how sovereignty has been spoken of and known in order to illuminate why it appears to be so difficult to speak of and know sovereignty today (p. 4). For, as he argues, "the topic of sovereignty . . . is so rigorously intertwined with the conditions of knowing, that we could inductively expect a change in the former to go hand in hand with a change in the latter" (p. 247). Bartelson therefore guides his readers through a genealogy of sovereignty/knowledge, a "history of the battles between different interpretations" (p. 76) of sovereignty.

Through a reading of selective political and philosophical texts, Bartelson traces the historical construction of authority structures within and beyond states, paying particular attention to when and how the modern international system, its reliance upon a specific understanding of sovereignty, and the knowledges that enable and sustain it came into being. In so doing, he rejects what he terms the Renaissance hypothesis, "which fixes the chronological origin of the disjunction between the state and the international onto the Renaissance" (p. 89), on the grounds that the international was not fully formed during this period. It was not within the knowledge of the times. Therefore, "to distinguish what was *within* states and what was *between* states was not fully possible, either in theory, or in practice" (p. 89). Bartelson makes a similar argument for the classical age, challenging the dating of the origin of the international system to the Peace of Westphalia and concluding that "the international was a possibility not yet open in knowledge" (p. 187). Only with the dawning of modernity is knowledge reorganized so that a political discourse can emerge which rigidly distinguishes the domestic from the international and locates the foundation of sovereign authority in man.

Bartelson's genealogy should be read through his own theoretical representation of sovereignty/knowledge. In the strongest theoretical chapter, Bartelson suggests that sovereignty is a parergon or frame. It is what makes representation possible because it provides the boundary between and/or around objects of inquiry. Yet, as a frame that situates concepts, it is itself unframed and unrepresentable. This explains why sovereignty is so difficult to define even in modernity. Sovereignty is not something which should be thought of as having an ontological content. Rather, it has a function. In international relations, sovereignty's function is to demarcate the inside from the outside, the domestic from the international, so that they appear to be self-evident, not discursively constructed through complex interworkings of power and knowledge. Bartelson's genealogy explains how the parergonal function of sovereignty emerged from various historical organizations of sovereignty/knowledge so that today sovereignty's function is so taken for granted that political scientists often make the mistake of assuming sovereignty and knowledge were always related as they are in modernity.

Along the way, Bartelson offers compelling critiques of recent international relations theorizations of sovereignty (e.g., Waltz, Wendt), sociological treatments of the state (e.g., Giddens), and hermeneutic approaches to knowledge (e.g., Skinner). Overall, *A Genealogy of Sovereignty* is like three books in one—a genealogy of sovereignty and knowledge, a general theory of sovereignty as a parergon, and a critique of recent understandings of sovereignty, the state, and knowledge.

Throughout, Bartelson borrows heavily from the works of Michel Foucault. His genealogy is theoretically indebted to Foucault for insights regarding the productive side of power and its connections to knowledge (see *History of Sexuality, volume 1*). Furthermore, Bartelson's periodization follows that found in Foucault's *The Order of Things* and *Discipline and Punish*. Yet, with all its debts to Foucault, *Genealogy* lacks two vital Foucauldian touches, one having to do with style, the other with theory.

While Foucault enthralls readers with stories, Bartelson instead offers argument upon qualifier upon theoretical proof. This book is more systematic than anything Foucault ever wrote, which proves to be both a strength and a weakness. For instance, Bartelson provides an excellent discussion of genealogy in chapter 3. It is clear, thoughtful, and an excellent guide for implementation to be consulted by anyone becoming acquainted with this approach. Yet, generally, Bartelson's systematic style weakens the text, for it relies upon stilted and often obtuse language which stifles the creative theoretical moments found primarily in the opening chapters.

More troubling—and perhaps connected—is where Bartelson departs from Foucault theoretically. What Bartelson does best—illuminate representations of sovereignty—ultimately limits him theoretically. His preoccupation with representations of sovereignty precludes his thinking about sovereignty when it ceases to function in a representational logic, when its parergonal qualities no longer sustain domestic/international, inside/outside boundaries. While Foucault's writings on power/knowledge operate mostly within representational logics, they always provide glimpses to postrepresentational configurations. Thus, they are not entirely incompatible with performative logics or with simulation à la Baudrillard, for example.

Yet, Bartelson's book offers no such escapes from representation. When Bartelson senses representation is in crisis, he concludes that sovereignty itself is in crisis. His final chapter raises the question of whether sovereignty is at its end. He suggests that "we got stuck with our parergonal notion of sovereignty and the dualities it entails within our political understandings, not as the result of any immanent logical necessity in history or in the development of political thought, but as the result of the cumulate consequences of random mutations in the conditions of knowledge" (p. 246). Because he seems to equate parergonality with representation, his explanation of how we "got stuck" with parergonality implies that we are also stuck with representation. And he laments: "Precisely this loss of parergonality is the destructive upshot of critical international political theory today" (p. 248). Thus, while critical theory helps us to recognize the limitations of parergonality, "it does not tell us where to go from here" (p. 248). Yet, I would argue that this is the case only if we follow Bartelson's advice that epistemic change "involves the political responsibility of deciding upon sovereignty, a decision we for the moment seem unfit to make" (p. 248). Instead—and still in a Foucauldian vein—we might at this juncture in the genealogy of sovereignty/knowledge accept the undecidability of sovereignty and remain attentive to where that takes us.

Beyond Sovereignty: Territory and Political Economy in the Twenty-First Century. By David Elkind. Toronto: Toronto University Press, 1995. 308p. $50.00 cloth, $19.95 paper.

Saul H. Mendlovitz, *Rutgers School of Law*

David Elkind asks: How might the territorial nation-state and its inhabitants be affected by the technological revolution's influence on the world's political economy and culture (floods of money, goods, and information)? The answer: It is possible and preferable to have a world of "anarchy from a rational choice perspective" (p. viii). He argues that the extraordinary effects of contemporary technological and economic processes strain the conventional controls available to the territorial state. It is the "decoupling of governance from its territorial roots" that fosters, indeed necessitates, new nonterritorial political and social configurations throughout the globe. (Governance here includes corporations, religious institutions, and other organizations.)

Two pages of the introductory chapter sketch twelve scenarios that may emerge from today's world. The list is intended to be evocative and not exhaustive, and all but two entries (Toffler's and his own) are pessimistic, for example, a world of nuclear proliferation, or dominated by trade war, or shattered by ecological collapse. Against this dark context, the remainder of the volume treats seriously only one possible future: the author's optimistic hoped-for "anarchy" derived from the following meta-narrative.

Elkind works his sense of changing times against a canonical description of the development from medieval feudalism to the social arrangements of the modern nation-state. Once, society was composed of lords, knights, vassals, and serfs, all focused on fiefdom, or the church, or commerce, or towns and cities. Individuals saw themselves only in relation to one or more of these social domains, not as members of an overarching polity. That system—undermined by commodification of land, by increased commerce, by exploration—transmuted into the nation-state system. Centrally, for the author, the individual thereupon conceived of him/herself as "citizen." The territorial state "bundled" its inhabitants through that controlling identity.

Whatever the differences between the medieval and modern state systems, Elkind presents here a unique perspective in the comparison of these two periods. He argues that there is a fundamental similarity between them regarding individual "choice." Social arrangements and self-conceptions are conceived as being shaped exclusively through existing societal structures. Currently, he believes, a radical change has occurred. An increasing number of people are experiencing "globalization," that is, have become acutely aware that new worldwide interactions allow creation of a previously undreamed-of variety of relations; the "citizen" is being "unbundled."

Elkind thinks the individual's new sense of multiple identity will be a transforming experience. "Table d'hote" individuals (citizens of nation-states) will enter an "à la carte" global society, selecting participation and membership among a wide variety of electronic communication groups, international groupings, new political processes and organizations, and perhaps even citizenships. Thus, globalization opens the possibility of the development of a benign anarchical global society created by rational choice.

As is typical of accounts of the contemporary world economy, this volume is concerned with transnational corporations, some global social movements, and world religions along with the growth of regional supranational economic organizations. Indeed, they are embedded in Elkind's depiction of his preferred "anarchy." What is notably absent from this account is any mention of the emergence of an articulated, self-conscious, transnational civil society. Thus, Elkind does not consider another possible emergence from that civil society: *global* governmental institutions, a possibility which should have been treated if only for serious negation. Pressures of technology and a range of other global problems may persuade a rational person of the need for world government and may already have functioned as centralizing influences. There have been, after all, around 30 U.N. global gatherings, starting with the environmental conference of 1972, to examine a full agenda of substantive international concerns. These serious efforts by the international community—including the significant NGO forum at each meeting—to develop a global normative order suggest that global institutions could develop.

Elkind sees the environmental movement as a stimulus toward nonterritorial political organization but discusses it mostly as a possible spiritual belief system. Environmentalists, like feminists and peace, human rights, and social justice activists, are given too little play in his analyses. In fact, these groups are predisposed to promoting decentralized governance (including "small is beautiful") and are now contributing to formulation of global standards. Increasingly, they are expressing their "choice" for global implementation of those standards and constitute a significant political impetus in this direction. By contrast, Elkind provides a limited description of political transition toward his preferred world. It is expected to emerge through some undefined combination of existing trends (drift) and rational choice (design).

As to Elkind's "unbundling" and the new awareness of "choice": The emergence of multiple identities is central to his analysis, but he does not adequately demonstrate that our sense of self and choice is phenomenologically different, let alone more satisfying, than that of our predecessors. Furthermore, if an anarchical world were to be produced by a newly acute sense of possibilities, would not socialization, as he himself suggests, operate as a constant pressure reimprisoning or "rebundling" the individual?

The non-Western world receives attention with many references to migration pressures. (Elkind believes, as do I, that these pressures are not likely to be diminished without major equitable redistribution of the world's resources.) Yet, although information about Asia, Africa, and Latin America is regularly brought to bear upon his theses, Elkind draws his viewpoint unabashedly and forthrightly from Western society. The accepted notion that Western culture and civilization are hegemonic underlies this approach. At the same time, his presentation is likely to be uncongenial to scholars, political activists, and policy officials from other parts of the globe. The long traditions, belief systems, and cultures of these areas, as well as the recognized shift of power to Pacific Asia, may produce "choices" other than those Elkind imagines.

One of Elkind's avowed and achieved purposes is to open the minds of his readers to a future which is rarely considered a serious option. Elkind provides admirable coverage of significant trends in communication, economics, and culture. In addition, he exhibits analytic sophistication with regard to how the trends fit into his scheme. Particularly provocative is his examination of the meaning and definition of territory and nonterritory, the use of French Canada and aboriginal regimes to show that territoriality is not essential to polity, how communities will emerge through the "unbundled" citizen, and some sense of their governance. Students are also likely to benefit from his treatment of "public/private;" the decoupling of nation and state; Drucker's analysis of emerging organizations, especially the "third force" (nongovernmental and nonprivate entities), the role of cities; and the resurgence of world religions and renewed spirituality.

The book will undoubtedly serve as a useful supplement to more traditional materials.

The Practice of Power: U.S. Relations with China since 1949. By Rosemary Foot. Oxford: Clarendon Press, 1995. 291p. $35.00.

Samuel S. Kim, *Columbia University*

Sino-American relations in recent years have fallen into the serious state of disrepair that recalls, once again, the wild mood swings of the historic U.S. love-hate relationship with China. With the clarity and simplicity of East-West conflict gone, the relations between the world's lone superpower and the world's most populous country have increasingly become mired in and symptomatic of charged domestic politics in Beijing and Washington. Contentious bilateral issues—previously overshadowed and repressed by the vision of global strategic cooperation—have re-emerged to demand primary attention. Viewed against this backdrop, this book is one of the most valuable and timely additions to the burgeoning literature on Sino-American relations.

A leading British authority on East Asian international relations, Foot, in her third major work, reconstructs the turbulent history of Sino-American relations from 1949 to 1994. This rich, well-documented, and closely argued study seeks to combine international relations theory with keen historically informed observations. The main premise is that the historic shift from hostility to rapprochement and then to normalization can be better understood not by rejecting but broadening realist theory. Realist explanation, while still central to the discussion of the rise, decay, and fall of the Sino-Soviet alliance, is too narrow in temporal scope and issue focus, obscuring broader currents of change that were taking place at both domestic and global levels. For this line of inquiry the book is organized under thematic headings of domestic political, diplomatic, economic, and strategic aspects of the relationship—the Chinese representation issue in the United Nations (chapter 2), the China trade embargo (chapter 3), U.S. public opinion on China (chapter 4), the Sino-Soviet alliance (chapter 5), China's conventional and nuclear capabilities (chapters 6–7), and China's "politico-economic model" (chapter 8).

What gives Foot's book its originality is the application of a two-dimensional concept of power—both hard and soft—and the connections between power and legitimacy to illuminate the multifaceted aspects and manifestations of power embedded in a wider structure of relationships at the domestic and global levels. As well, the evolving Sino-American relations embraced issue areas other than the global strategic balance. The first half of the book is simultaneously the best and the most provocative part in demonstrating the connections between legitimacy and power and the progressive erosion of domestic and international support for U.S. China policy in the 1950s and 1960s, leading up to the rapprochement in the early 1970s and China's much heralded entry into the United Nations. Structural power is far more important than relational power in the maintenance of America's hegemonic position in the global system and in the shaping of preferences and outcomes of U.S. China policy over the years. Where legitimacy is lacking or has eroded, as in the case of the U.S. position on the Chinese representation issue, the author argues, American "power" does not necessarily collapse but requires more coercion; hence, it is most cost-ineffective to maintain such a policy. A heavy and gratuitous investment of diplomatic capital was needed from 1951 to 1971, as Washington's position on this issue lacked willing consent.

Ironically, the U.S. policy of containment and isolation was a blessing in disguise for the Chinese government. America's struggle to establish and sustain a global allied coalition in support, first, of the moratorium procedure and, then, the important question resolution in the world body merely spotlighted its lack of willing consensus and legitimacy in this policy area and its command power limitations. All the same, Beijing's influence in world politics, especially in the Third World, was at an all-time high, spurred on by such other factors as the military burdens it had borne during the Korean War, the rise of a new Soviet leadership in the post-Stalin era more willing to put its relationship with China on a more equal footing, and the remarkable success of the first five-year plan at home. One indicator of China's high national identity status and influence was made evident in the figures compiled in the Office of Chinese Affairs: Between

1952 and 1956, six times as many overseas Chinese in Southeast Asia opted to return to the mainland rather than to Taiwan.

The latter half of the book focuses on China's perceived hard and soft power attributes in order to examine the effects on the relationship of Beijing's fuller exposure to the norms and values of the global political system in the post-normalization and post-Mao era. Contrary to the popular rise of China chorus, Foot sees China's decline as an alternative politico-economic model, while at the same time she lays out the difficulties that confront the United States in exercising its structural power in a multipolarizing world, where state actors are interdependent across a wide range of complex problems and issues. Even in the post–Cold War and post-Tiananmen setting, the United States has continued to demonstrate its desire to engage with a country widely perceived to be an up-and-coming superpower and, as such, invariably part of both the world order problem and the world order solution. Foot concludes, without venturing a prediction, that the post–Cold War era is an uncertain transition period in which Washington and Beijing will nonetheless be forced to manage "continuing if wary engagement, not a return to the estrangement and levels of hostility of earlier years" (p. 257).

The Practice of Power makes a major contribution to our understanding of Sino-American relations over the years. As already noted, Foot is at her best when accounting for the steady and progressive erosion of support for U.S. China policy in the 1950s and 1960s. She is somewhat less successful in describing and explaining the turns and twists of the Sino-American relationship in the post-normalization period, especially from 1989 to 1994. The theoretical and empirical linkages between legitimacy and power for this period are not as well or fully developed. The concept of structural power is not applied rigorously and systematically enough in the latter half of the book to add anything significant to the historically informed narrative. The argument that China's global learning—"the alteration in behavior and rhetoric"—was quite extensive in the post-normalization period is not fully operationalized, tested, and proven. Nonetheless, the book excels in its primary objective of reconstructing and reinterpreting Washington's evolving positions on key issues in the relationship with Beijing, and as such it deserves a wide audience.

The Age of Terrorism and the International Political System. By Adrian Guelke. London: I.B. Tauris, and New York: St. Martin's, 1995. 230p. $59.50. Out of stock.

Martha Crenshaw, *Wesleyan University*

This book makes the argument that the perception of an "age of terrorism" is fading away with the Cold War: "The multitude of challenges that exist to the stability of the international political system makes it unlikely that the world will remain preoccupied with the covert violence of small groups, even if it is a common occurrence" (p. 189). Time will tell, of course, but in the aftermath of the World Trade Center and Oklahoma bombs, the explosion of TWA flight 800, and the bombing at the Atlanta Olympics, the perception of "terrorism" as a threat seems likely to persist.

Adrian Guelke, formerly lecturer in political science at Queen's University, Belfast, and now professor of international relations at the University of the Witwatersrand in South Africa, contends that the idea of an "age of terrorism" is merely that, a subjective perception, and an incoherent and ideological one at that. In his view, "terrorism" is an absolutist term without clear empirical referent. It is loosely applied to violent attacks by small covert groups on Western democracies rather than to a precisely delineated range of phenomena. To Guelke, the concept has led observers to ignore violence in the non-Western world, to demand conformity of thought and even to impose authoritarian standards, to gloss over differences among the practitioners of violence, to neglect the political settlement of disputes, and even sometimes, paradoxically, to legitimize terrorism.

Criticizing the term as a label of opprobrium that is not a useful analytical tool is not new. As Guelke rightly notes, defining "terrorism" is contentious. Its notoriety is exceptional, although terms such as "revolution," "war," "imperialism," and "fascism" are also used as labels and stretched beyond logical definitional boundaries (even largely innocuous concepts such as the "balance of power" or the "national interest" are not free of these defects). The study of the social construction of terrorism is valuable, however, and here it is developed at length and in broad scope. Guelke's observations about problematic usages are frequently interesting. For example, terrorism usually refers to actions in which the gap between means and ends appears to be large, that is, to violence that does not seem to be instrumental. The impression of a unified and systematic threat to Western interests is solidified by the anti-imperialist rhetoric of the users of violence, which actually masks diversity and fragmentation. And he suggests that "terrorism" is violence that stands out as illegitimate and unwarranted against a background of general stability and democratic peace. Along these lines, a normative approach to definition would be feasible, if consistently applied. This means that the term is rarely applied to authoritarian contexts. It is also difficult to isolate "terrorism" in highly violent settings, such as civil war (although here, for example, is it impossible or improperly judgmental in the Sri Lankan context to distinguish between an attack on an army base in the Jaffna peninsula and a bomb in a central bank in Colombo?).

But these strengths also reveal a weakness. Guelke is sometimes diverted from examining the social construction of an "age of terrorism" to describing the history of the phenomenon. Thus, the book suffers from a certain disjuncture, although the author's intention is clearly to analyze the portrayal of "terrorism" rather than its empirical manifestations. The analysis should also clearly identify the proponents of the "age of terrorism" thesis. The public, the media, advocacy groups, think tanks, politicians, and scholars are indiscriminately lumped together. It would be useful to provide a careful tracing of the shifting uses of the term as it has changed over time and as different users have entered the scene.

Furthermore, many causal relationships are asserted but not demonstrated. For example, Guelke says that some academics favored the term "state terrorism" but that "confusion among the terms 'terrorist state,' 'state-sponsored terrorism,' and 'state terrorism' has discouraged use of the last term outside the academic world" (p. 70). Is there evidence for the proposition that these terms confuse non-academic audiences? Another speculation: "Public reaction to incidents in which small numbers of people are killed in covert violence carried out by fringe groups *may be* [emphasis mine] more intense than it is to the carnage wrought by civil war" (p. 187) If calling certain incidents "terrorism" (and who does the labeling? journalists? government officials? scholars?) intensifies public reaction, one should be able to measure a change in attitudes. Does the Western press stress small group violence over civil war? Do public opinion polls reveal different attitudes?

Finally, at some points Guelke is simply mistaken. The term "terrorism" is not reserved exclusively for violence against Western democracies. In India, for example, one finds numerous references to "terrorism," as practiced principally by internal separatist movements but also by leftist revolutionary groups such as the Naxalites. India and Pakistan routinely accuse each other of supporting "terrorism." Furthermore, it is hard to agree that "the credibility of the notion of terrorism has been undermined by the scale of instability that has characterized the post-bipolar world. This is because it has brought to the fore the instrumental value of violence" (p. 186). Thus, Guelke suggests that Serbian atrocities are seen as obviously linked to political goals, resulting in a "muted reaction to horrific events" (pp. 186–7).

In the introduction, Guelke lists various nonpolitical usages of the term in order to show its elasticity and incoherence. Unquestionably, the incorporation of the term into popular discourse, ranging from cultural (the dismissal of the director of the Paris Opera) to nutritional (efforts to frighten overeaters into a low-fat diet at Thanksgiving) to olfactory (wearing perfume that annoys those allergic to it), all references I have noted in the press over the years, can trivialize the concept. Yet, Guelke's inclusion of rape on his list, along with obscene phone calls and currency speculation, is inappropriate, especially in light of recent experience in Bosnia.

To conclude, Guelke is to be commended for the reminder that one cannot build an explanatory theory if the dependent variable is not satisfactorily defined. It is also well to be reminded of our obligation as scholars to use concepts responsibly.

Afrocentrism and World Politics: Towards a New Paradigm. By Errol Anthony Henderson. Westport, CT: Praeger Publishers, 1995. 222p. $55.00.

Molefi Kete Asante, *Temple University*

When I was a child growing up in Georgia, I would often bruise my knees playing football on the dirt roads of our town. My mother would look at the bruises, clean them up, and send me on my way. But when I cut my hand on the lid of a tin can, it was taken as serious business. Errol Anthony Henderson's *Afrocentrism and World Politics* must be taken as a serious examination of Afrocentricity. Unlike many of the snide and sometimes provocative commentaries written by would-be opponents of Afrocentrism, Henderson's work is that of a carefully trained analyst using his keen skills as a political scientist to explore and advance the Afrocentric paradigm.

Although this is a serious attempt to examine what Henderson calls Afrocentrism, a derivative of Afrocentricity, it is flawed in many respects. Henderson seems not to understand the foundations of the Afrocentric theory in the pioneering works of Asante and Welsh Asante, and he seems equally convinced that a narrow, Eurocentric conceptualization of political science can rectify what he sees as the omission of a paradigmatic manifestation of the theory vis-à-vis global politics. Furthermore, Henderson seems not to recognize that the popular book *Afrocentricity* (1980) was not conceived as a scholarly volume; it was an essay on the use of culture in raising consciousness, and it was written to the African American community. Yet, had he treated the origin of the theory, that is, its practical, conscious expression as theory, rather than seek to find mention of the term "Afrocentricity" as datum, it would have alerted him to the more formidable tasks confronting the construction of the Afrocentric idea. To say, as Henderson does, that the Association of African Historians (AAH) published a journal as early as 1973 entitled *Afrocentric Worldview* is only significant as a point of history but certainly not as a substantive statement about Afrocentricity as theory. The AAH created no theoretical works related to Afrocentricity. This is like saying the words *existence* or *existentialism* existed before Heidegger or Sartre. There was no conscious attempt to theorize or philosophize about Afrocentricity before 1980. Indeed, Henderson's lack of treatment of the methodological works of Afrocentrists leaves the impression that such work does not exist.

Henderson sets out to answer three questions: (1) To what extent does Afrocentrism pose a viable paradigm of world politics? (2) To what extent is Euro-American–centered world politics white supremacist and anti-African? (3) To what extent do culture and culture groups inform world politics? This is a decidedly political agenda. And there is value in answering these questions and posing them as defining characteristics of a study in world politics. There is nothing fundamentally wrong with this approach to a discussion of Afrocentricity, since the Afrocentrist may have many different interests. Yet, had Henderson followed the more accepted and acceptable definition (i.e., the quality of agencing the African presence in any given context), as Afrocentrists of the Temple Circle have done, or enlarged the definitions he uses from earlier Asante and Karenga works, he would have immeasurably advanced his project.

The reason is simple. Afrocentrists are claiming that African agency has been curtailed in its expression owing to the complex of social, political, cultural, and economic factors impinging upon Africans for the last five centuries. Even so, it is self-defeating to reduce the mission of Afrocentrists to a mere cultural reaction to white supremacy. The aim, as I understand it, is to advance the human project by insisting on the plurality of cultural perspectives without hierarchy. Where Henderson goes wrong is to assume that culture in the Afrocentrist's view does not include politics, economics, and psychology. That is why he can say that often the Marxists need Afrocentricity to give them a cultural arm, and the Afrocentrists need Marxist politics for analysis. This is very cute, but it ain't so.

Culture, for the Afrocentrist, has always meant the totality of how people organize their negotiation and confrontation with themselves and their environment. Almost no Afrocentrists write of culture in a narrow sense of popular culture; rather, they use a more expansive sense that includes various form of politics, economics, and art. Henderson seems to understand this when he introduces his *kimira* idea as a paradigm of world politics. This is an ambitious project, and I believe the work could have benefited from a more precise discussion of *kimira*. Sensitive to the rap that African Americans only see culture in the sense of singing and dancing, Afrocentrists have always used a broader definition.

What Henderson brings to the table is his special training in political science. That is the way Afrocentricity has developed—through contributions from people trained in communications, aesthetics, economics, political science, sociology, anthropology, psychology, history, and philosophy who view themselves as Afrocentrists. From the angle of a political scientist, Henderson wades into the Afrocentric waters with his own and rather self-indulgent concept, the *kimira* paradigm. This seems unnecessary and certainly does not add to his already wonderful analysis of the world political structure from an Afrocentric point of view.

A few serious issues are handled rather glibly in Henderson's work. He treats Cheikh Anta Diop and W. E. B. DuBois as Afrocentrists. I believe that Diop would not have understood the term and DuBois would have rejected it, given the intellectual place of both at the moment of their death. While

Diop and DuBois are the most important African intellectuals of the twentieth century, both were clearly trapped in Eurocentric terminology, concepts, and constructs. This is not a fatal criticism but merely acceptance of the oppressive nature of Eurocentric thinking on African intellectuals; it is truly one of the Crusian crises of the African intellectual. Furthermore, to say that DuBois or Diop was Eurocentric is not a condemnation of their work; rather, it is an understanding that their freedom was not what they would have desired had they known what we now know because of their work.

There is a definite contribution to scholarship in Henderson's book. It will become quite controversial because of the gaps in its analysis of Afrocentricity. Thus, while this is precisely the kind of scholarly treatise necessary to advance the Afrocentric paradigm, it suffers from a lack of rigor in its treatment of the intellectual history of Afrocentricity. This may be forgiven if we consider the book an attempt to claim some middle ground in the heated debate over the Afrocentric idea.

Theories of Dependent Foreign Policy and the Case of Ecuador in the 1980s. By Jeanne A. K. Hey. Monographs in International Studies, Latin American Series, No. 23. Columbus: Ohio University Press, 1995. 292p. $22.00 paper.

Carl Bromley and Howard J. Wiarda,
University of Massachusetts at Amherst

Compared to its neighbors, Ecuador has seldom been party to the political and ideological conflagrations that have characterized much of the rest of Latin America since the Cuban revolution. If anything, Ecuador has been "too quiet." Even the author admits Ecuador is, formally at least, "uninteresting" and "unimportant," characteristics that might be attributed to its small size, its strategic unimportance, and the absence of severe political emergency. Yet, Ecuador has not been insulated from the rest of the hemisphere and its political trajectory—military rule and import substitutionism in the 1970s, democratization and debt crisis of the 1980s, and contradictory relationship with the United States—suggests that the Ecuadorian experience may be just as "typical" as its more noteworthy neighbors. In fact, according to Hey, Ecuador is an "ideally dependent state."

Hey must realize that she is walking through an intellectual mine field when she describes Ecuador this way. Dependency theory has long lost its radical brio and carries something of what one might describe as dated intellectual baggage, especially in an era of globalization. Hey states her case carefully, however. Ecuador does seem to display the classic symptoms of dependence: Most of its exports are primary products destined for the United States, while by 1987 the foreign aid it received comprised 36.5% of the overall state budget, much of it originating from the United States. Furthermore, Ecuador, like so many of its hemispheric counterparts, has had to follow structural adjustment programs dictated by international institutions dominated by the United States.

The Ecuadorian example serves two purposes for Hey: first, as a case study of how economic weakness and dependence affect foreign policy and, second, to test the suitability of six traditional theories of foreign policy—compliance, consensus, leader preference, counterdependence, realism, and domestic politics. The question posed is, given economic dependence, does Ecuador have any leverage in foreign policy, or is it confined by its relationship to the United States? Specifically, does the different ideological orientation of the Ecuadorian executive affect foreign policymaking within the framework of dependence? In attempting to answer the question, Hey offers a series of vivid case studies spanning the wildly contrasting presidencies of Osvaldo Hurtado, a rather dull and cautious social democrat, and his charismatic successor, Leon Febres Cordero, very much a creature of the 1980s, a bellicose right-winger with a propensity to call anyone in disagreement with him "communist."

Each chapter offers a detailed discussion of an Ecuadorian foreign policy dilemma. Hey explores Hurtado's decision to reform foreign investment in the oil industry and his Conferencia Económica Latinoamericana (CEL), the first hemispheric gathering of its kind to address the debt problem. In turn, she scrutinizes Febres Cordero's decision to use U.S. troops on a road construction project and his aggressive free-market reforms, particularly in reference to his investment agreement with the U.S. Overseas Private Investment Corporation (OPIC). She also contrasts each president's relationship with Nicaragua under Sandinista rule. At the end of each chapter the six foreign policy models are applied to the case study, and Hey consistently finds that these theories offer only partial explanations of dependent foreign policymaking. While this displays a certain consistency and methodological rigor on the author's part, the style soon becomes repetitive. The narrative is impaired by this approach, and, as we discover how thin these theories are as "grand" theory, we get the sense that Hey has set up straw men to strengthen her own preferred explanation. In sum, the theories examined are too easy a target, and the ends of these chapters are often foregone conclusions. This is a pity, as it serves to diminish the author's thorough and impressive research.

In her conclusions, however, Hey shows greater sophistication. She notes that Ecuador's foreign policy orientation is not determined purely by its external relations (as the realists and vulgar *dependistas* would suggest) and that important policies friendly to the United States occur more frequently in the economic realm than in the diplomatic one. Dependency, she suggests, is less a variable than a *context* within which certain variables become important. Yet, the fact that Ecuador has never gone beyond the "bounds of the permissible" in its relations with the United States suggests that its diplomatic latitude is contingent on such moderation.

Hey suggests Hurtado's approach to Nicaragua was contradictory in that he pursued both pro- and anti-U.S. policies at the same time. His leadership "adhered steadfastly to the principles of non-intervention and self-determination," yet "Hurtado did not proceed to include what many may have anticipated to be a corollary of an anti-interventionist foreign policy—active support for Nicaragua in the consolidation of its revolution and its battle with the United States" (pp. 82–3). This demonstrates the president's "complexity" according to Hey. To the contrary, did this not show that Hurtado only paid rhetorical lip-service to high-sounding principles; and when it came to the most decisive confrontation with the United States in Latin America in recent memory, this social democrat was ultimately not *un*helpful to the United States! Verbal rather than any material support for Nicaraguan self-determination and the revolution stemmed not only from his fear of too much U.S. influence in the region, but also from a wish to contain the revolution in case it should prove attractive to sections of the population in Ecuador. While Hurtado was ambivalent, Febres Cordero, in contrast, was true to his class and its politics: He despised the Nicaraguan experiment, and his decision to join the Contadora support group stemmed from the fact that the group advocated the peaceful mediation of the conflict in Central America, a mediation that ultimately contained the insurgents. This did not necessarily contradict his breaking off diplomatic ties with the Ortega government, as Hey suggests.

Hey is to be lauded for bringing to light something of a forgotten history in the study of comparative politics and international relations in Latin America. Sometimes she is inclined to give her subjects too much benefit of the doubt; perhaps she is too kind to Hurtado and too soft on Febres Cordero (especially in her analysis of the Blazing Trials debacle, which is straight out of Graham Greenland). But Hey maintains a critical evenhandedness that scholars will find useful, and the fact that she is not as bellicosely left wing as many of her *dependencia* forebears is both a sign of the times and a tribute to her serious scholarship.

The Negotiation Process and the Resolution of International Conflicts. By P. Terrence Hopmann. Columbia: University of South Carolina Press, 1996. 360p. $39.95.

Russell Leigh Moses, *University of Minnesota*

No one leaves negotiation for very long. And with the passing of the Cold War, H. Terrence Hopmann argues, nonmilitary means of resolving disputes will be at the forefront of international relations practice and deserve additional theoretical investigation. His book arrives at this opportune moment seeking to summarize much of the literature on negotiation and to chart a way forward. His effort in the former area is rewarding, but his attempt to present a new framework is more restrained, and the results therefore are both modest and mixed.

Hopmann's book is the product of two decades of work in both the theory and practice of conflict resolution, primarily multilateral negotiations. He has interviewed many of the participants in the morass of multilateral negotiations on European security, and his case study here concerns the Partial Nuclear Test Ban Treaty (PNTBT) of 1963. Hopmann states early in his book that although he conducted extensive research and interviews in Europe in the mid-1970s and early 1980s, "most of that material is being saved for a subsequent, more substantive book on the negotiation of European security from 1973 through 1992" (p. viii). His discussion of the PNTBT is surprisingly brief and, as an overview, seems to be drawn primarily from secondary sources. Consequently, though his effort throughout the book is to bridge the gap between theory and practice (which he insists is one of the main themes), the emphasis is almost entirely theoretical.

Hopmann's theoretical focus derives from his goal "to develop a framework that will explain and evaluate how the negotiation process affects the outcome of negotiations" (p. 6). He posits that there are negotiations in which participants take part for mutual benefits specific to the subject at hand, and those designed to solve problems of larger, common international concern. Examined through the prism of "process," negotiation involves both common and conflicting interests and interdependent decision making. This insight is not all that remarkable, and Hopmann is careful to cite the previous literature on negotiation that produced this consensus. But he fails—as do many other authors he cites—to draw strong, sharp lines among negotiation, bargaining, and diplomacy; each are seen here as solving common problems and producing mutually beneficial results. Without an explicit delineation, nearly all matters of international activity could be squeezed into his consideration.

Because Hopmann wants to "to be able to explain the outcomes of negotiations as a consequence of the process of bargaining" (p. 30), he treats outcomes as dependent variables (p. 28). A negotiated agreement is an outcome, Hopmann argues, and should be evaluated in terms of its efficiency (the best agreement reached jointly given the conditions prevailing at that time); the stability or permanence of the agreement; and the distribution of benefits to the parties participating. But this aspect of his framework remains undeveloped in the book, and the causal relationship between certain types of negotiation processes and specific outcomes is weak in the literature and, despite references to game theory, unsupported here.

Hopmann's coverage of the literature is exceptionally good. Because his aim in this respect is "to organize the existing knowledge and integrate it wherever possible into a single analytical framework that is hopefully tight enough to provide some useful guidance while remaining flexible enough to respond to the tremendous variations in negotiations taking place around the globe" (p. 33), a wide range of material is surveyed. (Unfortunately, there is no bibliography in the book.) After presenting the case of the PNTBT, Hopmann focuses on the utility of existing bilateral bargaining models and notes that the rational actor assumption is not very helpful. Yet, he fails to note explicitly that these models were designed for all types of interactions, sometimes embracing but rarely limited to negotiation.

The third part of the book introduces the complexity of the negotiation process—where models of rationality fail. Hopmann surveys the previous work on the role of power and influence; the part played by individuals in negotiations; the input of bureaucracies; the process as affected by the interaction of the negotiating parties (an understudied aspect both elsewhere and here); the influence of the international environment; the involvement of third parties; and the character of multilateral negotiations. His ability to reach into and recapitulate the literature is most impressive. One of the more remarkable tendencies in the volume is Hopmann's unwillingness to dismiss previous work on negotiation and his effort to elicit some contribution from what has gone before. But one outcome of this tendency is frequently a re-presentation of useful, limited work and little integration into a new, more general approach. As Hopmann admits, eschewing the assembly of a single, overarching scheme means that "the analytical framework presented here is less than a full-blown theory" (p. 32). This is especially unfortunate because much of Hopmann's earlier work (in *International Studies Quarterly* and elsewhere) was both original and a challenge to prevailing assumptions. Hopmann's acceptance of much of what has been gone before pulls him away from recasting or deconstructing previous work. A more ruthless appraisal of past research would have been both exciting and theoretically rewarding.

More disappointing is that Hopmann's attempt to apply his conclusions about the literature to the case of the PNTBT is all too rapid (a little more than one-tenth of the volume), and the concluding chapter, purportedly a look at the theory and practice of negotiation, is two and one-half pages. Hopmann may be correct that "the three most important influences on the outcome of the [PNTBT] negotiations were domestic bureaucratic and political influences, the international environment, and the negotiation process itself" (p. 311). But with little integration of his conclusions with the case study, it is difficult to tell whether he is correct and, more important, what it means.

The Negotiation Process and the Resolution of International Conflicts tells us where much of the literature on negotiation (generally and generously defined) has been and offers hints about where it should go. Hopmann's promises about an original framework for bridging the gap between negotiation theory and practice are modest, and he largely delivers on those promises. But a better understanding of the character and course of negotiations in the post–Cold War era is precisely what is now desperately needed.

The Genesis of Chinese Communist Foreign Policy. By Michael H. Hunt. New York: Columbia University Press, 1996. 343p. $34.50.

Xiaobo Hu, *Morehead State University*

China's opening up has provided a virtual social laboratory for studying political, economic, and social transformation and has given researchers unprecedented opportunities for a fundamental reappraisal of conventional wisdom about Chinese foreign policymaking as well. A rich, intimate, yet theory-oriented political history in which both historians and political scientists should be interested, Michael Hunt's new book, using recently available materials from China, should be welcomed as a significant contribution to reexamining the explanatory power of some underlying concepts in studying the origins of Communist China's foreign policy.

With a serious analytic core, this book engages in the time-tested yet unresolved theoretical debates about the nature of CCP politics and the role of ideology and personality in policymaking. Refuting convincingly the static ideological explanation of the CCP's foreign policy, Hunt argues that Communist Chinese orthodoxy and its definitions of such essential concepts as imperialism (e.g., pp. 93–4) and the united front (e.g., pp. 131–2) underwent changes. Hence, according to Hunt, the CCP's foreign policy was guided in turn by patriotism, utopian faith in rapid social transformation, and opportunism in "manipulating other international actors" (p. 134). Not necessarily ideologically consistent, Hunt further maintains, CCP foreign policy parallels the increasingly dominant personality and concerns of Mao Zedong (chapters 5 and 6).

A major contribution of this book to the field of Chinese history and international relations is the bold redefinition of Chinese traditions by emphasizing the long history of "self-interested opportunism" in foreign policy (pp. 214, 216, and 6–8). This claim is further supported by new primary sources from the CCP and other recent Chinese publications, by which Hunt shows a foreign policy "responsive to the needs of the revolution and the changing configuration of international politics" (p. xii).

To substantiate such provocative arguments, Hunt starts with a summary review of Chinese history far back to the Warring States period (chapters 1 and 2). This provides a context for more intimate examination of constant concerns of prominent scholars in the late Qing era and of early nationalist and Communist leaders, such as territorial security, autonomy, and feelings of humiliation (chapters 3 and 4). To Hunt, the special relationship between the Soviet camp and the Chinese introduced through the common ideology only led to perplexity, division, and revision in the latter (p. 120). Hunt's new materials also show vividly how Mao took command of Chinese foreign policy under such conditions (chapters 5 and 6). This section of the book is the most resourceful.

In an effort to build a dynamic analytical framework, Hunt sets his explanatory variables (e.g., ideological currents and political circumstances) in motion and lets them interact with personality in decision making (chapter 7). Furthermore, he finds that "the institutional arrangements supporting CCP decision making [up to the early 1950s] were loose and shifting" (p. 227). More far-reaching is Hunt's proposal in chapter 8 to reform the historical agenda, along with some new scholarship (e.g., Quansheng Zhao, *Interpreting Chinese Foreign Policy*, 1996), by transcending the macro versus micro, domestic versus international, and other conceptual boundaries. Political scientists may want to read the last two chapters first, because they provide a cleaner conceptual framework and a clearer analytical map of the issues Hunt discusses in the previous pages.

There is much to learn from this book, but there is also much to question or critique. While ambitious, far-reaching, and rich, it makes many assertions that lack elaboration or substantial evidence. For instance, the historical pattern of "self-interested opportunism" is described, yet little systematic effort is made to provide its micro foundations. What kind of self-interest is involved in securing a remote and troublesome periphery (e.g., Xinjing), which is not "culturally Chinese-dominant," and which may cause international conflicts that threaten the weak state of the late Qing era? What is the theoretical difference between this central concept of self-interested opportunism and the notion of individual self-interest in the choice-theoretic literature? Minibiographies are provided, and various writings of Mao are examined, but coherent explanations are lacking as to how individual self-interests were transformed into an historical pattern of "self-interested opportunism" in China's foreign policymaking. The quick answer given by the author is Mao's growing personality cult and the lack of stable institutions. But it is dangerous to argue that "nothing happened last night because the dog did not bark," that is, without information on formal and informal institutional arrangements. It would be useful to know how the interaction between the personality cult and the "loose and shifting" institutions affected policy. New Chinese publications, including memoirs, have clearly indicated a growing institutional presence in policymaking after the late 1940s. Moreover, while constant variables cannot explain changes, a theoretical framework loses its explanatory power if every single explanatory variable is in flux simultaneously. In this sense, Hunt's book points out three great factors: (1) historical Chinese self-interested opportunism; (2) Communist Chinese self-interested opportunism; and (3) Mao's enlarging personality cult, but the next meaningful question to address is how they are related. There are huge gaps to fill in establishing a causal relationship among these factors and between them and the genesis of CCP foreign policy. It is a job that cannot be done without a micro foundation. Hunt has done an excellent job by bringing Chinese history so close to that foundation.

While new and exciting materials help us to understand Mao's handling of foreign policy, many other significant points in the book are supported only by dated secondary citations (e.g., pp. 34, 61, and 66–7). Furthermore, in examining the Sino-U.S. relationship, U.S. sources are strangely much underused. Nevertheless, Hunt's book should be commended for its bold conceptual framework and engaging historical reappraisal. The predictive power of Hunt's dynamic analytical conceptions could be further enhanced through generating and testing more substantial hypotheses from his current findings.

A foremost Western historian of China's foreign relations, Hunt has taken the lead in the quest for a new understanding that brings history and political science closer. This book contributes a great deal to this new understanding, not only of Chinese history and foreign policy but also of politics in general. In this quest, the literature guide at the end of the book is also very helpful.

Local Commons and Global Interdependence: Heterogeneity and Cooperation in Two Domains. Edited by Robert O. Keohane and Elinor Ostrom. London: Sage Publications, 1995. 261p. $75.00 cloth, $21.95 paper.

Barry B. Hughes, *University of Denver*

The editors of this volume identified two bodies of literature that have much to offer each other: (1) the search for generally local solutions to problems of common pool resources (CPR) and (2) the development, within international relations (IR), of regimes that imbed independent states in patterns of cooperation on a range of issues. Both seek to understand the emergence of cooperation from the self-interested interaction of multiple actors in the absence of solutions dictated by central political authority. Both emphasize the importance of institutional development, elaborate the processes of that development, and consider the roles that institutions play in facilitating ongoing cooperation.

Yet, the CPR and IR literatures seldom talk to each other. Surely, the editors suggest, there is much to be gained by comparing understandings of collective action problems at polar ends of geographic scale. The editors and contributors used two focal points for comparison and contrast. The first and dominant focus of the volume is the actor set (its size and heterogeneity) and how it affects the prospects for cooperation. To start, the contributors consider how the number of actors and their heterogeneity influence prospects for cooperation. Intriguingly, most of the IR literature argues that cooperation becomes more difficult as the actor set grows, while the CPR literature sees the number as relatively unimportant. Moving from consideration of size to heterogeneity, many contributions to both literatures, especially those based on formal theory, posit homogeneous actors. Some work, especially hegemonic theory in the IR literature, investigates the implications of size or capabilities differentials. Little work in either tradition considers heterogeneity of actor preferences or of information and beliefs, a substantial area of attention in the volume. (That these largely ignored heterogeneities then become major topics is somewhat surprising, and even distracting, considering the book's purpose of initiating dialogue between existing traditions).

The secondary focus of the collection is on the creation and character of institutions, especially the investigation of what design principles or processes lead most effectively to institutional development. The CPR literature, and especially the current and earlier contributions of Elinor Ostrom, tend to dominate this discussion. As with many edited collections, the reader needs to do a considerable portion of the work. In fact, the editors set the stage by identifying the literatures, outlining the case for comparing them, and describing the general charge to the contributors, but they then retire from it. They do not integrate the arguments of the contributors, clearly identify what the literatures can learn from each other (or why they cannot) and proceed to suggest fruitful bases for either future comparisons or for research in each tradition inspired by lessons of the other.

For instance, we do not leave the volume with any resolution of the relationship between actor number and cooperation. It is tempting simply to conclude that CPR studies seldom deal with situations in which a hegemon or other k-group emerges, and thus no actor subgroup has the incentive of leadership that larger numbers of free riders can erode. And although several authors explicitly recognize that the IR literature on cooperation looks at all four cells created by the intersection of rivalry and excludability (private goods, coordination or club goods, common pool or property resources, and pure public goods), there is little effort to tease from the IR work that subset of findings which deals with the common property resources of the CPR studies. Similarly, the book does not significantly investigate the applicability of the design principles of the CPR studies (such as nesting of groups and processes) to the regime formation literature.

Fortunately, the authors generally provide rich grist for the mills of those who may subsequently pursue greater synthesis. The chapters by Young, Snidal, and Martin productively carry forward the theoretical discussion of the editors' introduction. Young and Snidal provide two of the chapters that most explicitly compare and contrast the two literatures. In light of his past work, it is not surprising that Young gives us a discussion strong on institutions (rather than actors). For instance, he notes that the CPR literature emphasizes the evolutionary and self-generating elements of institutional development, in contrast with threshold change resulting from negotiations in the IR literature. Also not surprisingly, Snidal devotes more attention to actors. For example, his discussion of the differential potential for decentralized enforcement contributes important comparative insight (it is safer to allow such action on CPRs than in IR).

Martin explores the implications of actor heterogeneity in some depth and concludes that, in combination with issue linkage, heterogeneity in preferences can actually facilitate cooperation. A chapter by Hackett, Dudley, and Walker give us results from laboratory research, suggesting that differences in actor capabilities can hinder cooperation, but not as much as other literature suggests. Again, the various discussions of the volume on heterogeneity cry out for some reconciliation and synthesis. The rest of the volume consists of case studies and the chapters are rich in insights. Ostrom elaborates Coleman's concept of social capital. Because we normally think in terms of physical or human capital, this chapter illuminates how well-intentioned investment in physical capital can simultaneously destroy social capital, with net negative result. Liebcap presents three case studies of common pool resources that collectively provide some insight into the importance of timing and sequence of efforts to cooperate.

I found the chapter by Oye and Maxwell to be the strongest in the book. They not only move with facility between IR and CPR cases but also present and apply a useful theoretical framework. Specifically, they contrast "Stiglerian" and "Olsonian" situations and prospects for cooperation in each. In the former, benefits accrue to a few and costs to many, leading the few for selfish reasons to support action in the common environmental good (e.g., a small set of strong corporations with identified substitutes for chlorofluorocarbons supported their control). In the latter, only a few bear the costs of regulation with benefits for many. The authors argue that Stiglerian examples may be quite common.

In the final analysis, I completed this volume with greater understanding of and appreciation for the differences between the CPR and IR settings than when I began. That learning alone would make this an important book for anyone interested in cooperation in either domain. In addition, however, the book does mine a considerable amount of gold from the many remaining similarities. You may need to do some smelting, but the ore is there.

Currency and Coercion: The Political Economy of International Monetary Power. By Jonathan Kirshner. Princeton: Princeton University Press, 1995. 289p. $39.50.

Philip G. Cerny, *University of Leeds*

International monetary power has always been at the heart of international relations, from Thucydides to Jacob Viner and beyond. The very foundations of state sovereignty and power in both domestic and international politics are grounded in the issuance of and control over national currencies, and the Janus-faced nature of monetary power has always put it at the heart of both the structural hierarchy of states and their ability to exercise relational power over other states. Such monetary coercion has been somewhat neglected, however, in the contemporary revival of international political economy in favor of trade, aid, and other kinds of financial power, and Kirshner sets out to fill this gap.

Currency and Coercion begins with a theory of monetary power. Kirshner is not interested in international monetary power as a tool of economic policy or financial management but in how it meshes with the general capacity of states to exercise power over other states, especially in relation to security and political matters. In Part I, he identifies three modes of practice of monetary power—currency manipulation, monetary dependence, and systemic disruption—and considers the problems and implications of each. Part II, the core of the book, consists of three chapters, one on each of these modes and its use; the longest and most detailed looks at currency manipulation (chapter 3). These chapters are mainly concerned with the historical examination of both "focal" case studies—two in chapter 3 and one each in chapters 4 and 5—and shorter "illustrative" case studies—eight in chapter 3, three in chapter 4, and two in chapter 5. Part III looks at four cases in which states abstained from using monetary power when opportunity seemed to beckon, followed by general conclusions.

The main strength of the book lies in its interweaving of economic and security issues, both theoretically and empirically. The author demonstrates not only the ubiquity of monetary coercion but also its significance in structuring wider power relationships, and he clearly sets out its distinctive features. The case studies make fascinating reading, and scholars will welcome the breadth and depth of this survey for expanding their understanding of the three modes and of several key historical episodes. Currency manipulation takes the lion's share of attention, of course, with its centrality to a wide range of situations of both war and peace. Monetary dependence is a more limited mode, requiring the presence of currency areas, zones, or blocs; at the same time, its scope can be very wide, involving complex ongoing relations, whereas currency manipulation is more episodic. Systemic disruption appears as a much more limited mode, and one which usually concerns "strategic" attempts by members of a system to broaden their autonomy within that system. (Indeed, no cases of full-scale disruption with the intent to bring down the whole monetary system can be found, and a case might be made for classifying systemic disruption as a subcategory of monetary dependence.)

One major problem, however, is that the book does not fulfill its analytical promise; it does not effectively tackle the question of how monetary power interacts with the way the international system itself works. The case studies, while well executed, remain essentially descriptive, and the "theory" remains essentially taxonomic. The book's main assertion is that monetary coercion is a crucial and often neglected variable in a range of historical situations, but the author himself—despite enumerating a range of conditions in which monetary power will be more or less successful and situating his problematic in a range of important literature—frequently admits defeat in terms of being able to generate broader hypotheses and generalizations from the "fog" of historically complex material. Thus, the work tends to fall between the stools of history and social science. It is also rather uneven.

There is a major substantive problem, too, at the contextual level. Although the author does not say it in so many words, coercive monetary power is treated here as very much a feature of a traditional system of states, and its exercise in effect depends upon the capacity of individual states to threaten or achieve closure. Most of the cases studied concern either wartime or situations in which war, the Great Depression, or other disruptions to a relatively open international economic order are fundamental. The four "focal cases" concern China during the Japanese invasion, the 1956 Suez Crisis, the German mark bloc in the 1930s, and Franco-British conflict over the gold exchange standard in the late 1920s and early 1930s. International currency and related financial markets are seen as ancillary; markets are important, but only insofar as they lead to bandwagoning or countervailing effects to states' attempts at monetary coercion. For example, although the differences between exercising monetary power under fixed versus floating exchange rates is considered from time to time, these are presented merely as providing different opportunity matrices for state action; they are not analyzed as part of the wider structure of the international system or of the way that states and markets interact in general.

This problem is exacerbated by the choice of case studies. Most of them are from the two world wars or the interwar period, a time when the states system probably reached its apogee. Only one case study comes from the period after the collapse of the Bretton Woods system—the attempt by the United States in the late 1980s to use the monetary weapon against Panama (usually thought to have failed, despite the author's argument to the contrary). And except for one inconclusive page right at the end, Kirshner ignores the explosion of private financial markets since the 1960s and 1970s and overlooks how states have been drawn into complex transnational financial flows and webs of structural innovation; these lead them to focus on manipulating their currencies not in order to attack or defend against other states, but to retain or recover marginal influence over increasingly autonomous international capital flows and cross-national price sensitivity. Attempts at international monetary coercion will continue, but an understanding of their complex ramifications and limitations in a world of global finance will have to await another study.

World Cities in a World System. Edited by Paul L. Knox and Peter J. Taylor. Cambridge: Cambridge University Press, 1995. 347p. $64.95.

Susan Clarke, *University of Colorado*

As a heuristic, globalization threatens to become an intellectual Rashomon in which each sees just what s/he wishes. While it is clear that economic, social, and cultural trends manifested on a global scale are increasingly shaping political dynamics at other levels, explaining how, where, and when this occurs remains problematic. Continued reference to globalization as a unifying concept without specifying these relationships undermines the potential contribution of research framed in these terms. Knox and Taylor's edited volume brings a welcome willingness to engage and advance some of these theoretical and conceptual puzzles. Their success stems in part from organizing the book around John Friedmann's seminal 1986 hypothesis about world cities (reprinted as an appendix) and then inviting Friedmann and authors from different disciplines to reflect on the continued utility and robustness of the world city paradigm.

In contrast to the dispersion and diffusion trends marking most globalization accounts, the world city argument focuses on the emergence of strategic sites—an archipelago—of concentrated global command and control activities along with the producer services supporting these operations. The core agreements include viewing these world cities as articulating regional, national, and international economies into a global economy, recognizing that global capital accumulation occurs in a core space that excludes the majority of the world regions and their populations; seeing world cities as the nexus of intense economic, social, cultural, and political interaction; acknowledging that these world cities can be arranged hierarchically in ranks reflecting their economic power; and understanding that these configurations give rise to a transnational capitalist class with interests, culture, and ideology often in conflict with more place-bound classes (p. 26). In his reassessment, Friedmann characterizes these elements as an emergent research paradigm, one capable of synthesizing discrete disciplinary approaches and providing a meta-narrative to ground more critical views of globalization trends.

Despite the economic and technological nature of the core assumptions, this heuristic encompasses social and cultural trends as well. These global accumulation processes exacerbate dualism within world cities as well as within the world system. This class of cities is distinguished, therefore, by both strategic importance in a global economy and internal social dualism patterns. Within this approach, there is also an appreciation for the extent to which globalization, global identities, and world city status constitute socially constructed processes. While this emphasis on rhetoric, symbols, and metaphors seems to encourage the turgid prose of several of the contributions here, the interpretive dimension is an important complement to more economistic approaches to cities and globalization.

Lest this volume appear to be of interest primarily to urbanists, it promises nothing less than the revitalization of world systems theory and an agenda of research issues of direct interest to political scientists: the politics of scale and power underlying changes in state forms, the deprivileging and hollowing out of the nation-state, the problematic mutuality of states and cities, the growing importance of non-state actors, the emergence of transnational movements, the reconfiguration of class conflict on a transnational scale, the politically constructed links of global and local places, the flows and diffusion of ideologies and ideas on a global scale, and the prospects for governance in cities engaged in "the work of globalization," to use Saskia Sassen's felicitous phrase.

Although the book's strengths lie in these theoretical enterprises, spurred by Friedmann's willingness to engage in self-reflection and debate, its weaknesses also emerge there. Many of the chapters promise more than delivered, particularly those venturing into empirical analysis. Some deficiencies stem from the frustrations of research, but these delays underscore methodological and data problems that merit serious consideration. Since the theoretical framework centers on analysis of flows of capital, labor, information, and ideas rather than of static territorial units or actors, the methodological issues and data demands are staggering. Tapping into these flows through integrative methods, such as network analysis or spatial statistics, offers approaches that are promising but ultimately unexamined here. Even prospects for drawing on counterfactuals as an analytic strategy are overlooked. Nor are the data deficits realistically acknowledged. They are overcome, to some extent, in analyses of U.S. urban hierarchies (Don Lyons and Scott Salmon) and airline connectivity between city ranks (David J. Keeling). But these are persuasive analyses precisely because they are limited; overall, the world cities theoretical framework and hypotheses remain underspecified and untested. The methodological difficulties in determining the empirical validity of world cities arguments continue to present grave drawbacks.

The world cities approach is vulnerable to reductionist thinking: It appears to treat economic trends and activities as determinative, monolithic, and universalistic factors. The authors here avoid these pitfalls by carefully specifying the localized economic cores in cities they are analyzing; the chapters by Saskia Sassen, David Simon, and Graham Todd thoughtfully assess the world cities hypothesis in the context of cities in core, periphery, and semiperiphery regions. But the volume slights economists' debates on economic growth and globalization. Little of the sparring between those stressing new trade dynamics and those highlighting changes in the nature of competition, growth processes, and production systems is evident, for example. Nor is there recognition of recent work on conditional convergence theories of economic growth that emphasize the critical role of government policies and institutions in determining how well resources are used. The implications of these arguments are clearly relevant to the world cities hypothesis; they also underscore the absence of political analysis in this framework.

Even though economists are bringing institutions, intentionality, and policy choices to the fore in accounting for growth differentials, the role of the state in both globalization and world city processes is theoretically underdeveloped. This gap is acknowledged in Paul Knox's fine Introduction; chapters by Michael Smith, Peter Ward, and Robert Beauregard, in particular, underscore the importance of contextual political factors in regulating economic trends and mediating social dualism. But political science apparently offers little to inspire or support these efforts: The citations to political science journals in this volume can almost be counted on one hand. It may well be that the Rashomon effect is especially strong in political science and that our subfield boundaries hamper significant and substantive contributions to analyses of globalization and world cities. This highly recommended book offers a challenging research agenda to those interested in overcoming these boundaries and advancing this important debate.

The Return of Culture and Identity in IR Theory. Edited by Yosef Lapid and Friedrich Kratochwil. Boulder, CO: Lynne Rienner, 1996. 255p. $49.95 cloth, $18.95 paper.

Mark Neufeld, *Trent University*

"When I hear the word *culture*," Joseph Goebbels is said to have remarked, "I reach for my revolver." While it would be excessive to suggest that the relationship of the discipline of international relations (IR) to culture has reflected an analogous level of animosity, there is ample evidence of a not-so-benign neglect of cultural issues within the disciplinary mainstream. As the editors of the volume under review note, one would be "hard-pressed to locate within the realist/neorealist ambit an explicit concern with . . . issues of identity, culture, and legitimacy" (p. 106).

That this state of affairs may be changing is the hopeful subtext of *The Return of Culture and Identity in IR Theory*. Particularly welcome is the attention to meta-theory, an orientation which allows for a critical examination of the (all too often unexamined) assumptions that inform empirical analyses. It is, no doubt, because of the sensitivity of the volume's contributors to meta-theoretical questions that they avoid the positivist-induced trap of "including" culture as a variable in a linear equation of the type "culture causes b" or "culture is causal effect of a." Rather, adopting an explicitly interpretive orientation, culture is conceived and explored as a web of intersubjective meanings through which human beings construct their world and in which their very identities are formulated.

Following an introductory overview provided by Yosef Lapid and an intriguing historical review of issues surrounding culture and identity by Yale Ferguson and Richard Mansbach, two substantive themes are addressed. Part 2 of the book focuses on critical engagements with neorealism and represents, in differing ways, efforts "to culture" IR's still dominant tradition (p. 12). The opening chapter by Alex Wendt develops his notion of "collective identity formation" as a way to reconceptualizing neorealism's anarchy problematique. Specifically, Wendt argues that international politics is about more than acting on material incentives in given anarchic worlds; it is also about "the reproduction and transformation, by intersubjective dynamics . . . of the identities and interests through which those incentives and worlds are created" (pp. 62–3).

Wendt's constructivist arguments are subjected, in turn, to critical examination by Naeem Inayatullah and David Blaney, through the lens of "cultural encounters" (as developed by Tzvetan Todorov and Ashis Nandy), and by Sujata Chakrabarti Pasic, who reflects as well on the contributions of Barry Buzan and the legacy of the cultural historians of the British School (Toynbee and Wight). The section ends with a chapter by Lapid and Kratochwil in which they conclude that neorealism's treatment and understanding of contemporary nationalism will remain inadequate in the absence of a "progressive identity-based problem shift" in its research program (p. 15).

In the third section of the book, the emphasis shifts from realism to distinctively nonrealist approaches to world politics. David Deudney's contribution takes up the ecologically inspired question of the construction of human identity in relation to its biological and geographical contexts. Deudney predicts the rise of earth identities as serious challengers to ethnic and state-based nationalisms. J. Ann Tickner, in turn, explores the relevance of recent feminist theorizing for issues of culture and identify. Given the fact that "identity has been a central concern in contemporary feminist theory" (p. 148), it is not surprising that she finds much within the feminist literature that sheds light on contemporary problems in the discipline. Perhaps most important, Tickner observes that the disciplinary mainstream has difficulties with issues of culture and identity precisely because of the way these concepts are gendered.

In chapter 9, David Campbell takes up the question of subjectivity in distinctly postmodernist terms. His case study is that of U.S. (and, more generally, Western) policy toward the Balkan crisis. Campbell argues that Western inaction in the face of obvious human rights violations must be understood in terms of the construction of the state as ontologically prior to its foreign policy. Besides being intellectually suspect, the resulting notion of sovereign states in an anarchic realm, he argues, does not provide an adequate basis for ethics in the post–Cold War world. Accordingly, strategies of deconstruction must be deployed against the state if violence is to be resisted. Campbell's arguments are both intriguing and provocative. His conclusion that "without deconstruction there might be no questions of ethics, identity, politics, or responsibility" (p. 178) is meant as a clear challenge to modernist efforts. Yet, Campbell himself allows that strategies of deconstruction can/must be supplemented by others (p. 177), raising in turn the question of whether these other strategies will also be of postmodern provenance, or whether some variant of modernist theorizing will have to be incorporated if violence is to be resisted effectively. The volume is rounded out by a consideration of the notion of citizenship, conceptualizing it as an "instituted process" fundamental to the problem of political order, followed by a concluding chapter, both by Kratochwil.

In mainstream parlance, which divides the discipline into "rationalists" and "reflectivists," the contributions to this volume would clearly fall into the latter camp. And notwithstanding the mainstream prejudice that reflectivists can be lumped together indiscriminately, the contributors are working out of quite different theoretical traditions (scientific realist, hermeneutic, postmodernist, feminist, etc.). Accordingly, there is an understandable and perhaps unavoidable element of disjointedness across the chapters. The high level of its discourse, moreover, limits the volume's potential as a textbook for all but the most advanced graduate students of IR. None of this detracts, however, from the fact that the book raises important questions and presents a series of thought-provoking arguments, the consideration of which can be only salutary for the discipline as a whole.

Nuclear Designs: Great Britain, France, and China in the Global Governance of Nuclear Arms. By Bruce D. Larkin. New Brunswick and London: Transaction Publishers, 1994. 354p. $34.95.

Weixing Hu, *University of Detroit Mercy*

The dramatic nuclear reduction measures taken by the United States and Russia after the Cold War has prompted a great deal of interest in studying the global governance of nuclear arms. Bruce Larkin's *Nuclear Designs* is a timely contribution to that study by focusing on the role of three second-tier nuclear weapon states, Great Britain, France, and China.

Their role has long been either neglected or taken for granted in international nuclear politics. Scholars and commentators have tended to examine their nuclear weapon policy and strategy separately, and very few studies have explored the commonality of these three nuclear powers in a comparative way. With the end of the Cold War, there is growing need for more involvement of these states in the global nuclear governance. Their increasing importance lies in the fact, as Larkin argues, that they "not only reinforce nuclearism—in which nuclear war is held at bay by terror and uncertain self-restraint—but also by their example complicate the case against proliferation" (p. viii). It is for this reason that the author chooses Britain, France, and China for his study.

Among the few works that treat these three nuclear weapon states together, Larkin's study stands out because of its comprehensiveness and organization. But, like other authors, Larkin first has to explain why such de facto nuclear states as Ukraine, Belarus, Kazakhstan, Israel, India, and Pakistan are not included. He argues that the three identify strongly as "nuclear weapon states," while others do not. The United States and Russia have to give serious consideration to the "second-tier uncertainty" in their arms control and reduction measures, while other nuclear states present more a nuclear proliferation issue. Larkin uses three distinguishing criteria for the second tier. First, a state must have enough weapons and delivery capability to matter. Second, it must have a declared nuclear weapon program. Third, the program must have developed deliberately from nuclear research to weapon deployment. The states with small nuclear programs, he argues, fail the first test; the second criterion excludes

India, Israel, and Pakistan; and the third eliminates Ukraine, Belarus, and Kazakhstan. These criteria simplify the selection issue for the study but raise more questions than they can answer. For instance, how should we assess a state's importance in the global governance of nuclear arms? How helpful is it to separate declared and nondeclared nuclear states? Does the distribution of nuclear capabilities among states really matter as we move toward more nuclear management?

The main contribution of this book is its comparative examination of the three states' nuclear programs and policies through capability and scriptic analysis. After an overview of the British, French, and Chinese nuclear programs, chapters 3 through 6 examine the arms control position of each. Solid empirical work is presented on their policies regarding test bans and nonproliferation, nuclear arms reduction, verification, and coordination and transactions among nuclear weapon states. Chapters 7 through 9 address the why and why not of British, French, and Chinese commitments to nuclear weapons.

Unlike the deterrence approach to the nuclear weapon problem, British, French, and Chinese nuclear policies are treated here as a broader issue than national security strategy. It is especially appropriate to trace the source of those policies to the domestic political base of their independent nuclear deterrents and the central governance of nuclear weapons in the three states. In building a comparative framework, the author focuses on the following questions: (1) How was the nuclear program undertaken? (2) What are threat perceptions, declared purposes of nuclear programs, and arms control positions? (3) Who in the state favors and who opposes nuclear forces? How are nuclear weapons governed? Does the internal political contest to govern affect positions on nuclear forces? Given such a broad topic area, the author's comparative framework does not seem adequate to address the why and why not issues of these three second-tier nuclear states; it concerns more the how than the why.

Another matter that is raised but not resolved is the role of the three states with respect to proliferation. Is there any linkage between their nuclear programs and proliferation? Following the footsteps of the superpowers, Britain, France, and China acquired nuclear weapons for security, and the political leadership in the three states still strongly advocates nuclearism. This makes the case, Larkin argues, for nonnuclear states to follow suit. Yet, nuclear motivation is complex, and the judgment on the link between the three second-tier nuclear states and proliferation is arbitrary. From the perspective of nuclear arms and nuclear security, the three states should take no more blame for proliferation than the superpowers. The behavior of Britain, France, and China clearly indicates a learning curve in their nonproliferation policy, and they are now facilitators, rather than spoilers, of the global nuclear nonproliferation regime.

Larkin argues strongly for nuclear abolition. He also implicitly suggests some mechanisms for global nuclear management and abolition. The three second-tier states, in his view, have a special part to play as the world moves toward nuclear management and disarmament. More than any others, they could bring their weight to bear on Russia and the United States to reduce even more or abolish nuclear weapons. To potential nuclear powers, the three could make a compelling case for renouncing nuclear deployment and aspirations. As their current nuclear policies indicate, however, it may not be practical to expect the three to play such a special role in global nuclear governance. London, Paris, and Beijing strongly reject the notion that they have any special responsibility to change the status quo. In this sense, Larkin's study is valuable for contemplating the path to a future nuclear management regime. It broadens our thinking on global nuclear governance and raises some questions of central importance for further research on the issue.

Reputation and International Politics. By Jonathan Mercer. Ithaca, NY: Cornell University Press, 1996. 236p. $34.95.

Stephen Peter Rosen, *Harvard University*

This book draws upon the work done in experimental psychology to develop a set of hypotheses about how and in what ways leaders of states do or do not acquire reputations that exist in the minds of other statesmen. The result is a book full of fascinating and suggestive insights into the cognitive processes relevant to international relations. Mercer compels us to accept the proposition that reputations do not simply reflect past behavior. Resolute leaders, he convincingly shows, do not always gain reputations for resolve, and irresolute leaders do not always gain a reputation for weakness. Mercer's book should be read as arguing that it is difficult for statesmen to create the reputations they seek to create, a proposition the author clearly proves. Yet, the book can be read as making a stronger claim: Reputations rarely develop in the way predicted by standard deterrence theory, and they are not worth fighting for. This claim seems less well established by Mercer's theory or cases.

Mercer begins by asking what reputations are and how leaders acquire them. He quickly and correctly moves to the position that reputations exist in the minds of observers, not in our own minds, and consist of judgments about the character of actors that lead observers to believe a leader will behave in the same way in a variety of circumstances. Mercer then links this conception of reputation to the work done on attribution theory in experimental psychology, that is, subjects are given descriptions of the behavior of another person and then are asked to explain that behavior. The explanations are grouped into those that attribute the behavior to the situation ("he ran away because his life was in danger") and those that attribute the behavior to the person's disposition ("he ran away because he was a coward"). The psychologists found that subjects tend to attribute unexpected and desirable behavior of members of out-groups, from the standpoint of the experimental subject, to the situation, not to the positive disposition of the out-group member. Negative actions of members of out-groups that were expected and undesirable were attributed to their personal disposition. In other words, people tend to think the worst of members of out-groups; they explain bad behavior in terms of permanent bad dispositions and explain away good behavior by citing compelling external circumstances.

The link is then made to international politics. There, from the standpoint of a national leader, foreign adversaries are clearly members of out-groups. So also, argues Mercer, are allies, at least sometimes. In a crisis, if an adversary refuses to back down in response to the policies of a national leader, s/he is clearly acting in a way that is expected and undesirable; thus, leaders should attribute that behavior to the person's negative and fixed disposition. It then is possible that the next crisis will be seen as in some way the same as the first crisis. If it is, then the crises and crisis behavior may, in Mercer's words, be linked or interdependent. If the behavior of the adversary is attributed to permanent dispositional factors and if the crises are interdependent, then the adversary will have gained a reputation. Mercer notes that the theory is silent on just which dispositional factors will be attributed to the adversary, but since the adversary is not backing down, Mercer argues that it is plausible the adversary's behavior will be attributed to resolve. The adversary has then gained a reputation for resolve. In this, Mercer's hypotheses predict the same outcome as deterrence theory. But if the adversary gives in, Mercer argues that s/he will not gain a reputation for weakness. Why? The behavior was unexpected and desirable. Hence, it is explained away by reference to the transient situation. The next time, the foreign leader concludes, the adversary may well be tough, because the situation may be different. Therefore, adversaries can rarely acquire a reputation for weakness, but they can gain a reputation for toughness. By a similar process, Mercer concludes that allies can rarely gain a reputation for resolve (such behavior is desirable, but perhaps not unexpected), but they can gain a reputation for weakness.

What should we make of this theory as theory? First, it is based on experiments involving isolated subjects, largely U.S. college sophomores, who are asked to explain behavior described in a story. Do people in groups develop explanations of out-group behavior in the same way as isolated individuals? Do college sophomores display the same level of sophistication in developing explanations of complex foreign behavior as do statesmen? Are allies members of out-groups or not? What about the attributions of a leader who is neither friend nor foe? Do experiments about how subjects *explain* fictional behavior carry over into cases in which leaders *predict* behavior in settings where wrong predictions have serious consequences. Mercer is aware of these and other points, and he says that the proof of his theory is in the empirical tests.

Mercer then examines real-world crises to see whether reputations develop within and across them. He finds much evidence to support his hypotheses. In his favor, when the Germans backed down in the resolution of the first Moroccan crisis, the French did not attribute their behavior to weakness and did not expect the Germans to back down the next time. Indeed, they feared the Germans would seek revenge. Mercer finds no evidence that the Germans expected the French to back down in the first Moroccan crisis because they backed down to the British at Fashoda, and he does find that the Germans noted the difference in the situation of France in the two crises. Austria stood up for its German ally in the first Moroccan crisis, and the Germans concluded the Austrians might defect in the next crisis. Rather than unambiguously conclude from German steadfastness in the crisis involving the annexation of Bosnia-Herzegovina that the Germans were tough, the French combined a view of the Kaiser as fearful with a view that Germany was resolved on war. The Austrians were supported by Germany in their annexation, but they concluded that their ally wanted to treat them as a subordinate power.

Mercer is on strong ground in claiming that objective actions lead unproblematically to reputations and predictions for more such behavior. He provides much fascinating information that leaders acquire beliefs about what other leaders think of them and act on those beliefs, not on the real judgments of other leaders. But does it prove Mercer's thesis? The evidence is not so clear. The judgments of foreign leaders about future German behavior, for example, are mixed, but so are the actions of Germany: It stood tough and then backed down in the first Moroccan crisis, but it stood tough and did not back down in the annexation crisis. Then it backed down in the second Moroccan crisis. Germany did not have to establish a reputation for being willing to back down because it often was not so willing? The Germans did not fully trust the Austrians despite their support, yet there was ample reason for them to be edgy about Austria. Does this mean that reputations for resolve cannot be established among allies? The answer is not so clear.

Democracy Imposed: U.S. Occupation Policy and the German Public, 1945–1949. By Richard L. Merritt. New Haven and London: Yale University Press, 1995. 452p. $40.00.

Gerard Braunthal, *University of Massachusetts, Amherst*

Few political scientists have raised the intriguing question of whether one advanced industrial state, in this instance a victorious United States, can impose social change on another advanced industrial state, in this instance a defeated Germany after World War II. The evidence is clear that the United States and West European countries have been successful, with few exceptions, in transplanting their capitalist economic system to the Third World and, more recently, to the former Soviet Union and East European states; often with devastating social consequences. There is less evidence of a successful transplantation from one advanced industrial state to another within the First World.

Richard L. Merritt, a professor of political science and research professor in communications at the University of Illinois at Urbana-Champaign, provides such evidence by studying the United States and Germany in the postwar era. He contends that U.S. policymakers intended through their occupation policies to change German society in order to enhance the prospect of a democratic and peaceful Germany. To find out how successful such policies were, Merritt concentrates on the reaction of ordinary Germans to the directed social change rather than to the reaction of the country's emerging elites. This focus on the German people parallels that of Daniel Goldhagen's controversial study (*Hitler's Willing Executioners: Ordinary Germans and the Holocaust*, 1996), but the two volumes cover two different historical eras and use different methods. Given the totalitarian nature of the Nazi regime, Goldhagen could not base his study on any scientific public opinion polls and thus had to rely on other data; Merritt bases his study on the extensive surveys conducted by the Office of Military Government, United States (OMGUS), and other surveys begun in October 1945.

Portions of this book have appeared previously in two edited works by Anna J. Merritt and Richard L. Merritt (*Public Opinion in Occupied Germany: The OMGUS Surveys, 1945–1949* [1970] and *Public Opinion in Semisovereign Germany: The HICOG Surveys 1949–1955* [1980]) and in some of their other publications. But as the polls are incorporated into a new framework of analysis, the repetition of data can be justified in this instance. Moreover, to see whether ordinary Germans have changed their views since 1945–49, Merritt cites surveys for more recent decades. Such comparisons over time provide the reader with a valuable set of data and give the book an historical sweep not evident in the "1945–1949" subtitle.

In the introductory chapters, Merritt deals with U.S. wartime images of Germany and U.S. occupation policies. He states that the effect of Nazism on the German population not only was visible in surveys carried out between November 1945 and April 1947, when an average 48% of respondents concurred with the statement that National Socialism was "a good idea badly carried out" (p. 97), but also is still evident in the 1990s. A national survey in 1992 showed that, despite the initial U.S. attempts to reeducate Germans, merely 17% of respondents (the democrats) said that National Socialism had only bad qualities, while 36% said it had more bad than good qualities. Yet, 42% said it had both good and bad qualities, and 2% (the unreconstructed Nazis) responded that it had more good than bad (p. 99).

A similar ambivalence toward the Nazi past is evident in surveys dealing with public attitudes toward Jews, one focus of the cited Goldhagen book. In the occupation years, Merritt notes, one-third to two-fifths of survey respondents "were either overtly anti-Semitic or inclined toward anti-Semitism" (p. 138). Attitudes changed little over the next decades. An EMNID survey in 1989 indicated that more than half of those queried were very (4%), somewhat (10%), or a bit anti-Semitic (40%); 46% showed no trace of anti-Semitism (pp. 138–9).

Most Germans approved the Nuremberg trials at first but were less supportive when subsequent trials included soldiers who had "merely followed orders" and when occupation authorities failed to differentiate between hard-core Nazis and nominal party members. The bulk of respondents regretted the Nazi past but felt little personal responsibility or guilt for its heinous crimes.

In assessing U.S. attempts to build a democratic system, Merritt deals with the German attitudes toward the U.S. military, the newly formed public school system, the information centers and the media, the youth activities, and other programs intended to socialize Germans to democracy. He concludes that most Germans, but not always the German policymakers, welcomed the U.S. imposition of democratic values. Too often, however, the U.S. policymakers, lacking clarity in goals and policies, made compromises rather than drastic social changes. Nevertheless, Merritt sees the U.S. influence as decisive in imposing a democratic system on the West Germans. This reviewer wishes Merritt had made some comparisons to U.S. postwar policy toward Japan, because his model of social change needs to be tested cross-nationally.

Merritt correctly asserts that few Germans wanted to return to the discredited Nazi system, and relatively few since then have supported neo-Nazi parties. Yet, he does not emphasize sufficiently the Germans' rejection during the post-1945 era of many U.S. proposals, such as the ones to reshape the stratified school system and the encrusted bureaucracy. Thus, the concept of "imposing" democracy had its limitations, especially in a state that had already passed through a democratic phase during the Weimar period. Most of the population could remember the Weimar Republic, during which democratic (and, alas, antidemocratic) values had flourished and in which Germany had one of the most democratic constitutions in the world.

These reservations aside, the author, in sophisticated analyses and a felicitous style, has made an important contribution to an area of scholarship that needs further exploration. Our understanding of a key transition period in German history is enhanced by this book at a time when the unified country has become a powerhouse in a fractured Europe.

The State of the European Union, Volume 3: Building a European Polity? Edited by Carolyn Rhodes and Sonia Mazey. Boulder, CO: Lynne Rienner, 1995. 520p. $49.95.

Michael Baun, *Valdosta State University*

This third volume in a biennial "yearbook" series sponsored by the European Community Studies Association covers 1993–94 and is of particular interest for its focus on policy and institutional developments after final ratification of the Maastricht Treaty on European Union. While most of the 21 chapters are written by political scientists and international relations scholars, the book also includes contributions from economists and a law professor. About half the authors are European, which provides an opportunity for assessing the current state of a long-standing theoretical divide in EU studies between U.S. and European scholars.

In the introductory chapter the editors state that the volume's main purpose is to place recent EU developments in theoretical perspective. For the most part, this is done successfully as the contributing authors, in chapters on such diverse topics as EU monetary union, regional policy, enlargement, and external relations, seek to link their case studies to broader theoretical debates about integration and the emerging European polity. A useful theoretical framework for the empirical chapters is provided by both the introductory chapter and Caporaso and Keeler's survey of the historical evolution of integration theory.

Overall, the book provides strong evidence that EU studies are increasingly dividing into two separate branches, each with its own predominant theoretical focus. The first branch concerns the "high politics" of European integration, including treaty-making on such prominent issues as monetary union, foreign and defense policy, enlargement, and institutional reform. The second involves evaluation of policymaking and governance within established institutional structures. While the former branch is dominated by "grand theory" debates between intergovernmentalists and neofunctionalists, the latter increasingly applies comparative politics and public policy theories originally developed for national-level analysis, including theories of policy networks, interest groups, and federalism.

The two branches are not unrelated. As the editors seem to admit, and as many of the individual chapters bear out, the grand theory debate appears to have been won by the intergovernmentalists, although some of the authors argue for what Wood (p. 239) terms a "refined" intergovernmentalism that takes into account both domestic interests and structures and the international institutional environment. As a result, many scholars sharing the neofunctionalist and pluralist bias against the state have gravitated to sectoral policy studies, where they are able to generate more convincing evidence of the emergence of a multilevel European polity, with the diffusion of state power upward to the EU level and downward to the regions, as well as of the important role of supranationality and a variety of nongovernmental and transnational groups.

The individual chapters are generally well written and argued, although some are very densely detailed accounts of policymaking in particular sectors. Some chapters make more important theoretical points than others. The chapter by Chilton notes that defense integration since the end of the Cold War has largely proceeded ahead of EU political integration and within the separate institutional context of NATO, providing intriguing evidence that defense integration has developed its own logic separate from the functionalist one of economic integration and may not necessarily spill over into EU political union. The chapter by Deeg on German federalism and the EU argues that two simultaneous trends are observable: (1) the Europeanization of German federalism, with the EU becoming a third actor in the German federal system, and (2) the federalization of the EU, with the growing involvement of the German Länder in policymaking at the EU level. Ironically, he argues, the decentralization of the EU via increased application of the subsidiarity principle could actually promote integration by making it more democratic and accountable.

Some of the contributors differ in their interpretation as to who has the power in EU decision making or as to where that power is gravitating. Peterson and Fuchs, in their respective chapters on EU research and telecommunications policies, argue that the commission is a powerful and autonomous actor. Zito and Pollack, however, in their respective discussions of EU environmental and regional policies, stress the important "gatekeeper" role of national governments and the Council of Ministers. Accepting an intergovernmentalist framework, Pollack argues that the challenge for EU theorists lies "in understanding the conditions under which member states consent to an increase in supra- and subnational power, and the conditions under which the Commission and subnational governments are able to exercise that power to influence policy outcomes" (p. 385).

The argument for conceptualizing two distinctive levels of EU politics—a "high politics" level that is inherently intergovernmental and a "low politics" level of detailed regulation in established policy sectors, with a key role for the commission and interest groups—is made by Mazey and Richardson in their chapter on the emerging European policy style. It is also contained in the chapter on Germany and European monetary union (EMU) by Smith and Sandholtz, who describe a two-stage policy process in which the phase and institutional setting of integration largely determines which political actors have power. In their case, the EMU treaty-negotiation phase was dominated by the German federal government, in particular Chancellor Kohl; once the treaty was concluded, however, the focus shifted to the politics of ratification and day-to-day policymaking, allowing for the much greater role of the Bundesbank and other domestic institutions and interests.

A notable gap in the coverage of this volume, with the exception of Partan's chapter on the justiciability of the subsidiarity principle, is discussion of the European Court of Justice's role in the integration process. This omission is particularly curious in light of heightened interest in the court in recent theoretical work on the EU. Overall, though, this book is a valuable and welcome addition to the literature on the EU and integration, and it should be of considerable interest to any serious scholar of EU politics.

The Spread of Nuclear Weapons: A Debate. By Scott Sagan and Kenneth Waltz. New York: W.W. Norton, 1995. 160p. $16.95.

Richard Rosecrance, *University of California at Los Angeles*

Why have nuclear weapons? People used to believe that the relative stability of the Cold War was due to the presence of bipolarity and the existence of nuclear weapons on both sides. Bipolarity has already been questioned as a stabilizing force. But perhaps nuclear weapons were necessary to deter a Soviet attack on Western Europe. This conclusion, however, has also been queried. Even in the absence of nuclear weapons, a Russian invasion would have unleashed another world war, a war Moscow could not have won. In fact, despite all the argumentation since the first nuclear detonation on July 16, 1945, we still cannot unambiguously conclude that nuclear weapons are a net benefit to world stability.

In theory, nuclear weapons may be useful to prevent conventional as well as nuclear strikes. In which case, they may economize on the need to spend on conventional forces. But if nuclear weapons only deter the use of an opponent's nuclear force, nations must also possess a large stock of conventional weapons to deter nonnuclear attack.

Nuclear weapons have to surmount two hurdles to be useful for deterrent purposes: (1) The conditional probability of nuclear use must be high, if a given provocation, X, occurs; (2) the probability of use of nuclear weapons must be very low (or nonexistent) if X does not occur. As Albert Wohlstetter and many others (including Scott Sagan) have shown, resolute attempts to solve one problem exacerbate the other. On the one hand, if one has made sure that no accident can ever occur, then the "dud rate" in a real nuclear crisis will be high. On the other hand, if one places weapons in a state of high readiness, then accidents are likely to happen (and they have happened). In any event, as time has passed, the credibility of (1) has greatly declined.

In this interesting debate, Kenneth Waltz disagrees; he believes that the conditional probability of nuclear use by new nuclear states is high but that this need not raise problems of accident or unauthorized use. Sagan contends that the problem of accident has not been solved even for large and competent nuclear powers like the United States and would be far worse for new, small nuclear states.

Trends in nuclear declaratory strategy seem to raise more questions about the continuing credibility of nuclear use even for extremely powerful nations like the United States. In the 1980s a Department of Defense study group questioned whether U.S. strategic tasks might not better be performed by extremely accurate conventional weapons, obviating the need for nuclear strikes.[1] Thomas Schelling underscored the problem when he noted that there was now a de facto "nuclear taboo" which made nuclear initiation difficult for all powers.[2]

The historical record shows that nonnuclear states have sometimes attacked the forces of nuclear powers, as did North Korea, China, and North Vietnam, without engendering a nuclear reprisal. In 1982 Argentina seized islands owned by Britain, a nuclear state, but Britain did not bomb Argentina. In 1973 Syria and Egypt attacked Israel, a state they knew to be armed with nuclear weapons; nor were these attacks merely peripheral ones, as is sometimes said. The Syrian forces broke through Israel's Barak Brigade and moved down into Israel from the Golan Heights. Iraqi Scud missile attacks rained on the center of Tel Aviv in 1991 without reprisal (and certainly without nuclear reprisal). With each nonresponse, the credibility of nuclear retaliation declines farther.

Waltz seems to believe in the inevitable spread of weapons. Yet, the issue remains open. Today, nations are denuclearizing more rapidly than they are becoming nuclear powers. South Africa, Argentina, and Brazil are notable recent cases of nuclear reversal. North Korea, South Korea, and Taiwan have held back. Canada, Australia, Sweden, Switzerland, and, indeed, all developed West European countries (except for Britain and France) have considered the nuclear option and rejected it. Japan occasionally talks about becoming a nuclear power but has refrained from doing so. Ukraine, Belarus, and Kazakhstan have recently denuclearized, despite foreign advice to retain their capabilities. Germany is not going to become nuclear. The World Court now holds that initiation of nuclear weapons is in violation of international law except in the very narrow case in which, without their use, national extinction would occur.

The plain fact is that nuclear credibility is low and getting lower. If it were high, as Sagan amply shows, it would pose further dangers of accidental or unauthorized use which already cannot be neglected. Most nations are aware of the problem posed by the increasing barriers to nuclear initiation, and some of them have sought other alternatives (e.g., developing chemical or biological warfare strategies), but these have not solved the problem of declining credibility of weapons of mass destruction.

---

[1] K. Smith, "Economic and Technological Implications of Discriminate Deterrence," *Bulletin of Peace Proposals* 20 (June 1989):205–17.

[2] "The Conventional Status of Nuclear Weapons," UCLA Center for international Relations Working Paper, 1992.

Most nations have come to recognize that conventional forces will not be obviated by nuclear ones and, in particular, that a nuclear response to a conventional attack is now nearly unthinkable. Hence, in adding nuclear weapons to an absolutely necessary conventional force, nations create major opportunity costs. How much will nations spend upon a force they cannot or will not use? This is a question for the United States today as well as for other countries. Developed nonnuclear states progressed much more rapidly in the 1980s than those that devoted large funds to their nuclear forces, like the old Soviet Union and Reagan's United States. Even China, a nuclear power, has spent relatively little on nuclear weapons and their associated long-range delivery capabilities; it has concentrated its energies on investment in civilian production and conventional defense.

In sum, the spread of nuclear weapons is no longer inevitable. With the passage of time, nations have begun to wonder whether nuclear weapons are in fact usable; if they possessed genuine and hair-trigger usability, they would present major problems of accident. That is why, despite Waltz, there is a net tendency away from the spread of nuclear weapons. As the question arises in terms of realistic technological alternatives, many more nations are deciding not to go nuclear than to acquire weapons. And some nations that previously opted for nuclear are changing their minds. There will be more nuclear reversals in the future.

Democracy and Its Discontents: Development, Interdependence, and U.S. Policy in Latin America. By Howard J. Wiarda. Lanham, MD: Rowman & Littlefield Publishers, 1995. 367p. $68.50 cloth, $27.95 paper.

John D. Martz, *Pennsylvania State University*

During the past two decades, few scholars with expertise in Latin American affairs have followed U.S. policy and the course of inter-American relations as closely as Howard Wiarda. He has maintained a host of both formal and informal connections with Washington policymakers while keeping a foot in the academic world as well. His writings, which have appeared in a number of both obscure and well-known outlets, have been republished in periodic anthologies, beginning in 1984 with his *In Search of Policy: The United States and Latin America*. This is his fourth such compilation—the first, as he notes, which reflects the post–Cold War reconceptualization of U.S. policy. The contents include either excerpted or complete chapters from several books, along with the usual journal sources.

This collection is divided into five parts, followed by a conclusion. The first incorporates two chapters providing a useful overview of U.S. policy from Reagan through Bush and into the early Clinton years. Each appeared previously in book form: the first in this reviewer's *United States Policy in Latin America: A Decade of Crisis and Challenge* (1995) and the second in Wiarda's *The End of the Cold War: Continuing U.S. Policy Interests and Options in Major World Regions* (1995). The second section presents informative discussions of what Wiarda properly terms complex policy issues. The four topics are democratization and human rights; think tanks vis-à-vis U.S. Latin American policy; elections and democracy throughout the hemisphere; and the domestic politics of NAFTA. Of these, the greatest source of new and fresh information is on the Washington think tanks. While those inside the Beltway will find the treatment familiar, many other students of policymaking will benefit greatly from the data and assessment presented. The other selections in this section are well-argued presentations but cover relatively familiar ground.

"Theory and Interpretation," which constitutes the third part, is based on Wiarda's early writings in comparative politics rather than foreign policy and international relations. Thus, he questions and probes the nature of Latin American democracy through reconsiderations of centralism, authoritarianism, and bureaucratic patrimonialism, enriched by a brief soupçon of Dominican politics as personified by Joaquín Balaguer, who finally stepped down from the presidency in mid-1996 as he approached his ninetieth birthday. Taken as a whole, this grouping of selections serves to underline the importance of understanding Latin America on its own political and conceptual terms. Otherwise, it is somewhat peripheral to the rest of the volume.

Part four consists of five country-specific case studies. In a sense they are asymmetrical, including both brief and extended treatments drawn from articles, chapters, and conference papers. Notwithstanding the merits of individual pieces, there is perhaps inevitably an absence of thematic continuity to tie them together. To be sure, each in its own context helps to point the way toward questions about the future course of political transitions in Latin America. As such, they help lead the reader into the two last chapters. This is notably true of the piece setting forth future policy options for U.S. relations with the Caribbean, along with an all-too-brief review of political reform in the Southern Cone which broadens into a statement about the new democracies and their linkage to development.

In his conclusion, Wiarda views the December 1994 Miami Summit of Western Hemisphere heads of state from the initial aura of euphoria to the later wave of pessimism. This presumably presents the author's thinking as of early 1995. Future publications will doubtless carry us farther into the unending and kaleidoscopically refracted image of U.S. policy in Latin American affairs.

As a totality, these essays are stimulating and provocative but intellectually unfulfilling. Wiarda has brought together selections of his studies and observations which are deeply embedded in the world of Washington policymaking yet richly informed by a discerning and experienced scholar. There may be those who will differ with various of his political judgments, but none will deny the fertility of his writings. Certainly he is justified in pulling together this compendium. At some future point, however, one hopes for an extended, systematic, and tightly organized book in which Wiarda synthesizes his views and puts forward a detailed statement of his policy recommendations. The result is likely to be unsympathetic toward a generation or more of policymakers presumably knowledgeable about Latin America. Yet, it also would constitute a forum in which Wiarda could provide guidance toward a more rational and constructive approach to Latin America as a new century nears.

M
IN
NE
SO
TA

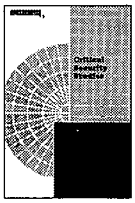
MICHAEL J. SHAPIRO

Violent Cartographies

Mapping Cultures of War

"Shapiro manages to weave together philosophical investigations, history, military analysis, and film criticism into one free-flowing and wide-ranging narrative." *Michael Hardt, Duke University*

$19.95x paper (2921-8) $49.95xx cloth (2920-X) 264 pages March



KEITH KRAUSE AND MICHAEL C. WILLIAMS, EDITORS

Critical Security Studies

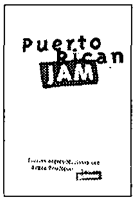
Concepts and Cases

"A text of central relevance to the study and teaching of international relations widely defined." *Peter Lawler, University of Manchester*

"An exceptionally strong set of essays. . . . This is a pathbreaking book!" *Steve Smith, University of Wales, Aberyswyth*

$22.95x paper (2857-2) $57.95xx cloth (2856-4) 408 pages March

Borderlines Series, Volume 8



FRANCES NÉGRON-MUNTANER AND RAMÓN GROSFOGUEL, EDITORS

Puerto Rican Jam

Rethinking Colonialism and Nationalism

Puerto Rican Jam elaborates alternatives to dominant postcolonial theories, and includes essays written from the perspectives of groups that are not usually represented, such as gays and lesbians, youth, blacks, and women.

$19.95x paper (2849-1) $49.95xx cloth (2848-3) 256 pages August

Also of interest

CARY NELSON

Will Teach for Food

Academic Labor in Crisis

Foreword by Barbara Ehrenreich

Will Teach for Food provides a clarion call to academic workers, summoning them to take action against the continued decline in working conditions on American campuses.

$19.95 paper (3034-8) $49.95xx cloth (3033-X) 248 pages April

Cultural Politics Series, Volume 12

at bookstores
or from

**University of
Minnesota Press**
111 Third
Avenue South
Suite 290
Minneapolis, MN
55401-2520
612-627-1970

The *Politics* of Aristotle

TRANSLATED, WITH INTRODUCTION, ANALYSIS, AND NOTES BY PETER L. PHILLIPS SIMPSON

"This translation is of high quality . . . precise and follows the Greek without sacrificing readability. . . . The only available translation which enables students to read the *Politics* in a way which many scholars believe makes the best sense of Aristotle's argument."—Fred D. Miller Jr., Bowling Green State University

Restores the *Politics* to its logical organization and coherence and places it in its proper philosophical context.

320 pp. $39.95 cloth / $12.95 paper

Eroding Military Influence in Brazil

Politicians against Soldiers

WENDY HUNTER

"Convincingly challenges the conventional wisdom that military tutelage in new democracies will necessarily continue or get worse."
—J. Samuel Fitch, University of Colorado

A careful analysis of how a weak democratic regime expanded its newly acquired autonomy relative to a once-powerful military, suggesting a more optimistic view of civilian democratic rule in Latin American.

260 pp. $39.95 cloth / $18.95 paper

The Vietnam Lobby

The American Friends of Vietnam, 1955–1975

JOSEPH G. MORGAN

"Breaks new ground, is well researched and documented, and will be a welcome addition to [scholarship on] Vietnam and the role of organized interest groups in American foreign policy."—Larry Berman, University of California, Davis

Joseph Morgan shifts the focus away from the much-examined antiwar demonstrations that took place in America to concentrate instead on the actions of those who endorsed U.S. intervention in Vietnam.

250 pp. $39.95 cloth

at bookstores or by toll-free order

The University of North Carolina Press

Chapel Hill • Phone (800) 848-6224, Fax (800) 272-6817
http://sunsite.unc.edu/uncpress/

75 years

POLITICAL SCIENCE FROM CHICAGO

The Long Affair

THOMAS JEFFERSON AND THE FRENCH REVOLUTION, 1785-1800

Conor Cruise O'Brien

"In O'Brien's view, Jefferson was far from the intrepid egalitarian he's been made out to be. . . . In O'Brien's opinion, it's time that Americans face the fact that Jefferson, long seen as a champion of the 'wronged masses,' was a racist who should not be placed on a pedestal in an increasingly multicultural United States."
—*Boston Phoenix*

"O'Brien traces the roots of Jefferson's admiration for the revolution in France but notes that Jefferson's enthusiasm for France cooled in the 1790s, when French egalitarian ideals came to threaten the slave-based Southern economy that Jefferson supported....[A] revisionary interpretation of Jefferson."
—*Library Journal*

Cloth $29.95 360 pages 12 halftones

Public and Private in Thought and Practice

PERSPECTIVES ON A GRAND DICHOTOMY

Edited by Jeff Weintraub
and Krishan Kumar

"Not only is the subject of this anthology very important but the conception and design are quite intelligent. The editors are right about the protean, as opposed to unitary, character of the public/private opposition. The contributors make a distinguished group, and Weintraub's introductory essay is thoughtful and well-written, usefully delineating a four-part typology of public/private distinctions."
—Nancy Fraser, New School for Social Research

***Paper $19.95 344 pages**

Morality and Society series

The Pursuit of Absolute Integrity

HOW CORRUPTION CONTROL MAKES GOVERNMENT INEFFECTIVE

Frank Anechiarico
and James B. Jacobs

"Although it professes to be a study of the impact of corruption controls on government, this book is really a study of public administration, and one of the best in recent years. It helps restore the grand tradition of Leonard White, Sayre and Kaufman, Simon, Waldo, and Wildavsky while managing to confront the special problems of the 1990s."—Theodore J. Lowi, Cornell University

Cloth $24.95 296 pages

Studies in Crime and Justice

The Legacy of Rousseau

Edited by Clifford Orwin
and Nathan Tarcov

By exploring polarities articulated by Rousseau—nature versus society, self versus other, community versus individual, and compassion versus competitiveness—these fourteen original essays show how his thought continues to shape our ways of talking, feeling, and thinking.

***Paper $17.95 296 pages**

The Socially Responsive Self

SOCIAL THEORY AND PROFESSIONAL ETHICS

Larry May

"May has given us something we very much need—a spirited, intelligent, and psychologically realistic moral and social theory that combines the communitarian emphasis on the social dimensions of morality with the liberal ideals of autonomy, integrity, and a plurality of goods."
—Mark Johnson, University of Oregon

***Paper $15.95 220 pages**

**Cloth edition available*

NOW IN PAPER

Self-Rule

A CULTURAL HISTORY OF AMERICAN DEMOCRACY

Robert H. Wiebe

"No one can deny the depth of [Wiebe's] scholarship, the breadth of his learning, the richness of his imagination."
—Gordon S. Wood, *New Republic*

Paper $15.95 332 pages

Socrates and Aristophanes

Leo Strauss

"Strauss gives us an impressive addition to his life's work—the recovery of the Great Tradition in political philosophy. . . . [Strauss] translates it into the basic problem of the relation between poetry and philosophy, and resolves this by an analysis of the function of comedy in the life of the city." —Stanley Parry, *National Review*

Paper $15.95 332 pages

Spinoza's *Critique of Religion*

Leo Strauss

"A substantial contribution to the thinking of all those interested in the ageless problems of faith, revelation, and reason."
—*Kirkus Reviews*

Paper $15.95 334 pages

The Myth of Democratic Failure

WHY POLITICAL INSTITUTIONS ARE EFFICIENT

Donald A. Wittman

"It is useful that we have economists such as Wittman . . . who push their enterprise to maximal limits."—James Buchanan, *Times Literary Supplement*

Paper $16.95 240 pages

American Politics and Political Economy series

THE UNIVERSITY OF CHICAGO PRESS

5801 South Ellis Avenue, Chicago, Illinois 60637 Visit us at http://www.press.uchicago.edu

PITTSBURGH
Presents

Shaping Suburbia
How Political Institutions Organize Urban Development
Paul G. Lewis
"Lewis makes a compelling theoretical argument that institutional choices have important social and economic consequences."—Carl Abbott
Cloth $44.95 / Paper $19.95

Edmund Burke and India
Political Morality and Empire
Frederick G. Whelan
"[A] thoughtful and definitive reading of Burke's moral indictment of imperialism in India."—Isaac Kramnick. "Whelan not only makes sense of Burke on India, but he also uses the occasion for a searching and thoughtful review of Burke's political philosophy as a whole."—Harvey C. Mansfield
Cloth $49.95

The Enigma of Automobility
Democratic Politics and Pollution Control
Sudhir Chella Rajan
"Provides an innovative approach to an important question of public policy."—Michael Margolis
Cloth $44.95 / Paper $19.95

Reconceiving Liberalism
Dilemmas of Contemporary Liberal Public Policy
Oren M. Levin-Waldman
Provides a rare practical application of political theory to contemporary public policy problems; in so doing, it offers a path to reconnect liberalism with the values of American society.
Cloth $44.95 / Paper $19.95

The Politics of Garbage
A Community Perspective on Solid Waste Policy Making
Larry S. Luton
"Anyone seriously interested in environmental policy must read this book. Anyone who teaches environmental policy must include it in their courses."—Rosemary O'Leary
Cloth $49.95 / Paper $22.95

Risking Free Trade
The Politics of Trade in Britain, Canada, Mexico, and the United States
Michael Lusztig
"A fresh and inventive argument. Its distinctiveness will allow it to be seen and heard within a very crowded literature."—G. John Ikenberry
Cloth $49.95 / Paper $22.95

Freeing the Hostages
Reexamining U.S.-Iranian Negotiations and Soviet Policy, 1979-1981
Russell Leigh Moses
"Fascinating"—Bruce Laingen
Cloth $45.00

Shame and Humiliation
Presidential Decision Making on Vietnam
Blema Steinberg
"innovative in its interpretation, carefully argued, well researched, and engaging to read"—Janice Stein
Cloth $40.00 / Paper $17.95

Understanding Attitudes About War
Modeling Moral Judgments
Gregory G. Brunk, Donald Secrest, and Howard Tamashiro
"highly original"—Robert Y. Shapiro
Cloth $45.00 / Paper $19.95

National Elections and the Automony of American State Party Systems
James Gimpel
"Gimpel's book is a breath of fresh air."—U.S. Representative Steve Schiff
Cloth $45.00 / Paper $19.95

Debt Wish
Entrepreneurial Cities, U.S. Federalism, and Economic Development
Alberta M. Sbragia
"a sweeping reinterpretation of U.S. political economy"—Paul E. Peterson
Cloth $49.95 / Paper $22.95

Public Spirit in the Thrift Tragedy
Mark Carl Rom
Cloth $49.95 / Paper $22.95

Gender Bias and the State
Symbolic Reform at Work in Fifth Republic France
Amy G. Mazur
Cloth $49.95 / Paper $22.95

Steel, State, and Labor
Mobilization and Adjustment in France
Anthony Daley
Cloth $49.95 / Paper $22.95

Continuity and Disruption
Essays in Public Administration
Matthew Holden, Jr.
Cloth $45.00

The American Impasse
U.S. Domestic and Foreign Policy After the Cold War
Edited by Michael Minkenberg and Herbert Dittgen
Cloth $49.95 / Paper $22.95

Negotiating Democracy
Transitions from Authoritarian Rule
Gretchen Casper and Michelle M. Taylor
Cloth $44.95 / Paper $19.95

Privatization and Political Change in Mexico
Judith A. Teichman
"Essential reading to anyone interested in understanding Mexico's transformation in the 1990s."—Rod Camp, Tulane University
Cloth $45.00 / Paper $19.95

Democracy Without Equity
Failures of Reform in Brazil
Kurt Weyland
Investigates the crucial issue for many Latin American countries: the possiblity for redistributing wealth and power through the democractic process.
Cloth $49.95 / Paper $19.95

University of Pittsburgh Press

CUP Services, Box 6525, Ithaca, NY 14851
Credit card orders 607-277-2211

R. Michael Alvarez
INFORMATION AND ELECTIONS

Using survey data from five presidential elections, Alvarez studies how voters decide between candidates. A volume in the Michigan Studies in Political Analysis series.

cloth $39.50

John Brehm and Scott Gates
WORKING, SHIRKING, AND SABOTAGE
Bureaucratic Response to a Democratic Republic

A study of how federal, state, and local bureaucrats allocate their efforts by working or shirking, or even by sabotaging policy. A volume in the Michigan Studies in Political Analysis series.

cloth $39.50

Christine A. DeGregorio
NETWORKS OF CHAMPIONS
Leadership, Access, and Advocacy in the U.S. House of Representatives

DeGregorio investigates the role of representatives and advocacy groups in passing legislation. Based on extensive interviews with members of Congress, key staff members, and the leaders of advocacy groups, her book provides critical new insights into the legislative process.

cloth $37.50

Kaare Strøm and Lars Svåsand, Editor
CHALLENGES TO POLITICAL PARTIES
The Case of Norway

The framework provided by this unique collection sheds light on the long-term prospects for political parties worldwide.

cloth $49.50

Sunita Parikh
THE POLITICS OF PREFERENCE
Democratic Institutions and Affirmative Action in the United States and India

A study of the history of affirmative action programs in two ethnically heterogeneous democracies, India and the United States.

cloth $37.50 October

Robert G. Moeller, Editor
WEST GERMANY UNDER CONSTRUCTION
Politics, Society, and Culture in the Adenauer Era

Focusing on the intersections between the politics of daily life and the politics of the nation-state, the contributors offer a critical reexamination of the early history of the Federal Republic. A volume in the series Social History, Popular culture, and Politics in Germany.

cloth $54.50/paper $24.95

Stephen Crowley
HOT COAL, COLD STEEL
Russian and Ukrainian Workers from the End of the Soviet Union to the Post-Communist Transformation

Using extensive on-site research—including interviews with miners and steelworkers, labor leaders and plant managers—Crowley develops a detailed picture of the conditions under which workers organize. His findings have application beyond the conditions of post-communist Russia and Ukraine to other societies undergoing fundamental change.

cloth $39.50

Forrest Maltzman
COMPETING PRINCIPALS
Committees, Parties, and the Organization of Congress

Maltzman offers a new model to explain the role of congressional committees in the legislative process.

cloth $34.50

David L. Bartlett
THE POLITICAL ECONOMY OF DUAL TRANSFORMATIONS
Market Reform and Democratization in Hungary

Focusing on Hungary, Bartlett explores the impact of institutional change on reform policy and the ability of Eastern Europe's new democracies to manage simultaneous political and economic change.

cloth $39.50

Paul ‘t Hart, Eric K. Stern, and Bengt Sundelius, Editors
BEYOND GROUPTHINK
Political Group Dynamics and Foreign Policy-making

Building on the pioneering work of Irving Janis, *Beyond Groupthink* draws on alternate views of small-group dynamics to link the group process to the broader organizational and political context of the policy process and stress the need to develop a multi-level understanding of the collegial policymaking process.

cloth $49.50/paper $19.95

Now in paper

James A. R. Miles
THE LEGACY OF TIANANMEN
China in Disarray

"James Miles has written a fascinating, sobering book that contrasts with much of the conventional wisdom on China. . . . [His] argument needs to be considered by all those who think about the future of the world's largest country and its potential impact on the rest of us." —Kenneth Lieberthal

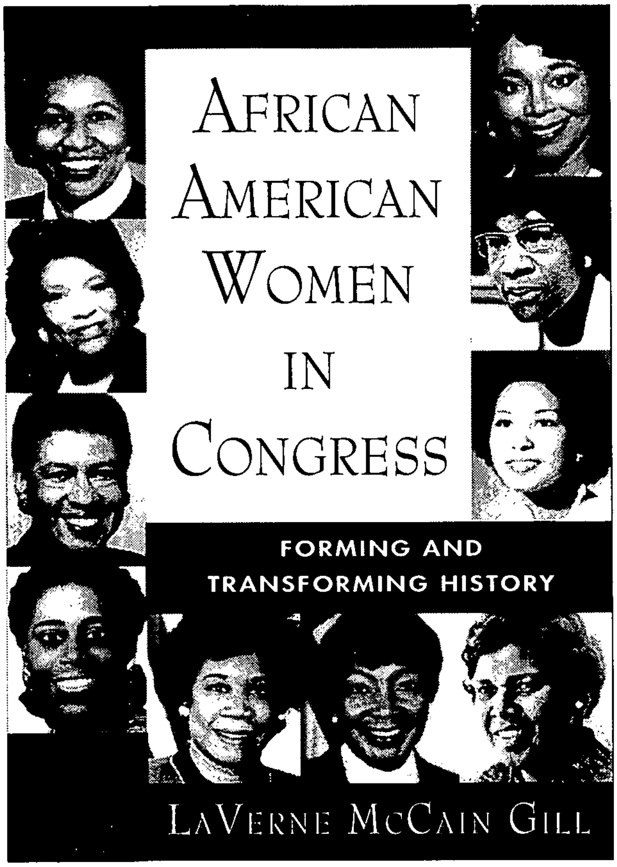
$17.95

Credit card buyers may fax orders to (800) 876-1922.

The University of Michigan Press
Dept. FJ, Ann Arbor, Michigan 48106-1104

MICHIGAN

"It is time for us to talk about race.
It is time for us to talk about racism.
It is time for us to use our power."
—Congresswoman Maxine Waters


AFRICAN AMERICAN WOMEN IN CONGRESS
FORMING AND TRANSFORMING HISTORY
LAVERNE MCCAIN GILL

"A fascinating look at the lives and contributions of African American women in Congress. It should prove to be an indispensable reference guide."—Carol M. Swain, Princeton University

The first book to profile African American women political leaders, *African American Women in Congress* preserves the history of the struggles and accomplishments of fifteen courageous women who have served in Congress between 1969 and the present.

Detailed are the lives and career histories of Shirley Chisholm, Barbara Jordan, Yvonne Burke, Cardiss Collins, Katie Hall, Eleanor Holmes Norton, Maxine Waters, Barbara-Rose Collins, Carol Moseley-Braun, Corinne Brown, Carrie Meek, Cynthia McKinney, Eva Clayton, Eddie Bernice Johnson, and Sheila Jackson-Lee.

256 pp. Paper, $19.95; Cloth, $48.00

RUTGERS UNIVERSITY PRESS
Available in bookstores or call (800) 446-9323

Rebecca Howard Davis

WOMEN AND POWER IN PARLIAMENTARY DEMOCRACIES

Cabinet Appointments in Western Europe, 1968–1992

Why do women, who make up more than half of the world's population, occupy so few positions at the highest levels of political power? Why are women making inroads in government in some countries while not in others? And what difference does women's presence—or absence—make in terms of policy outcomes?

Davis addresses these questions by examining women's access to power through appointive channels in Western European parliamentary and parliamentary-type systems. Tracing women's participation from 1968 to 1992 in fifteen countries, she accounts for the variation from high levels of women's representation in Norway and Sweden to low levels in Italy and Britain.
$32 cloth

Also available in the Women and Politics series
Women, Elections, and Representation
Second edition, revised
R. Darcy, Susan Welch, and Janet Clark
$15 paper/$35 cloth

University of Nebraska Press
publishers of Bison Books
Lincoln NE • 800-755-1105
www.unl.edu/UP/home.htm

PENN STATE PRESS

(Dis)Entitling the Poor

The Warren Court, Welfare Rights, and the American Political Tradition

ELIZABETH BUSSIERE

"*(Dis)Entitling the Poor* seamlessly weaves together a sophisticated understanding of American history, a subtle exposition of complex Supreme Court doctrines, and a hard-headed treatment of the realities of American politics. Bussiere's insistence that it really did make a difference that the recipients of and activists in welfare policy were poor, female, young, and disproportionately African American—political pariahs, in other words—is not a bit sentimental, but eloquently refutes claims that all citizens are alike in the eyes of the law."—Jennifer Hochschild, Princeton University

192 pages $26.50

The Politics of Warfare

The Great Powers in the Twentieth Century

STEPHEN J. CIMBALA

"This is one of the best analyses of the relationship between war and politics in the twentieth century. Applying Clausewitzian concepts to major issues, Cimbala provides a masterful study covering a wide span from World War I through the current period. This is not a military history, but an analytical study of critical issues of war and politics. It is unsurpassed in its analysis and insights and should be required reading for those concerned with the nature of war, conflict, and their political ramifications, and how all this may affect the United States." —Sam C. Sarkesian, Loyola University Chicago

254 pages $16.95 paper

Democracy Within Reason

Technocratic Revolution in Mexico

MIGUEL ÁNGEL CENTENO

Second Edition with a New Postscript

"An important study . . . that seeks to explain the people, organization, and ideology that lay behind . . . the Salinas period. Exceptionally strong in its explanation of the political identity of the Salinistas. . . ."—*American Political Science Review*

304 pages 10 illustrations $16.95 paper

Health Care and Reform in Industrialized Countries

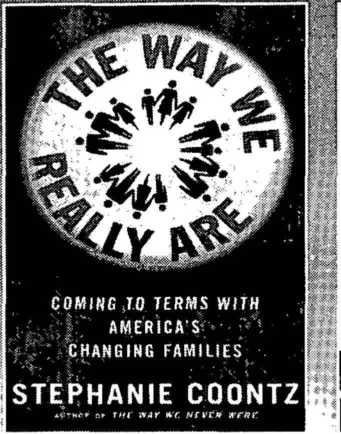
EDITED BY MARSHALL W. RAFFEL

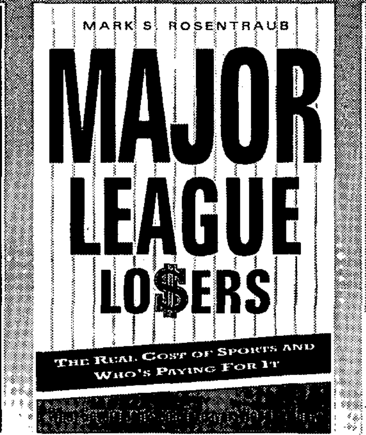
This timely volume examines the health care systems of Canada, Denmark, France, Germany, Japan, the Netherlands, New Zealand, Sweden, the United Kingdom, and the United States. All democratic and affluent, with well-educated populations and high health standards, these countries have interacted with each other extensively over the years in commercial, cultural, and scientific affairs. Contributors are Peggy Leatt, A. Paul Williams, Allan Krasnik, Signild Vallgårda, Wolfgang Greiner, J.-Matthias v.d.Schulenburg, Marie-Pascal Pomey, Jean-Pierre Poullier, Toshitaka Nakahara, J. A. M. (Hans) Maarse, Claudia Scott, Stefan Håkansson, Sara Nordling, Peter R. Hatcher, Marshall W. Raffel, and Norma K. Raffel.

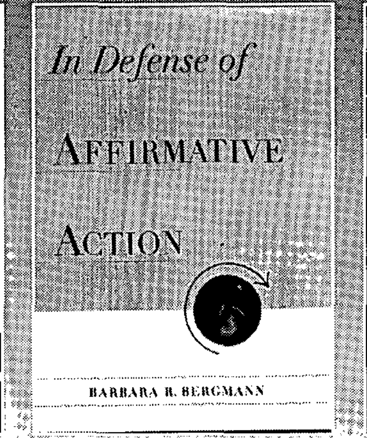
324 pages 19 illustrations $19.95 paper

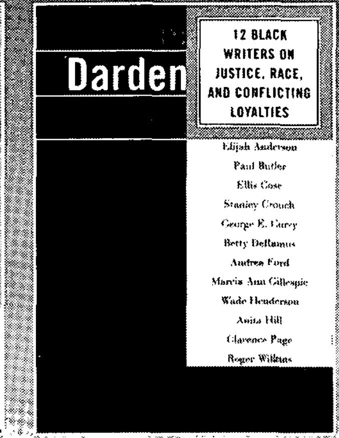
PENN STATE PRESS
820 North University Drive • University Park, PA 16802-1003
Orders: **1-800-326-9180**

Political Science is Basic


THE WAY WE REALLY ARE
COMING TO TERMS WITH AMERICA'S CHANGING FAMILIES
STEPHANIE COONTZ
AUTHOR OF THE WAY WE NEVER WERE


MARK S. ROSENTRAUB
MAJOR LEAGUE LO$ERS
THE REAL COST OF SPORTS AND WHO'S PAYING FOR IT


In Defense of
AFFIRMATIVE
ACTION
BARBARA R. BERGMANN


Darden
12 BLACK WRITERS ON JUSTICE, RACE, AND CONFLICTING LOYALTIES
Elijah Anderson
Paul Butler
Ellis Cose
Stanley Crouch
George E. Curry
Betty DeRamus
Andrea Ford
Marvis Ann Gillespie
Wade Henderson
Anita Hill
Clarence Page
Roger Wilkins

The Way We Really Are

Coming to Terms With America's Changing Families

Stephanie Coontz

Coontz's historically informed perspective on family life provides compelling evidence that the structure of the modern family, though much changed from the traditional model, is intact and is actually working better than ever.

"An enormously knowledgeable and readable affirmation of the kinds of families people really live in, and a critique of the political and economic structure that cuts them off at the knees."

—Karen Brodkin, UCLA

BasicBooks: $22.00 hardcover

ALSO BY STEPHANIE COONTZ

The Way We Never Were

American Families and the Nostalgia Trap

BasicBooks: $16.00 paperback

Black Dog of Fate

The Legacy of Genocide in an American Family

Peter Balakian

"A passionate and endearingly personal book on the genocide committed against the Armenian people by the Turks during the First World War. Balakian, a gifted poet and descendent of Armenian refugees, knows exactly how to bring the pain of the past into the landscape of the present. An extraordinary book."

—Alfred Kazin

BasicBooks: $24.00 hardcover

Major League Losers

The Real Cost of Sports and Who's Paying for It—What Governments and Taxpayers Need to Know

Mark S. Rosentraub

"The most definitive study to date of the corporate welfare system that major league sports has become....This book is important, timely, and well written."

—Roger G. Noll,
Stanford University

BasicBooks: $27.50 hardcover

NEW IN PAPERBACK

Migrations & Cultures

A World View

Thomas Sowell

Here is a study of immigration with an emphasis on the effects of immigration in economic terms—both for the immigrants and for the host countries—by the author of such books as *The Vision of the Anointed* and *Ethnic America.*

"Well reasoned and impressively researched...a formidable achievement."

—*New York Times Book Review*

BasicBooks paperback $16.00

NEW IN PAPERBACK

Ending Affirmative Action

The Case for Colorblind Justice

Terry Eastland

"The best argument yet for the principle of colorblind law...shows us that colorblindness is not code for indifference to minorities, but is rather the great test of American democracy itself."

—Shelby Steele, author of *The Content of Our Character*

BasicBooks: $13.00 paperback

NEW IN PAPERBACK

In Defense of Affirmative Action

Barbara R. Bergmann

A distinguished economist cuts through the incendiary rhetoric surrounding affirmative action to show that these programs are necessary and get results.

"Hereafter anyone entering this debate will have to reckon with her brief."

—*New York Times Book Review*

BasicBooks/A New Republic Book: $13.00 paperback

Out of America

A Black Man Confronts Africa

Keith B. Richburg

In this powerful book, Keith Richburg takes the reader on an extraordinary journey that sweeps from Somalia to Rwanda to Zaire and finally to South Africa. He shows how he came to terms with the divide within himself: between his African racial heritage and his American cultural identity.

"Whether you accept or reject Keith Richburg's deeply felt conclusions, his book—and his unusual journey—will certainly make you think."

—Ellis Cose, author of *The Rage of the Privileged Class*

BasicBooks: $24.00 hardcover

From HarperCollins

The Darden Dilemma

Black Writers on Justice and Race Relations in America

Edited by Ellis Cose

With 12 essays from Stanley Crouch, Clarence Page, Anita Hill, George Curry, Randall Kennedy, and other esteemed African-American writers, thinkers, and legal professionals, *The Darden Dilemma* is about much more than the significance of the O.J. Simpson trial; it is abou functioning in a world in which black and white perceptions seem increasingly at odds.

HarperPerennial: $13.00 paperback

Bayard Rustin

Troubles I've Seen

Jervis Anderson

"In Jervis Anderson's biography, Rustin lives again in all his protean complexity and eccentricity. This is a fine, sensitive book."

—David Levering Lewis, author of *King: A Biography*

HarperCollins: $30.00 hardcover

NEW IN PAPERBACK!

Showing My Color

Impolite Essays on Race in America

Clarence Page

"A non-threatening and accessible guide to the political consciousness of Black America at a moment in history when few have been able to understand or reach across the racial divide."

—Michael Lerner, editor, *Tikkun*

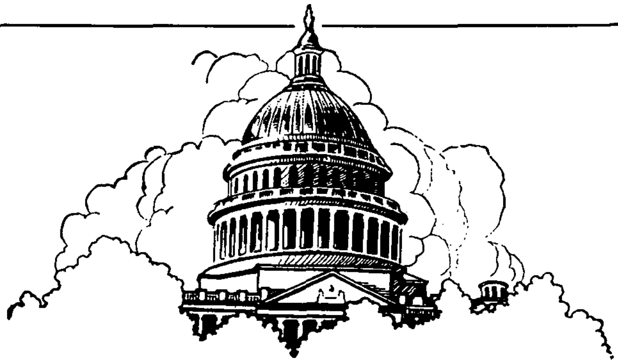
HarperPerennial: $13.00 paperback

New from BasicBooks & HarperCollins

Available in bookstores or call to order: 1 800 331-3761. Visit our academic website: http://www.hcacademic.com

ELECTRONIC REPRODUCTION PROHIBITED

New Titles From Northeastern



Apocalypse in Oklahoma
Waco and Ruby Ridge Revenged

Mark S. Hamm

In this detailed examination of the deadly Oklahoma City bombing, Mark S. Hamm looks at the ultra-right-wing organizations and ideologies behind the attack on the Murrah Building and argues that the FBI's handling of the Waco and Ruby Ridge sieges provided one of the motives by fueling the radical right's suspicion and hatred of the federal government.

$26.95 cloth

Hate Speech on Campus
Cases, Case Studies, and Commentary

Edited by Milton Heumann and Thomas W. Church

This volume offers a cogent and objective analysis of the increasingly common use on college campuses of speech codes prohibiting religious, ethnic, racist, and sexist epithets. Presenting judicial opinions, actual case studies, and expert commentary, the collection explores in depth the legal, political, and social complexities of such bans.

$50.00 cloth • $20.00 paper

Arming Iraq
How the U.S. and Britain Secretly Built Saddam's War Machine

Mark Phythian

Foreword by Nikos Passas

This penetrating analysis of the dramatically failed U.S. and British policy of covertly arming Iraq during the 1980s untangles the complex web of major players involved and offers a disquieting look at government duplicity.

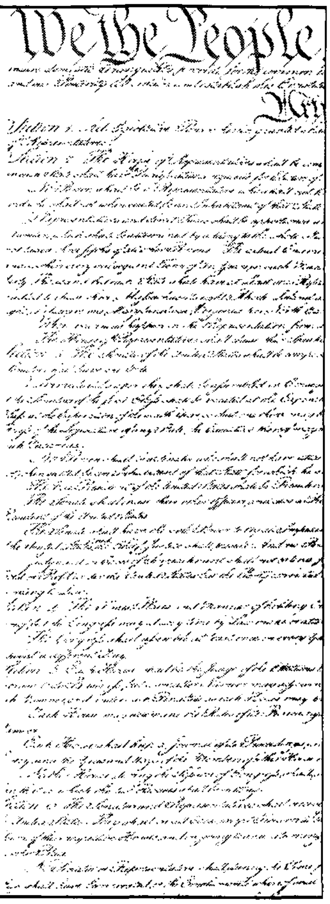
"A detailed case study of the so-called 'boomerang effect,' where weapons and technology that we sell come back to haunt us, and even to kill U.S. troops."

—Paul Simon, former United States Senator, Illinois

The Northeastern Series in Transnational Crime

$30.00 cloth

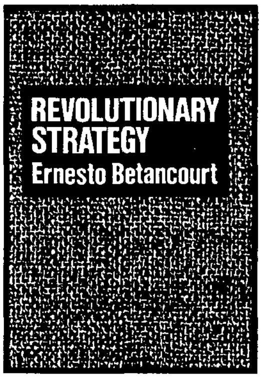
NORTHEASTERN
University Press
360 Huntington Avenue, 416CP
Boston, Massachusetts 02115



We the People

---

transaction



REVOLUTIONARY STRATEGY
Ernesto Betancourt

Revolutionary Strategy

Ernesto Betancourt

The cold war has ended peacefully, but other conflicts—in the Middle East, in Africa, in Latin America—continue to threaten international security. Even in Eastern Europe, the transition away from communism offers the potential for revolutionary situations. In these unstable times, an understanding of revolutions and their causes is a necessity when those involved—journalists, businessmen, military and political authorities, government advisors—must act quickly, often without complete information and limited decision-making time. *Revolutionary Strategy* is written for these people. Drawing on thirty years of experience with revolutionary situations, Betancourt's handbook offers a comprehensive examination of methodology, establishes a framework for the analysis of revolutionary dynamics, and applies this framework to two representative case studies: Castro's successful revolution against Batista in Cuba and the Tupamaros' unsuccessful effort in Uruguay.

ISBN: 1-88738-411-0 (cloth) 203 pp. $32.95/£20.95

Order from your bookstore or direct from the publisher. Major credit cards accepted. Call (908) 445-2280.

transaction

Transaction Publishers
Department RS97A1
Rutgers—The State University of New Jersey
New Brunswick, NJ 08903

Transaction Publishers (UK) Ltd.
Book Representation and Distribution, Ltd.
244A London Road,Hadleigh, Essex SS7 2DE
United Kingdom

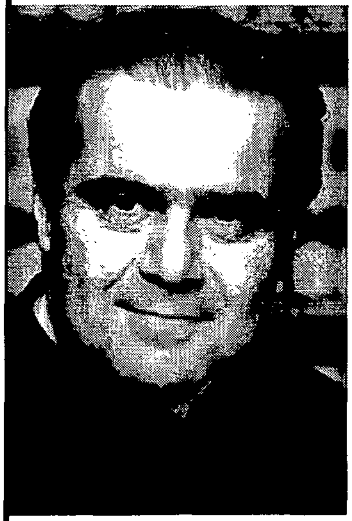
POLITICS IN PRACTICE

A Matter of Interpretation

Federal Courts and the Law

Antonin Scalia

Edited by Amy Gutmann



In this incisive new book, Supreme Court Justice Antonin Scalia shares his opinions about the law in general and the role of judges in particular. In his unique style, both erudite and witty, he exhorts judges to resist the temptation to rely on the intent and history of legislation in making decisions. Instead, he argues, they should give up trying to understand what lawmakers meant and focus on what they actually wrote into law.

A Matter of Interpretation includes responses from four writers with differing points of view: Gordon Wood, Laurence Tribe, Mary Ann Glendon, and Ronald Dworkin. In the spirit of debate, Justice Scalia responds to these critics.

This is an unusual and compelling book. It offers the rare opportunity of witnessing a sitting Supreme Court Justice in dialogue with some of the best legal minds of our time. It is a book that will be widely discussed.

The Princeton University Center for Human Values Series

Cloth: $19.95 ISBN 0-691-02630-0

Classics in Game Theory

Edited by Harold W. Kuhn

Classics in Game Theory assembles in one sourcebook the basic contributions to the field that followed on the publication of *Theory of Games and Economic Behavior* by John von Neumann and Oskar Morgenstern (Princeton, 1944). Game theory has become the dominant model in economic theory and has made significant contributions to political science as well.

Harold Kuhn, himself a major contributor to game theory, has chosen eighteen essays that constitute the core of game theory as it exists today. Drawn from a variety of sources, they will be an invaluable tool for all researchers in game theory.

Frontiers of Economic Research

Paper: $22.95 ISBN 0-691-01192-3 Cloth: $49.50 ISBN 0-691-01193-1

Open-Economy Politics

The Political Economy of the World Coffee Trade

Robert H. Bates

Coffee is traded in one of the few international markets ever subject to effective political regulation. In *Open-Economy Politics*, Robert Bates explores the origins, the operations, and the collapse of the International Coffee Organization, an international "government of coffee" that was formed in the 1960s. In so doing, he addresses key issues in international political economy and comparative politics, and analyzes the creation of political institutions and their impact on markets.

Drawing upon field work in East Africa, Colombia, and Brazil, Bates explores the domestic sources of international politics within a unique theoretical framework that blends game theoretic and more established approaches to the study of politics.

Cloth: $35.00 ISBN 0-691-02655-6

© Archive Photos

The Road to Nowhere

The Genesis of President Clinton's Plan for Health Security

Jacob S. Hacker

During the 1992 presidential campaign, health care reform became a hot issue, paving the way for one of the most important yet ill-fated social policy initiatives in American history: Bill Clinton's 1993 proposal for comprehensive coverage under "managed competition." Here Jacob S. Hacker not only investigates for the first time how managed competition became the president's reform framework, but also illuminates how issues and policies emerge.

Drawing on records of the President's task force, interviews with a wide range of key policy players, and many other sources, Hacker explores key questions: Why did health reform become a national issue in the 1990s? Why did Clinton choose managed competition over more familiar options during the presidential campaign? What effect did this have on the fate of his proposal?

Princeton Studies in American Politics

Cloth: $39.95 ISBN 0-691-04423-6

PRINCETON UNIVERSITY PRESS

AVAILABLE AT FINE BOOKSTORES OR DIRECTLY FROM THE PUBLISHER: 800-777-4726 • VISIT OUR WEBSITE: PUP.PRINCETON.EDU

Kansas

Justice Stephen Field

Shaping Liberty from the Gold Rush to the Gilded Age

Paul Kens

"This is history of American political thought, of law and economic development, and of law and politics, as well as a study of judicial activism. It is an absolutely essential revision of what we know about Field and laissez-faire." • **Martin Shapiro,** author of *Who Guards the Guardians.* "A masterful job." • **Melvin I. Urofsky,** author of *A March of Liberty.* "A major contribution to our understanding of the jurisprudence of the late nineteenth century." • **James W. Ely, Jr.,** author of *The Chief Justiceship of Melville W. Fuller, 1888–1910.*

392 pages, illustrated. $39.95

Available at bookstores or
from the press
Phone 913-864-4155
Fax 913-864-4586
Visa and MasterCard accepted

Enforcing Civil Rights

Race Discrimination and the Department of Justice

Brian K. Landsberg

"Combines an insider's firsthand knowledge with an academic's impartiality to give readers a close-up view of the CRD's development." • **Julian Bond.** "Landsberg's reflections and suggested reforms deserve serious attention from all who are concerned with civil rights, law, and politics." • **David M. O'Brien,** author of *Storm Center.*

296 pages. $35.00

Constitutional Conflicts Between Congress and the President

Fourth Edition, Revised

Louis Fisher

"As close to being indispensable as anything published in this field." • ***Congressional Quarterly.*** "Ought to be required reading for the President, for every member of Congress, and for anyone who seeks to understand the subtle complexities of a constitutional arrangement that formally separates legislative from executive powers." • **Choice.** "An extremely worthwhile book, adaptable to a variety of courses." • **Jeffrey K. Tulis,** author of *The Rhetorical Presidency.*

358 pages. $35.00 cloth, $14.95 paper

Power Steering

Global Automakers and the Transformation of Rural Communities

Michele M. Hoyman

"The most comprehensive study to date of the longer-term political, economic, and governmental effects of plant siting decisions on communities. This is a critical reading not just for those interested in the political economy of industrial location, but also for students of theory and practice of public administration." • **Bryan Jones,** coauthor of *The Sustaining Hand.* "Hoyman's dynamic conception of the impact process adds something essential to our understanding of her subject and holds the potential to fundamentally influence and enrich the literature on economic development." • **Dennis R. Judd,** editor of *Leadership and Urban Regeneration.*

272 pages. $35.00 cloth, $17.95 paper

E PLURIBUS UNUM

University Press of Kansas

2501 West 15th Street
Lawrence KS 66049

Kansas

Announcing A New Series

Landmark Law Cases and American Society

Peter Charles Hoffer and N.E.H. Hull, Series Editors

Compact, readable, authoritative, and affordable, each series volume tells the story of a significant American law case (or cases); traces its origins, issues, personalities, and consequences; and shows how it enlarges our understanding of American law and society. The books are designed for the student and general reader.

Now available

The Reconstruction Justice of Salmon P. Chase

In Re Turner and *Texas v. White*

Harold M. Hyman

"This is constitutional history as it should be written, but seldom is. Combining an excellent sketch of Chase's life with the social, intellectual, and moral climate of the times, Hyman provides a brilliant analysis of two landmark decisions. He also presents a stimulating, original, and provocative treatment of the Chase Court that sheds new light on our understanding of the thirteenth and fourteenth amendments." • **John Niven,** editor of *The Salmon P. Chase Papers.*

192 pages. $25.00 cloth, $12.95 paper

Affirmative Action on Trial

Sex Discrimination in *Johnson v. Santa Clara*

Melvin I. Urofsky

"An ideal primer on affirmative action." • **Kermit L. Hall,** author of *The Magic Mirror.* "An unparalled blend of exacting legal scholarship with engaging narrative history. Students and teachers alike will cherish this book." • **William M. Wiecek,** author of *Liberty under Law.* "Urofsky never forgets that the story he is telling involves real people, who spring to life in these pages and provide a human face to the debate and to the judicial process." • **Philippa Strum,** author of *Louis Brandeis: Justice for the People.*

208 pages. $25.00 cloth, $12.95 paper

About the series

"A splendid addition to the arsenal of texts available to teachers, this series fills a major vacuum in the literature of American constitutional and legal history."

• **Kermit Hall**

Forthcoming:

The Court-Martial of Lieutenant Calley

The Loyalist Cases

The Salem Witchcraft Trials

Roe v. Wade

Tinker v. Des Moines

Lochner v. New York

The Slaughter-House Cases

Standard Oil v. United States

The "Great Negro Plot" of 1741

Marbury v. Madison

The Pullman Case

The Skokie Case

Available at bookstores or
from the press
Phone 913-864-4155
Fax 913-864-4586
Visa and MasterCard accepted

University Press of Kansas
2501 West 15th Street
Lawrence KS 66049

YALE BOOKS

The Constitution of Deliberative Democracy

Carlos Santiago Nino

"Breathtaking in its scope, this important and original book provides an account of constitutionalism, a sophisticated variation of the liberal theory of rights, a review of contending views of the foundations of democratic theory, practical institutional proposals for modern democratic states, and a consideration of the justification of judicial review in a democracy."
—Lawrence Sager $30.00

Radical Evil on Trial

Carlos Santiago Nino

In this provocative book, Nino offers a first-hand analysis of the Argentine experience of the 1980s, when a brutal military dictatorship gave way to a democratic government. Nino, who played a key role in the transition and in the shaping of human rights policies in Argentina, examines the impact of war crimes trials there and considers their potential to strengthen a new democratic government.

"Carlos Nino was an extraordinary combination of theoretical philosopher, practical statesman, and heroic patriot. His work engages and repays attention at all three levels."—Ronald Dworkin
$27.50

Secret Agencies

U.S. Intelligence in a Hostile World

Loch K. Johnson

"Johnson ably mixes an overview of the purpose and philosophy of clandestine operations with a chronicle of the activities and relationships of current U.S. agencies....highly readable."
—*Publishers Weekly* $30.00

The Strategy of Rhetoric

Campaigning for the American Constitution

William H. Riker

This book, the last work of an eminent political scientist, is an innovative study of political persuasion during the 1787-88 campaign to ratify the U.S. Constitution. Employing historical and rational choice analysis to examine the rhetoric and strategic maneuvers the victorious Federalists used, Riker develops new tools and approaches to advance the science of political campaigns.
$30.00

NEW IN PAPER

Recasting Conservatism

Oakeshott, Strauss, and the Response to Postmodernism

Robert Devigne

"An exploration of ways in which Oakeshott and Strauss have shaped recent developments in British and American conservative thought and, to a lesser degree, policy....There is much in this book that is useful and enlightening."—Steven B. Smith, *American Political Science Review*
$16.00

NEW IN PAPER

Taxation and Democracy

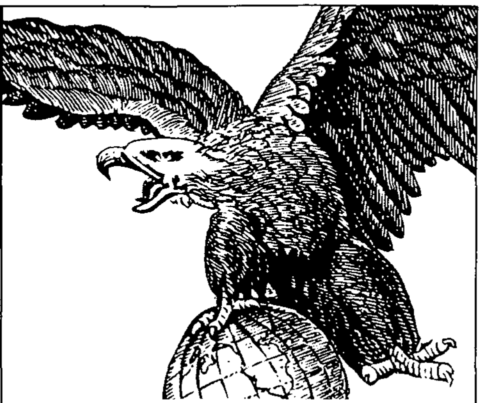
Swedish, British, and American Approaches to Financing the Modern State

Sven Steinmo

"A fast-paced, well-written book that makes interesting comparisons.... Steinmo's book would be an excellent choice for an undergraduate course in comparative public policy."
—Michael Wallerstein, *American Political Science Review*

Winner of the 1994 American Political Science Association section award for the Best Book in Political Economy

Winner of the 1987-88 Gabriel Almond Award given by the American Political Science Association $17.00



NEW IN PAPER

American Hegemony

Political Morality in a One-Superpower World

Lea Brilmayer

"An original, creative, and convincing book....Her thesis is...philosophically and politically attractive. If adopted and implemented by democratic leaders in their conduct of foreign policy, the world would be a better place."—Fernando R. Tesón, *Michigan Journal of International Law*
$17.00

NEW IN PAPER

The Secret World of American Communism

Harvey Klehr, John Earl Haynes, and Fridrikh I. Firsov

"The story told by this book is as gripping as a spy thriller, as sobering as a douse of ice water."—Roger Kimball, *Wall Street Journal*

"This book contains the first new revelation about American Communism in a generation. It is superbly edited and admirably presented. No one interested in the history of American Communism can afford to miss it."—Theodore Draper

ANNALS OF COMMUNISM SERIES $16.00

NEW IN PAPER

Stalin's Letters to Molotov

1925-1936

Edited by Lars T. Lih, Oleg V. Naumov, and Oleg V. Khlevniuk

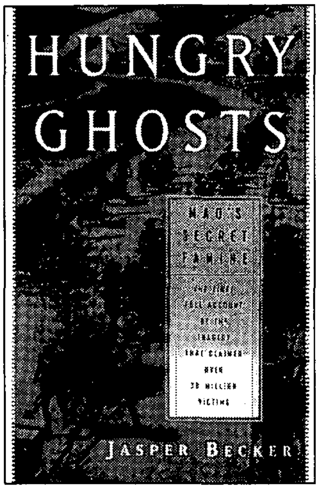
"These documents provide the best basic insight yet into the workings of Stalin's political mind."—Steven Merritt Miner, *New York Times Book Review*

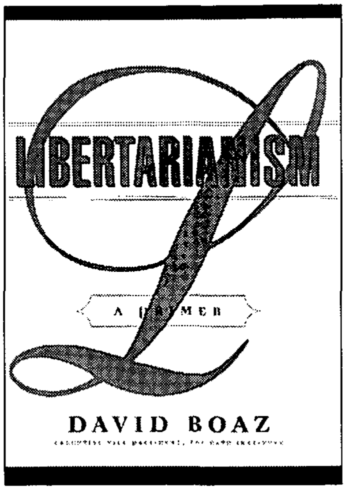
"These previously unpublished secret letters are arguably the most revealing documents from the Soviet archives yet available in the West."—Alan Bullock

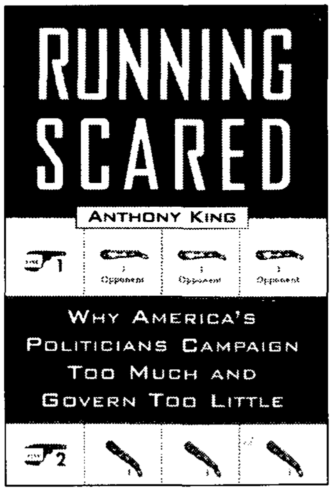
ANNALS OF COMMUNISM SERIES $16.00

YALE UNIVERSITY PRESS P.O. BOX 209040, NEW HAVEN, CT 06520 1-800-YUP-READ

NEW FROM THE FREE PRESS







HUNGRY GHOSTS

Mao's Secret Famine

Jasper Becker

❝A pathbreaking book that by laying bare the reality of the great famine that went almost unnoticed outside of China in the early '60s even as it devastated Mao's 'people's republic,' calls into question the acumen of foreign 'experts' in assessing China during that period. But it also serves as a much needed warning against too readily accepting the common wisdom about China's hectic development today.❞
—**Orville Schell,** University of California, Berkeley, author of *Mandate of Heaven: The Legacy of Tiananmen Square and The Next Generation of China's Leaders*

1997 ISBN: 0-684-83457-X $25.00

AFFAIRS OF STATE

The Rise and Rejection of the Presidential Couple Since World War II

Gil Troy

❝Troy's riveting history of the halting, controversial emergence of the modern presidential couple is both enormously entertaining and highly provocative. It explains the deep tension between the growing power of feminism in American culture and the continuing strength of traditional notions of women's roles—a tension to which America's First Ladies have fallen victim time and again.❞
—**Alan Brinkley,** author of *The End of Reform: New Deal Liberalism in Recession and War*

1997 ISBN: 0-684-82820-0 $27.50

LIBERTARIANISM

A Primer

David Boaz

❝In an age in which the end of big government is used by politicians as a pretext for bigger and worse government, it is refreshing to find a readable and informative account of the basic principles of libertarian thought written by someone steeped in all aspects of the tradition. Boaz unites history, philosophy, economics, and law—spiced with just the right anecdotes—to bring alive a vital tradition of American political thought that deserves to be honored today in deed as well as in word.❞
—Richard A. Epstein, University of Chicago, author of *Simple Rules for a Complex World*

1997 ISBN: 0-684-83198-8 $23.00

Also Available—

THE LIBERTARIAN READER

Classic and Contemporary Writings from Lao-Tse to Milton Friedman

David Boaz

1997 ISBN: 0-684-83200-3 $27.50

RUNNING SCARED

Why Politicians Spend More Time Campaigning Than Governing

Anthony King

❝This is an important book. Truly. Many are called so. This one is. It has been written for Americans by one of their best friends abroad. Voters ought to read it. So should their elected representatives, who are at once the victims and the causes of their own plight, here brought into focus by telling comparisons with London and Bonn. What's more, the book is fun to read. King is as good a writer as he is an insightful analyst.❞
—Richard E. Neustadt, author of *Presidential Power and the Modern Presidents*

1997 ISBN: 0-684-82730-1 $24.00

Now in paperback—

New Fourth Edition Now Available!

ADMINISTRATIVE BEHAVIOR

A Study of Decision-Making Processes in Administrative Organization

Herbert A. Simon

1997 ISBN: 0-684-83582-7 $17.95

FOUNDING FATHER

Rediscovering George Washington

Richard Brookhiser

1997 ISBN: 0-684-83142-2 $12.00

For credit card orders, call toll-free 1-800-323-7445. Or send check to:

fP **THE FREE PRESS**
An Imprint of Simon & Schuster • 1230 Avenue of the Americas, New York, NY 10020
Celebrating 50 Years of Free Thought

THE BEST IN POLITICS

Partisan Politics, Divided Government, and the Economy

Alberto Alesina and *Howard Rosenthal*

"Alesina and Rosenthal have made what will surely be an enduring contribution. . . . represents a genuine advance in the political business cycle literature. . . . This book is accessible and holds value for both economists and political scientists. It should prove very useful."

—*Journal of Politics*

Political Economy of Institutions and Decisions

43029-1	Hardback	$69.95
43620-6	Paperback	$18.95

Democracy and the Marketplace of Ideas

Communication and Government
in Sweden and the United States

Erik Asard and *W. Lance Bennett*

This book explores the institutional links between society and government that shape political communication. The authors show how these core elements of political systems affect the ways in which people communicate, and how effective that communication is at defining public problems and identifying workable solutions.

56336-4	Hardback	$59.95
56525-1	Paperback	$18.95

Democratic Experiments in Africa

Regime Transitions in Comparative Perspective

Michael Bratton and *Nicolas van de Walle*

Between 1989 and 1994, almost all of the countries in Sub-Saharan Africa underwent significant political reform, including in many cases the first competitive elections in a generation. This study constitutes the first comprehensive analysis of democratic transitions in Sub-Saharan Africa.

Cambridge Studies in Comparative Politics

55429-2	Hardback	about $59.95
55612-0	Paperback	about $19.95

The Retreat of the State

The Diffusion of Power in the World Economy

Susan Strange

This study argues that big businesses, drug barons, insurers, accountants and international bureaucrats all encroach on the so-called sovereignty of the state. It examines the implications of this rivalry, pointing to new directions for research.

Cambridge Studies in International Relations 49

56429-8	Hardback	$49.95
56440-9	Paperback	$16.95

Heroic Defeats

The Politics of Job Loss

Miriam A. Golden

This book is a comparative investigation of how unions and firms interact when economic circumstances require substantial job loss. Using simple game theory to generate testable propositions about when these situations will result in industrial conflict, Golden illustrates the theory in a range of situations between 1950 and 1985 in Japan, Italy, and Britain.

Cambridge Studies in Comparative Politics

48209-7	Hardback	$59.95
48432-4	Paperback	$18.95

America and Europe

A Partnership for a New Era

David C. Gompert and
F. Stephen Larrabee, Editors

"A book to help us think—energetically, carefully, creatively—about the way Europe and America interact. Read, be stimulated, and enjoy these penetrating essays."

—*George P. Shultz, former Secretary of State*

Contributors: *David C. Gompert, Ronald D. Asmus, Gregory F. Treverton, James A. Thomson, John Van Oudenaren, F. Stephen Larrabee, Zalmay Khalilzad, John Roper*

Rand Studies in Policy Analysis

59107-4	Hardback	$34.95

Historical Statistics of the United States on CD-ROM

Colonial Times to 1970–Bicentennial Edition

U.S. Bureau of the Census

". . . contains an indispensable collection of data on every conceivable subject in American history, from population shifts to economic production, motor vehicle deaths to sources of energy." —*Eric Foner, Columbia University*

Network License Available:
Please call 1-800-221-4512 x 312

58541-4	CD-ROM	$195.00

Why Switzerland?

Second Edition

Jonathan Steinberg

First published in 1976, this revised and extended edition offers a unique analysis of the structures that make Switzerland work. Linking an analysis of the microeconomy to politics, history, religion and language, it reveals how a "bottom up" society has survived in a world of "top down" states.

48170-8	Hardback	$59.95
48453-7	Paperback	$19.95

FROM CAMBRIDGE

Contemporary Capitalism

The Embeddedness of Institutions

J. Rogers Hollingsworth
and Robert Boyer, Editors

This book argues that there is no single best institutional arrangement for organizing modern societies. It maintains that the economic institutions of capitalism exhibit a large variety of objectives and tools that complement each other and can not work in isolation.

Contributors: *Catherine Alter, William Coleman, Benjamin Coriat, Lorraine Eden, Wyn Grant, Jerald Hage, Fen Osler Hampson, Paul Hirst, Charles Sabel, Philippe Schmitter, Wolfgang Streeck, Grahame Thompson, Jonathan Zeitlin*

Cambridge Studies in Comparative Politics

56165-5	Hardback	$59.95

Presidentialism and Democracy in Latin America

Scott Mainwaring and
Matthew Soberg Shugart, Editors

The contributors examine variations among different presidential systems and skeptically view claims that presidentialism has added significantly to the problems of democratic governance and stability.

Contributors: *Matthew Soberg Shugart, Scott Mainwaring, Ronald P. Archer, Brian F. Crisp, John M. Carey, Jeffrey Weldon, Mark P. Jones, Julio Faundez, Peter M. Siavelis, Eduardo Gamarra*

Cambridge Studies in Comparative Politics

57266-5	Hardback	about $64.95
57614-8	Paperback	about $22.95

Ukrainian Nationalism in the 1990s

A Minority Faith

Andrew Wilson

This work examines the impact of nationalism on the politics of Ukraine, arguably the most important of the non-Russian States to emerge from the collapse of the USSR in 1991. To date Ukraine has managed to steer clear of the kind of aggressive ethno-nationalism that has destabilized politics elsewhere in the region. However, an increasingly vocal and frustrated minority may become increasingly influential if the economic and/or security situation in the region rapidly deteriorates.

48285-2	Hardback	$59.95
57457-9	Paperback	$19.95

Intelligence Power in Peace and War

Michael Herman

"...the most thoughtful, detached and up-to-date primer on the subject ever produced by a recent member of the British intelligence community." —*The Independent*

Drawing mainly on British and American examples, this book provides an analytic framework for understanding the "intelligence community" and assessing its value.

Copublished with the Royal Institute for International Affairs

56231-7	Hardback	$59.95
56636-3	Paperback	$22.95

Goldbugs and Greenbacks

The Antimonopoly Tradition and the Politics of Finance in America

Gretchen Ritter

In the late nineteenth century, there was a popular and heated debate over what sort of financial system America should have. Ritter contends that there was a distinctive and neglected political tradition in the United States—the antimonopoly tradition—that was championed by nearly every major agricultural and labor group during the period from the Civil War until 1900.

56167-1	Hardback	$54.95

Media and Political Conflict

News from the Middle East

Gadi Wolfsfeld

The news media have become the central arena for political conflicts today. This book is the first to offer a dynamic and comprehensive approach to studying confrontations as small as protests and as large as wars, and applies this approach to analyze three conflicts: the Gulf War, the Palestinian Intifada, and the attempt by the Israeli right-wing to derail the Israeli-Palestinian peace accord.

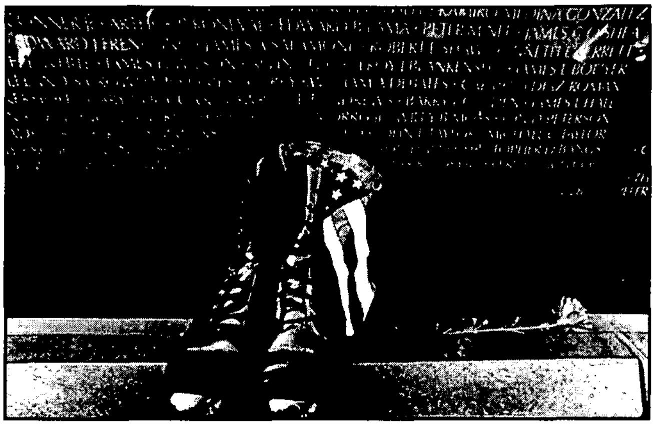
58045-5	Hardback	$59.95
58967-3	Paperback	$18.95

Available in bookstores or from

CAMBRIDGE
UNIVERSITY PRESS

40 West 20th Street, N.Y., NY 10011-4211
Call toll-free 800-872-7423. MasterCard/VISA accepted.
Prices subject to change. Web site: http://www.cup.org

POLITICAL SCIENCE **CALIFORNIA**


From *Tangled Memories*

Farewell to the Factory

Auto Workers in the Late Twentieth Century

Ruth Milkman

"A profound exploration into the decline of factory labor in the U.S. . . . Hers is one of those rare books that brilliantly illuminates current transformations in the organization of work and work lives." —Fred Block, University of California, Davis

"A valuable contribution to the national conversation on restoring the American Dream for working families."
—John J. Sweeney, President, AFL-CIO

$45.00 cloth, $14.95 paper

Tragedy and Enlightenment

Athenian Political Thought and the Dilemmas of Modernity

Christopher Rocco

"Reading Classical Greek texts in the light of Critical Theory and vice versa, Rocco's *Tragedy and Enlightenment* produces a dialectic of mutual illumination, cross-fertilizing two seemingly distinct traditions that still have much to offer the postmodern world." —Martin Jay, author of *The Dialectical Imagination*

Classics and Contemporary Thought, $40.00 cloth

Tangled Memories

The Vietnam War, the AIDS Epidemic, and the Politics of Remembering

Marita Sturken

"A startlingly original and integrated work that considers the ways in which American culture narrates, remembers, and thereby reenacts traumatic events in order to found and refound itself as a national culture."
—Lisa Lowe, author of *Immigrant Acts*

"A major contribution to our understanding of U.S. culture in the past two decades."
—John Carlos Rowe, coeditor of *The Vietnam War and American Culture*

$45.00 cloth, $16.95 paper, illustrated

From *Farewell to the Factory*

Images of Rule

Art and Politics in the English Renaissance, 1485-1649

David Howarth

Howarth examines the intersection of art and political power between the accession of the Tudors and the outbreak of civil war. He concentrates on the public uses and political exploitation of Renaissance art, rather than its quality or the creative process behind it.

$50.00 cloth, $19.95 paper, illustrated

NEW IN PAPERBACK

Afghanistan

The Soviet Invasion and the Afghan Response, 1979-1982

M. Hassan Kakar

"An account of what happened in Afghanistan—in the cities, villages, schools, refugee camps, and prisons where the author spent five years . . . [which] sheds great light on the violence that has continued long after the last Soviet soldier left."—*Foreign Affairs*

$16.95 paper

American Indian Treaties

The History of a Political Anomaly

Francis Paul Prucha

"A montage of treaty-making that is complicated, multiperspectival, ideological, historical and anomalous . . . [and] contributes to an understanding of the ongoing interactions between American Indians and whites in the United States."—*America*

$19.95 paper, illustrated

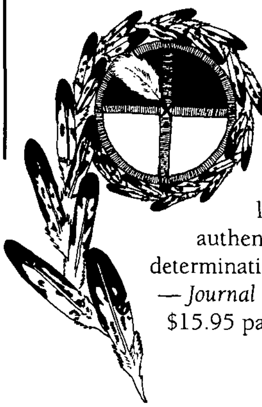
Braid of Feathers

American Indian Law and Contemporary Tribal Life

Frank Pommersheim

"[Pommersheim] sometimes poetically, sometimes polemically, but always pointedly argues that tribal courts are the fundamental institution of legitimate, authentic tribal self-determination."
—*Journal of Political Ecology*

$15.95 paper



At bookstores or order toll-free 1-800-822-6657

UNIVERSITY OF CALIFORNIA PRESS

NEW TITLES IN POLITICAL SCIENCE

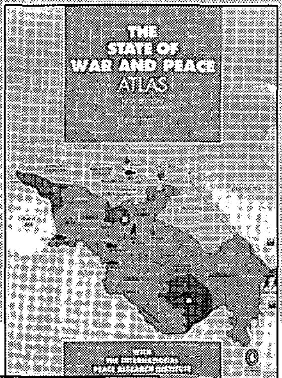
THE STATE OF WAR AND PEACE ATLAS

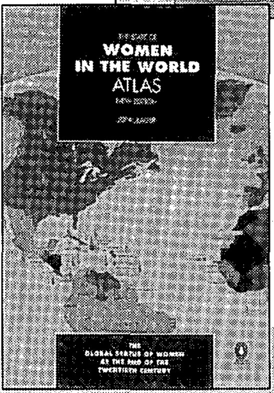
Updated Third Edition

Dan Smith

Zeros in on the dozens of local and regional armed conflicts going on in the world today, bringing together a global picture of the bloody costs of war. Also charts the deployment of armed forces, military spending, and arms trade, as well as the economics of peace, peacekeeping, and peace talks from Cambodia to Northern Ireland, from the Middle East to South Africa. 128 pp.

PENGUIN REFERENCE **0-14-051373-6** **$16.95**





THE STATE OF WOMEN IN THE WORLD ATLAS

New Edition

Joni Seager

Covers the issues facing women all over the globe, including property rights, domestic violence, and women in government. "An innovative atlas no thoughtful person, male or female, should be without."—*The Washington Post*. 128 pp.

PENGUIN REFERENCE **0-14-051374-4** **$16.95**

THE ROAD FROM SERFDOM

The Economic and Political Consequences of the End of Communism

Robert Skidelsky

"A brilliant account of the political economy of freedom."—*The Wall Street Journal*. Offers a provocative interpretation of the forces that precipitated Communism's collapse, and a trenchant vision for the future. 224 pp.

PENGUIN **0-14-024219-8** **$12.95**

COLOR BLINDED

How Liberals Got Race Wrong. How America Can Get It Right

Jim Sleeper

Part hard-hitting attack on the idea of race and its destructive tendencies, as well as clarion reminder of the principles that promise the most of American life for all peoples. Sleeper's essay asks liberals to scrutinize their politics and policies and to root out the demon of American politics: race. 208 pp.

VIKING **0-670-87391-8** **$21.95**

PENGUIN USA

ACADEMIC MARKETING DEPARTMENT • 375 HUDSON STREET • NEW YORK, NEW YORK 10014-3657

For a FREE Political Science catalog, please call (212) 366-2372.

http://www.penguin.com

NEW YORK UNIVERSITY PRESS

To Secure These Rights

The Declaration of Independence and Constitutional Interpretation

SCOTT DOUGLAS GERBER

WITH A FOREWORD BY HENRY J. ABRAHAM

"A delight to read. . . . Gerber states the principles of a liberal originalism far more clearly and accessibly than any other work on constitutional interpretation. . . . a rare and welcome addition."

—American Political Science Review

0-8147-3089-2 / $20.00 paper

New, Updated Edition

Bosnia

A Short History

NOEL MALCOLM

"An extraordinary book—the best available in English on the background of the Bosnian war."

—Warren Zimmermann, former U.S. ambassador to Yugoslavia, in the National Interest

0-8147-5561-5 / $18.95 paper

Modern Black Nationalism

From Marcus Garvey to Louis Farrakhan

EDITED BY WILLIAM L. VAN DEBURG

0-8147-8789-4 / $18.95 paper

Classical Black Nationalism

From the American Revolution to Marcus Garvey

WILSON JEREMIAH MOSES

0-8147-5533-X / $15.95 paper

War or Peace?

Nationalism, Democracy, and American Foreign Policy in Post-Communist Europe

STEVEN L. BURG

A Twentieth Century Fund Book

0-8147-1270-3 / $35.00 cloth

2nd Edition

Comparative Constitutional Engineering

An Inquiry into Structures, Incentives, and Outcomes

GIOVANNI SARTORI

Translated into five languages

0-8147-8063-6 / $18.50 paper

The Constitution of Interests

Beyond the Politics of Rights

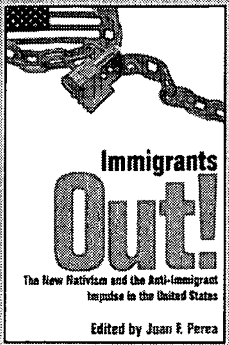
JOHN BRIGHAM

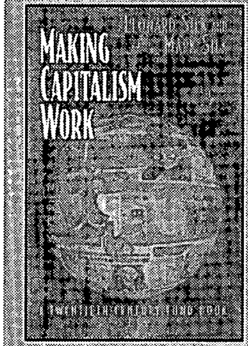
0-8147-1285-1 / $37.50 cloth

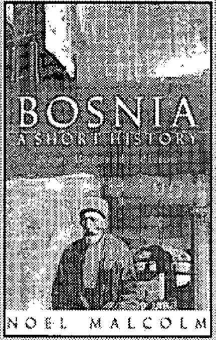
Riots and Pogroms

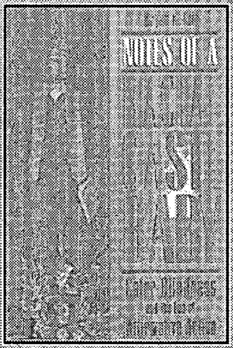
EDITED BY PAUL R. BRASS

0-8147-1274-6
$40.00 cloth


Immigrants
Out!
The New Nativism and the Anti-Immigrant Impulse in the United States
Edited by Juan F. Perea







Immigrants Out!

The New Nativism and the Anti-Immigrant Impulse in the United States

EDITED BY JUAN F. PEREA

Critical America Series

0-8147-6642-0 / $19.95 paper

Notes of a Racial Caste Baby

Color Blindness and the End of Affirmative Action

BRYAN K. FAIR

"Encapsulating court rulings and historical anecdotes with the details of his own struggle, Fair shows how racially sensitive policies can give disadvantaged minorities a leg up in a society where the best jobs go to the best educated students."

—Publishers Weekly

Critical America Series

0-8147-2651-8 / $24.95 cloth

Please Don't Wish Me a Merry Christmas

A Critical History of the Separation of Church and State

STEPHEN M. FELDMAN

Critical America Series

0-8147-2637-2 / $29.95 cloth

Taxing America

EDITED BY KAREN B. BROWN AND MARY LOUISE FELLOWS

Critical America Series

0-8147-2661-5 / $24.95 paper

Making Capitalism Work

LEONARD SILK AND MARK SILK

"An instructive survey of capitalism as it now exists in the world's great centers of economic power. . . . An invitation to a debate."

—The New York Times Book Review

A Twentieth Century Fund Book

0-8147-8064-4 / $24.95 cloth

Who Will Care for Us?

Aging and Long-Term Care in Multicultural America

RONALD J. ANGEL AND JACQUELINE L. ANGEL

0-8147-0629-0 / $29.95 cloth

A Glimpse of Hell

Reports on Torture Worldwide

EDITED BY DUNCAN FORREST FOR AMNESTY INTERNATIONAL

0-8147-2666-6 / $17.95 paper

Tenth Edition

The Judiciary

HENRY J. ABRAHAM

0-8147-0653-3
$18.50 paper

New York University Press
70 Washington Square South • New York, NY 10012
1-800-996-6987
http://www.nyu.edu/pages/nyupress

Oxford University Press

The Habits of Legality

Criminal Justice and the Rule of the Law

FRANCIS A. ALLEN

In this broad consideration of American criminal justice today, Allen suggests that the way to a more effective penal policy can be found in a closer adherence to the law rather than in the current tendency to bypass certain laws in the name of the "war on crime".

(Studies in Crime and Public Policy)

1996 168 pp. **$35.00**

Forced Justice

School Desegregation and the Law

DAVID J. ARMOR

"Likely to set off a fresh debate among lawyers, educators and social scientists."—*The New York Times Book Review*

"The most impressive compilation of empirical evidence showing a depth of contextual knowledge published to date."

—*American Political Science Review*

1995 (paper 1996) 288 pp. paper **$13.95** cloth **$38.00**

Police for the Future

DAVID H. BAYLEY

"A tour de force of research on police organization and an analysis of new data on police activities, workloads, and budgets.... One of the most rewarding aspects of the book is its comparative orientation, which includes police in the United States, Australia, Canada, Great Britain, and Japan."

—*Contemporary Sociology*

(Studies in Crime and Public Policy)

1994 (paper 1996) 208 pp. paper **$14.95**

Chinatown Gangs

Extortion, Enterprise, and Ethnicity

KO-LIN CHIN

Rich in data collected from gang members, gang victims, community leaders, and law enforcement authorities, this book offers a systematic study of New York City's Chinatown gangs.

(Studies in Crime and Public Policy)

1996 256 pp. **$35.00**

New in paperback!

The Politics of Child Abuse in America

LELA B. COSTIN, HOWARD JACOB KARGER, and DAVID STOESZ

"Informative, readable, widely researched, and balanced.... Thoughtfully place[s] child abuse in a broad historical context, showing how periodic 'discoveries' of the mistreatment of children reflect shifting issues and moods."

—*History of Education Quarterly*

1996 (paper 1997) 208 pp. paper **$15.95** cloth **$29.95**

Against Capital Punishment

The Anti-Death Penalty Movement in America, 1972-1994

HERBERT H. HAINES

"Compelling....This humane book is a must read for serious students of capital punishment."

—Robert Johnson, *American University*, and author of *Condemned to Die: Life Under Sentence of Death*

1996 272 pp. **$35.00**

European Socialists Respond to Fascism: Ideology, Activism and Contingency in the 1930s

GERD-RAINER HORN

Based on documents collected in six European countries, this book offers a transnational study of largely parallel developments in Austria, Belgium, France, Germany, and Spain in the years 1933-1936.

1996 224 pp. **$45.00**

Imperfect Justice

An East-West German Diary

INGA MARKOVITS

"A beautiful, intelligent and sensitive book."

—*Frankfurter Allgemeine*

This book, already a bestseller in Germany, is an account of the sudden death of Socialist law in East Germany and of the reactions, hopes and fears of some of its survivors.

1995 216 pp. paper **$21.00** cloth **$49.95**

Judicial Power and American Character

Censoring Ourselves in an Anxious Age

ROBERT F. NAGEL

"Chronicles the decline of American moral character by examining the cultural meaning of recent Supreme Court jurisprudence....[A] searching critique of contemporary culture."

—*Harvard Law Review*

"Thought-provoking and well-written."

—*American Political Science Review*

1994 (paper 1996) 208 pp. paper **$14.95** cloth **$35.00**

Free Markets and Social Justice

CASS R. SUNSTEIN

"This collection of essays, revised and shaped to persuasive unity, will be enormously helpful to all who wish to explore the uses and abuses of market reasoning in the political and legal sphere."

—Peter L. Strauss, *Columbia University*

1997 416 pp. **$35.00**

Race, Law, and Culture

Reflections on *Brown v. Board of Education*

Edited by AUSTIN SARAT

"Remarkable....[Offers] some of the most powerful and original reassessments of *Brown* and its legacies to appear in recent years. By placing the *Brown* case both in the perspective of its own time and ours, the contributors help us understand what a great divide separates us from the vision of racial harmony of the 1950s."

—Robert W. Gordon, *Yale University*

1997 256 pp. paper **$16.95** cloth **$45.00**

Imagining the Balkans

MARIA N. TODOROVA

Based on a rich selection of travelogues, *diplomatic accounts, academic surveys,* journalism, and belles-lettres in many languages, this work explores the contested *idea* of the Balkans, uncovering the ways in which an intellectual tradition was constructed, became mythologized, and is being transmitted as discourse.

1997 272 pp. paper **$19.95** cloth **$39.95**

Living High and Letting Die

Our Illusion of Innocence

PETER UNGER

"Unger's discussion of how much the well-off should sacrifice for the world's most needy stands as the state-of-the-art treatment of the subject."

—*Times Literary Supplement*

1996 200 pp. paper **$14.95** cloth **$39.95**

New in paperback!

Named an Outstanding Academic Book for 1995 by Choice

Incapacitation

Penal Confinement and the Restraint of Crime

FRANKLIN ZIMRING and GORDON HAWKINS

"[An] outstanding book...Its thoughtful reading and discussion should be considered mandatory for anyone interested in criminal justice policy in the United States and the role that penal incarceration will take in the future."

—*Journal of Sociology and Social Welfare*

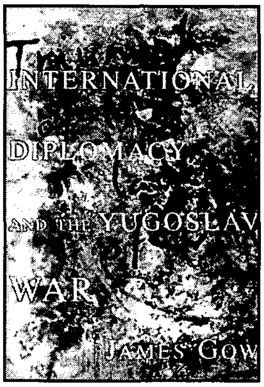
(Studies in Crime and Public Policy)

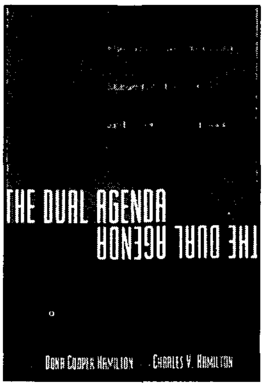
1995 (paper 1997) 208 pp. paper **$12.95** cloth **$29.95**

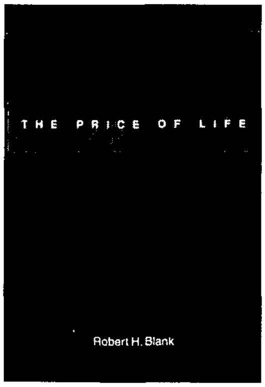
Prices are subject to change and apply only in the U.S. To order or for more information please call 1-800-451-7556. In Canada, call 1-800-387-8020.

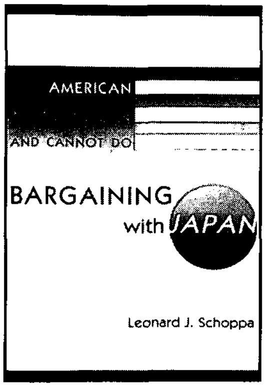
Visit our new website:
http://www.oup-usa.org

New from Columbia









TRIUMPH OF THE LACK OF WILL

International Diplomacy and the Yugoslav War

James Gow

Why did the major Western powers fail to resolve the War of Dissolution in Yugoslavia? Gow evaluates the range of attempts to find a workable peace, and identifies four factors that helped subvert the peace process: bad timing, bad judgment, poor cohesion, and, above all else, the absence of political will.

April 1997 ▪ 250 pages ▪ $29.50, cloth

THE DUAL AGENDA

Race and Social Welfare Policies
of Civil Rights Organizations

Dona Cooper Hamilton
and Charles V. Hamilton

"THE DUAL AGENDA documents that, contrary to popular misconception, the civil rights agenda is an American agenda. Organizations like the National Urban League and the NAACP have throughout their history labored to persuade this nation to live up to the edict in our Constitution that 'all men are created equal' and to honor the Preamble which calls upon government to 'promote the general welfare.'"—Hugh B. Price, President, National Urban League

Power, Conflict, and Democracy: American Politics Into the 21st Century ▪ Robert Y. Shapiro, Editor
May 1997 ▪ 384 pages ▪ $24.95, cloth

Now available in paperback, with a new chapter:

THE RWANDA CRISIS

History of a Genocide

Gérard Prunier

"[Prunier] presents his balanced and painstaking research with clarity and skill, and he shows how the ideological, political, and economic components of Rwanda's human time bomb slowly assembled."
—*Foreign Affairs*

389 pages ▪ $16.50, paper

THE POLITICAL ECONOMY OF REGIONALISM

Edward D. Mansfield and
Helen V. Milner, Editors

There are ongoing, intense debates about the current status and future direction of regionalism. The contributors of this book explore regionalism from a political economy perspective, investigating why regional arrangements are formed, the conditions under which these arrangements solidify, and why they take on different institutional forms.

New Directions in World Politics ▪ John G. Ruggie, Editor
May 1997 ▪ 288 pages ▪ $19.50, paper

BARGAINING WITH JAPAN

What American Pressure Can and Cannot Do

Leonard J. Schoppa

In a detailed critical examination of the outcome of recent trade talks, Schoppa offers strategies most likely to bring about concessions from Japan, and identifies the conditions under which these strategies are likely to work, with an insightful analysis of the links between Japan's domestic and foreign political agendas.

400 pages ▪ $17.50, paper

THE PRICE OF LIFE

The Future of American Health Care

Robert H. Blank

Blank goes outside current debates about who pays for medical care to illuminate the fundamental social, economic, and cultural dimensions of health care in American society. This powerful book will be an essential resource for policy-makers, scholars in political science, public health, and medical ethics, as well as anyone interested in health care reform.

248 pages ▪ $29.50, cloth

THE POLITICS OF JERUSALEM SINCE 1967

Michael Dumper

"A major contribution to serious scholarship on the politics of Jerusalem and the drastic changes since 1967 affecting the politics, economics, architecture, demography and geography of the city. This thoughtful painstakingly researched account of a controversial topic is objective and comprehensive . . . by far the best single work on the subject." —Naseer H. Aruri, University of Massachusetts

320 pages ▪ 26 maps ▪ $29.50, cloth

DAILY LIFE IN A CRUMBLING EMPIRE

The Absorption of Russia Into the World Economy

David H. Lempert

This work details the collapse of the Russian empire and describes the political, social, and cultural processes which have been underway this century in Russia. It is the first comprehensive ethnography of urban Russia and its political, economic, legal, cultural, and social system reforms.

East European Monographs
2 volumes ▪ 600 pages each ▪ $168.00, cloth

Columbia University Press
http://www.columbia.edu/cu/cup

GEORGETOWN

New Textbooks—

From Many One

Readings From American Political and Social Thought

Richard C. Sinopoli, Editor

"An indispensable text for the study of *e pluribus unum*."—Clarence E. Walker, Professor of History, University of California, Davis

"This reader will quickly prove itself a superb teaching instrument. It provides an unusually broad range of selections, without neglecting standard items. The thematic arrangements are clear and direct, and easily adaptable to a wide variety of course organizations. Most important, the editor's own essay contributions are fair, balanced, and insightful contributions to building contextual frameworks for all and each of the selections."—H. Mark Roelofs, professor of political science, New York University

From the classic texts of the American political tradition to diverse minority writings, this broad and balanced anthology offers a wide spectrum of ideas about identity, gender, immigration, race, and religion, and addresses how these issues relate to the concept of national unity.

In the series,
Text and Teaching/Politics, Policy, Administration
544 pp. February 1997 ISBN 0-87840-626-3, paper, $29.95

Expert Advice for Policy Choice

Analysis and Discourse

Duncan MacRae, Jr. and Dale Whittington

Economic reasoning has thus far dominated the field of public policy analysis. This new introduction to the field posits that policy analysis should have both a broader interdisciplinary base—including criteria from such fields as political science, sociology, law, and philosophy, as well as economics—and also a broader audience in order to foster democratic debate.

American Governance and Public Policy series
432 pp. June 1997
ISBN 0-87840-640-9, cloth, $65.00
ISBN 0-87840-641-7, paper, $25.00

Memos to the Governor

An Introduction to State Budgeting

Dall W. Forsythe

Written by a former state budget director, this book is a concise, behind-the-scenes guide that explains in clear, understandable prose the technical, economic, and political dynamics of budget making in the state capitols.

In the series,
Text and Teaching/Politics, Policy, Administration
112 pp. March 1997
ISBN 0-87840-636-0, cloth, $27.95
ISBN 0-87840-637-9, paper, $ 9.95

The Political Economy of Special-Purpose Government

Kathryn A. Foster

This book is the first comprehensive examination of the causes and consequences of special-purpose governments in the United States. It presents new evidence on the economic, political, and social implications of relying on these special districts while offering a conceptual framework for understanding them and important findings about their use and significance.

American Governance and Public Policy series 288 pp. May 1997 ISBN 0-87840-638-7, cloth, $55.00

Careers in International Affairs

Sixth Edition

School of Foreign Service,
Georgetown University

"A godsend . . . "—Donald Stokes, former dean Woodrow Wilson School, Princeton University

The leading guide to job hunting and career development in the field of international affairs.

320 pp. November 1996
ISBN 0-87840-630-1, paper, $17.95

How Does a Policy Mean?

Interpreting Policy and Organizational Actions

Dvora Yanow

"Offers an innovative and enlightening approach to understanding how public policies, which are always ambiguous, come to have particular meanings for diverse audiences. . . . The book is especially cogent in its discussion of interpretation, offering one of the most impressive analyses of that key process I know. . . . [Yanow] contributes insightfully to our understanding of political language and symbolism.—Murray Edelman, professor emeritus of political science, University of Wisconsin

288 pp. ISBN 0-87840-611-5, cloth, $55.00
New Paperback Edition:
ISBN 0-87840-612-3, paper, $23.95

GEORGETOWN UNIVERSITY PRESS

P.O. Box 4866 Hampden Station, Baltimore, MD 21211-4866
Phone toll-free 800-246-9606 or 410-516-6995 Fax 410-516-6998

New Titles from GPG GREENWOOD PUBLISHING GROUP INC.

To Sheathe the Sword
Civil-Military Relations in the Quest for Democracy
Edited by **John P. Lovell** and **David E. Albright**
Contributions in Political Science, No. 379

An important new volume, sponsored by the Indiana Center on Global Change and World Peace, which brings together academicians and practitioners in comparative politics and international relations to examine the important impact of civil-military relations on the process of democratization.

Greenwood Press. 1997. 248 pages. 0-313-30037-2. $59.95

Civil-Military Relations in Post-Communist States
Central and Eastern Europe in Transition
Edited by **Anton A. Bebler**

Experts from Europe and the United States examine how the foundering of the autocracies of Central and Eastern Europe brought about important changes in civil-military relations.

Praeger Publishers. 1997. 176 pages. 0-275-95350-5. $55.00

Industrial Control Over the Socialist Town
Benevolence or Exploitation?
By **Boleslaw Domanski**

An examination of the powerful state industrial firms in Central European towns under socialism and their impact on social inequalities.

Praeger Publishers. 1997. 272 pages. 0-275-95633-4. $65.00

The Future of Islam in the Middle East
Fundamentalism in Egypt, Algeria, and Saudi Arabia
By **Mahmud A. Faksh**

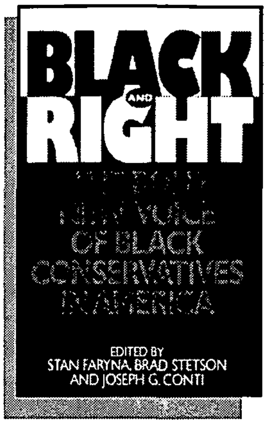
An important new study that assesses comparatively the future of Islamic fundamentalism in three key countries—Egypt, Algeria, and Saudi Arabia—and argues that the threat of an Islamic avalanche is overstated, unrealistic, and lacking in credibility.

Praeger Publishers. 1997. 152 pages. 0-275-95128-6. $49.95

The U.S. Media and the Middle East
Image and Perception
Edited by **Yahya R. Kamalipour**
Recipient of the 1996 Distinguished Book Award from the International and Intercultural Division of the Speech Communication Association
Foreword by George Gerbner

Experts portray the disturbing ramifications of media today and analyze the stereotypes and misconceptions that Americans have of Arabs and the United States impact on the Middle East.

Praeger Paperback. 1997. 264 pages. 0-275-95914-7. $22.95
A hardcover edition is available: 0-313-29279-5. $59.95.



Black and Right
The Bold New Voice of Black Conservatives in America
Edited by **Stan Faryna, Brad Stetson,** and **Joseph G. Conti**

While applauding the moral and legal victories of the civil rights movement, the conservative spokespeople in this collection reject the claims of inequities and what they consider to be the self-serving agendas of the present civil rights establishment. National leaders such as Justice Clarence Thomas and Representative Gary Franks and writers such as Shelby Steele and Glenn Loury appear as contributors and/or subjects in this volume. They emphasize grassroots aspects of black conservatism with a reliance on common sense and common humanity.

Praeger Trade. 1997. 192 pages. 0-275-95342-4. $19.95

Headline Diplomacy
How News Coverage Affects Foreign Policy
By **Philip Seib**

Headline Diplomacy explores the many ways in which news coverage shapes the design and implementation of foreign policy. By covering a wide range of issues and examples, this important text will stimulate thoughtful appraisal of the relationships between the news media and those who make policy.

Praeger Paperback. 1996. 208 pages. 0-275-95375-0. $19.95
A hardcover edition is available: 0-275-95374-2. $59.95.

Political Violence in Northern Ireland
Conflict and Conflict Resolution
Edited by **Alan O'Day**

Assesses the causes and nature of violence in Northern Ireland and considers the possibilities for peace.

Praeger Publishers. 1997. 264 pages. 0-275-95414-5. $59.95

Foreign Investment and Political Conflict in Developing Countries
By **John M. Rothgeb, Jr.**

A systematic investigation of the degree to which international economic linkages are related to domestic political conflict in developing countries.

Praeger Publishers. 1996. 160 pages. 0-275-94548-0. $55.00

To Have and To Hold
The Meaning of Ownership in the United States
By **Neala Schleuning**

An interdisciplinary look at ownership which focuses primarily on the transition in the modes and meaning of ownership since the emergence of the Industrial Revolution. .

Praeger Publishers. 1997. 256 pages. 0-275-95723-3. $59.95

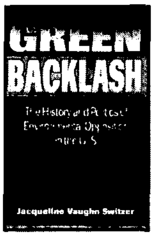
**PLACE YOUR CREDIT CARD ORDER TOLL-FREE,
24 HOURS-A-DAY: 1-800-225-5800**

GPG GREENWOOD PUBLISHING GROUP INC.
88 Post Road West • P.O. Box 5007 • Westport, CT 06881-5007• Telephone (203) 226-3571 • Office FAX (203) 222-1502

AD97

Visit us on the web at http://www.greenwood.com, or gopher to: gopher.greenwood.com

LYNNE RIENNER PUBLISHERS



Green Backlash
The History and Politics of the Environmental Opposition in the U.S.

Jacqueline Vaughn Switzer

"Nothing has been attempted that is as comprehensive and objective. Well written and researched." —Zachary A. Smith

hc $55 • pb $21

Sex as a Political Variable
Women as Candidates and Voters in U.S. Elections

Richard A. Seltzer, Jody Newman, and Melissa V. Leighton

"Likely to become a touchstone for all future work on women and politics." —William G. Mayer

hc $45 • pb $19.95

Politics and the Press
The News Media and Its Influences

edited by Pippa Norris

"Thoughtful and well-written." —Susan Herbst

July 1997 • hc $55 • pb $19.95

The Other Elites
Women, Politics, and Power in the Executive Branch

edited by MaryAnne Borrelli and Janet M. Martin

"A pathbreaking, long overdue book. It makes a significant contribution to both presidential and women's studies."
—Michael A. Genovese

hc $55

U.S. Public Policy
series editor, James F. Gosling

In the Cross Fire
A Political History of the Bureau of Alcohol, Tobacco and Firearms

William J. Vizzard

"Consistent and powerfully convincing.... A definitive history of the Bureau of Alcohol, Tobacco and Firearms."
—Chester A. Newland

hc $49.95

The Amtrak System
The History and Politics of a National Railroad

David C. Nice

November 1997 • hc $45

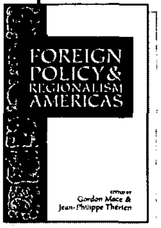
Global Europe
The European Union in World Affairs

Christopher Piening

"A well-conceived, carefully crafted, and well-written book. It is absolutely first-rate."
—Carolyn Rhodes

April 1997 • hc $49.95 • pb $19.95

Foreign Policy and Regionalism in the Americas



edited by Gordon Mace and Jean-Philippe Thérien

"Carefully crafted and insightful."
—Martha Cottam

hc $48 • pb $19.95

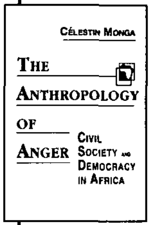
Economic Development, Social Order, and World Politics

Erich Weede

"Weede has produced a creative and useful framework.... generates provocative insights."
—Mershon International Studies Review

hc $47.50

The Anthropology of Anger
Civil Society and Democracy in Africa



Célestin Monga

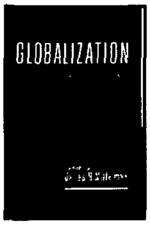
"An exciting, challenging, and highly original work from one of Africa's most important democratic thinkers."
—Larry Diamond

hc $45

New in Paperback!

Globalization
Critical Reflections

edited by James H. Mittelman



"A rich and worthy treatment.... one of the best studies of globalization that I have seen."
—Phillip McMichael

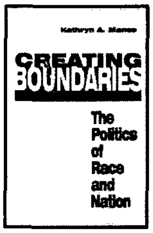
hc $49.95 • pb $19.95

International Politics and the New Asia Pacific

Derek McDougall

August 1997 • hc $55 • pb $19.95

Creating Boundaries
The Politics of Race and Nation



Kathryn A. Manzo

hc $43

Political Islam
Revolution, Radicalism, or Reform?

edited by John L. Esposito

"Substantial and important.... Lucidly written and cogent in analysis." —James Piscatori

May 1997 • hc $55 • pb $19.95

"Property" and the Making of the International System

Kurt Burch

Critical Perspectives on World Politics series

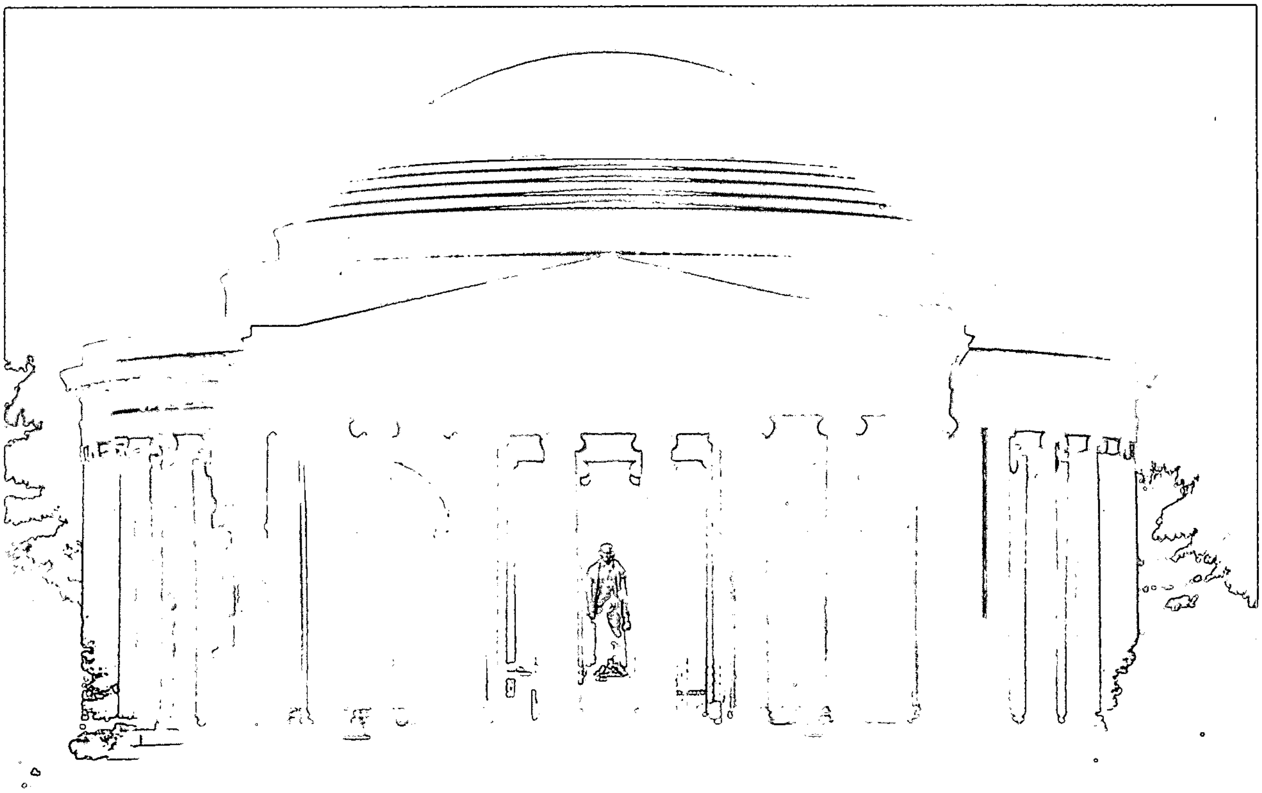
August 1997 • hc $49.95

LRP · 1800 30th Street, Ste. 314 · Boulder, CO 80301 · Tel: (303) 444-6684 · Fax: (303) 444-0824

APSA

93rd Annual Meeting

August 28–31, 1997
Washington, DC



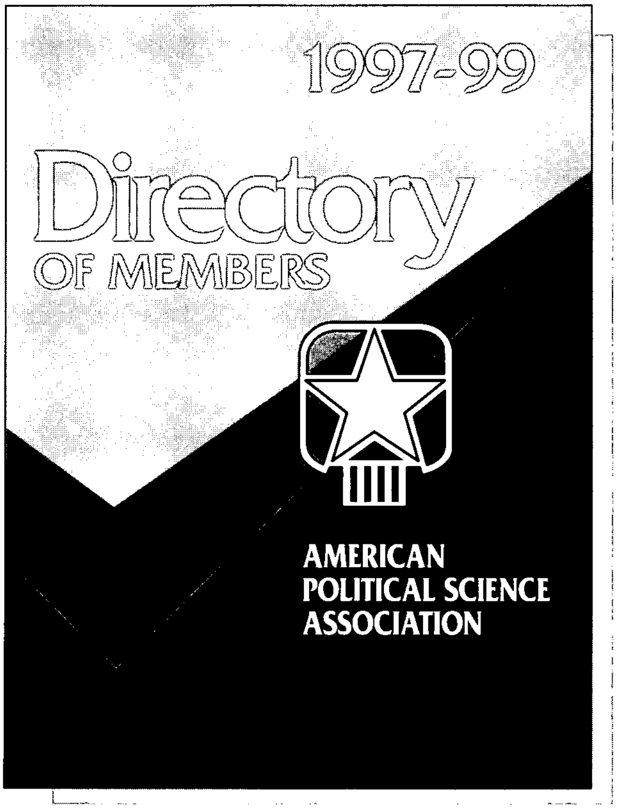
APSA Directory of Members, 1997-99

...THE SOURCE for identifying, locating and contacting political scientists in the U.S. and abroad

The **1997-99 APSA Directory of Members** lists the names of 13,000 members with their addresses, telephone, e-mail, and fax numbers, as well as each member's current position, institution and date of highest academic degree, fields and subfields of specialization, and membership in Organized Sections. The APSA Directory of Members is a reference for faculty, publishers, journal editors, academic advisors and administrators, funding agencies and program organizers.

Appendices list women members, African American, Asian American, Native American, and Latino members, fields and subfields of specialization, and geographic locations of members.

The price for members is $50. The price for nonmembers is $75.



New, Larger, Comprehensive Edition Available in Spring 1997

ORDER FORM

Enclosed is payment for _____ copies of the **Directory of Members 1997-99** at the rate of $50 for members and $75 for nonmembers. For each copy, add shipping at the rate of $6.00 for domestic; $18 for international air mail.

☐ Check Attached

☐ VISA/MASTERCARD

No.________________________________

Exp. Date__________

Signature___________________________

ALL ORDERS MUST BE PREPAID.

Name________________________________

Address______________________________

____

City/State/Zip__________________________

Phone (_____) ___________________

Mail to: Publications/APSA, 1527 New Hampshire Avenue, NW, Washington, DC 20036-1290
Or fax your order to 202-483-2657

CONTACTING APSA BY E-MAIL

apsa@apsanet.org	Use this address for general inquiries about APSA Services not mentioned below.
membership@apsanet.org	Send mail to this address to receive information about membership rates and services; request an address change or renew your membership (if you wish to transmit your credit card number by e-mail) or find out about joint memberships with ASA, ASIL, and AHA.
bunche@apsanet.org	Receive information and applications for the Ralph Bunche Summer Institute. A one month program intended for African-American students between their junior and senior years of undergraduate study.
meeting@apsanet.org	Find out the latest information on the APSA annual meeting, including registration costs, hotel accommodations and future sites and dates.
PS@apsanet.org	Use this address to submit material for the People in Political Science section and for the "News and Notes" sections; Questions about deadlines and manuscript submissions.
publications@apsanet.org	Receive a list of APSA publications, find out the status of an order, and place orders (if you wish to transmit your credit card number by e-mail).
minority_id@apsanet.org	Use this address to submit names of students who are candidates for the Minority Indentification Project and to request additional information about the program.
PSN@apsanet.org	Send employment listings to the Personnel Service Newsletter, a monthly publication listing employment opportunities for political scientists.
advertising@apsanet.org	Insertion rates for advertising in *PS*, *APSR*, and the Annual Meeting Final Program.
apsr@ssc.msu.edu	Use this address to contact the editorial office of the *American Political Science Review*. Please note that instructions for submission of articles to the *Review* are printed in each issue. The editorial office does not handle business issues. For address changes, advertising, or other non-editorial issues, please contact APSA headquarters at the e-mail addresses listed above.
information@apsanet.org	For general information about APSA services this automated server will return specific files you request. There are files on membership, publications, research grants, fellowships, and more. Type "send list" in the subject field of your message to receive a list of available files.

The APSA Political Scientists on E-Mail:

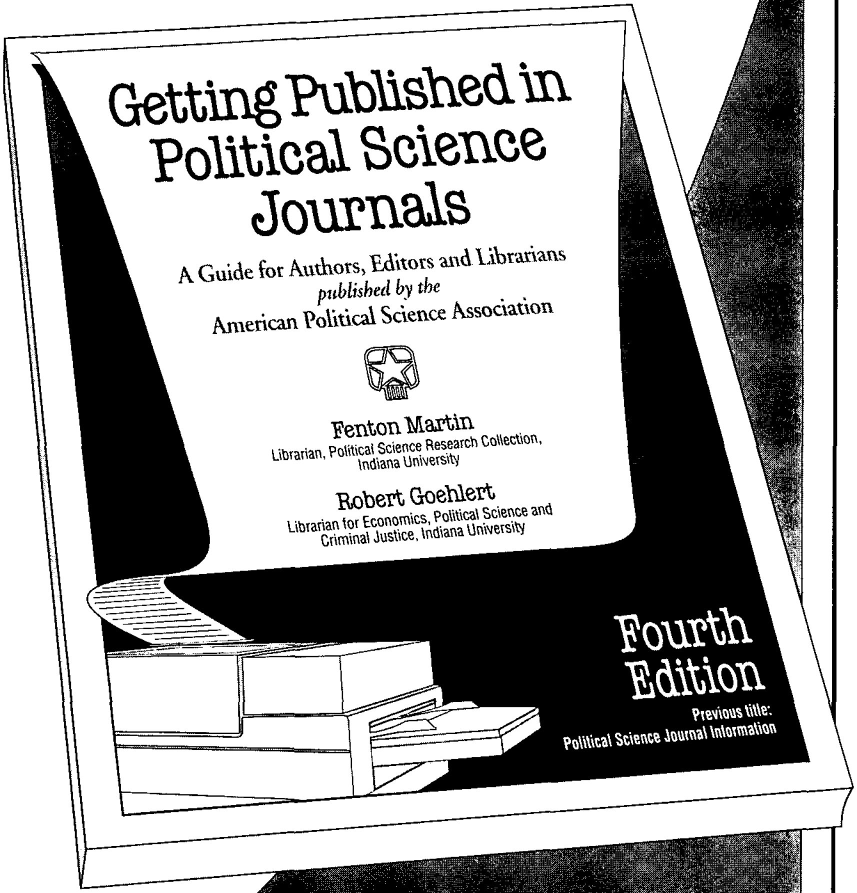
rudder@apsanet.org	Catherine E. Rudder, Executive Director
rhauck@apsanet.org	Robert J-P. Hauck, Deputy Director
smann@apsanet.org	Sheilah Mann, Director of Educational Affairs
woodard@apsanet.org	Maurice Woodard, Director of Minority Affairs
jyin@apsanet.org	Jun Yin. Program Director

Publishing an Article? Editing a Journal? Locating Research?

You Need the New Expanded Guide to Political Science Journals...

A comprehensive guide to 103 journals covering all fields in political science that describes each journal's:

- requirements for submitting and for style of manuscripts
- procedures for reviewing manuscripts
- numbers of manuscripts received and published
- Also, advice to authors on identifying and selecting journals for your research articles



Getting Published in Political Science Journals

A Guide for Authors, Editors and Librarians
published by the
American Political Science Association

Fenton Martin
Librarian, Political Science Research Collection, Indiana University

Robert Goehlert
Librarian for Economics, Political Science and Criminal Justice, Indiana University

Fourth Edition
Previous title:
Political Science Journal Information

Getting Published in Political Science is a reference for

- authors — on the specialties, requirements and procedures of journals
- teachers and students — on where to find research
- editors — on the processes, policies and styles of journals
- librarians — on the topics, circulation and formats of journals

Note to APSA Members:

APSA considers this book to be such an important reference for our members, that we have held the member price to just $18 — the same cost as the 3rd edition 7 years ago — though the new edition is a much larger, more comprehensive publication!

ORDER FORM

Enclosed is payment for ______ copies of **Getting Published in Political Science Journals** at the rate of $18 for members and $35 for nonmembers. For *each copy, add $4 for shipping.*

☐ Check Attached

☐ VISA/MASTERCARD

No.______________________________

Exp. Date__________

Signature______________________________

ALL ORDERS MUST BE PREPAID.

Name______________________________

Address______________________________

____

City/State/Zip______________________________

Phone (_____) ______________________

Mail to: Publications/APSA, 1527 New Hampshire Avenue, NW, Washington, DC 20036-1290
Or fax your order to 202-483-2657

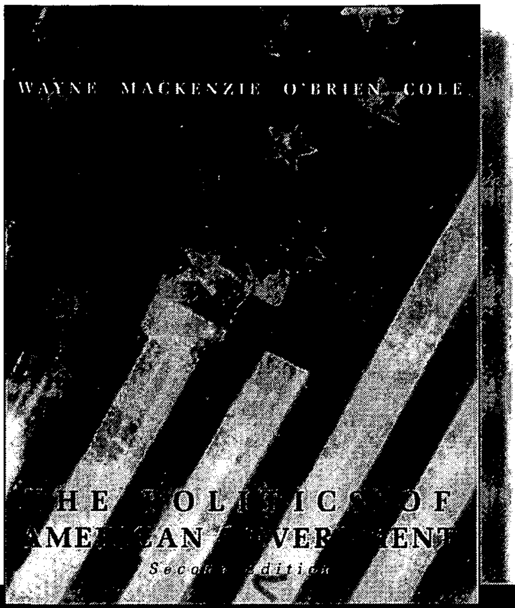
**Think Critically,
Not Cynically.**

THE POLITICS OF AMERICAN GOVERNMENT, 2/E

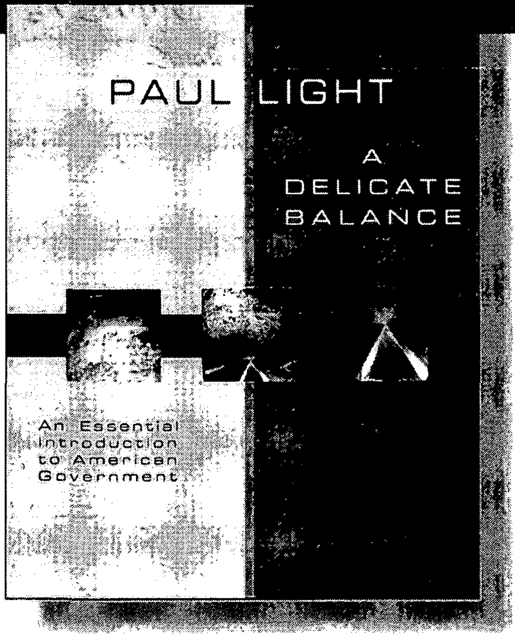
STEPHEN J. WAYNE, *Georgetown University*
G. CALVIN MACKENZIE, *Colby College*
DAVID M. O'BRIEN, *University of Virginia*
RICHARD L. COLE, *University of Texas at Arlington*

Complete Edition (Hardcover): ISBN 0-312-13775-3
Paperbound: ISBN 0-312-14797-X

Basic Edition (no policy chapters): ISBN 0-312-13915-2


WAYNE MACKENZIE O'BRIEN COLE
THE POLITICS OF AMERICAN GOVERNMENT
Second Edition

Bringing You Choice in American Politics


PAUL LIGHT
A DELICATE BALANCE
An Essential Introduction to American Government

Just Read It.

A DELICATE BALANCE
An Essential Introduction to American Government

PAUL LIGHT, *Pew Charitable Trusts and the University of Minnesota*

ISBN 0-312-08969-4

ST. MARTIN'S PRESS
345 Park Avenue South, New York, NY 10010
1-800-446-8923 · Fax:(212)686-9492
email: facultyservices@sasmp.com

CHATHAM HOUSE PUBLISHERS

Box One, Chatham NJ 07928 **Tel: (201) 635-2059** **Fax: (201) 635-9366**
E-mail: chatham@gti.net **http://www.gti.net/chatham**

The Election of 1996

Reports and Interpretations

Edited by Gerald M. Pomper, *Rutgers University*

The latest in the Chatham House series on presidential elections offers timely, insightful commentary by a distinguished team of expert observers: Burnham sets the stage, Mayer reviews the campaign, Just reports on the media, Corrado sums up the financing, Keeter surveys public opinion, Pomper analyzes the presidential election, Hershey tallies the congressional elections, and McWilliams explains the meaning of it all. *Reserve your copy today.*

1-56643-055-0 *paper*

Judges on Judging

Views From the Bench

Edited by David M. O'Brien, *University of Virginia*

This comprehensive collection of papers by state and federal judges in the United States looks at how they approach their function as judges and the role of the judiciary in the American constitutional system. David O'Brien seeks to contribute to the ongoing debate over off-the-bench commentaries and encourage readers to think about the qualities of judges and the role of courts in American politics.

1-56643-042-9 *paper*

Present Discontents

American Politics in the Very Late Twentieth Century

Byron E. Shafer, *Oxford University*, et al.

For introductory American politics courses, this is a diverse collection of lively and provocative assessments of the evolution of American politics across the postwar period: **Joel Silbey** (parties), **Michael Barone** (society), **Charles Jones** (institutions), **Alan Ehrenhalt** (culture), **Edward Carmines and Geoffrey Layman** (issues), **Thomas Edsall** (agendas), and **Byron Shafer** (coalitions).

1-56643-050-X *paper*

Thinking Politics

Perspectives in Ancient, Modern, and Postmodern Political Theory

Leslie Paul Thiele, *University of Florida*

Thinking Politics describes the art and craft of political theory by way of a refreshingly original examination of ancient, modern, and postmodern political thought. While familiarizing students with the tradition of political theory, this innovative book integrates many contemporary theoretical and political concerns. It develops both an understanding of the domain of politics and a sensibility to the ways in which this domain may be intellectually and morally engaged. What it means to theorize, and to theorize well, is a central focus.

1-56643-053-4 *paper*

How Russia Votes

Stephen White, *University of Glasgow*, Richard Rose, *University of Strathclyde*, and
Ian McAllister, *University of Manchester*

"A mine of information and analysis, *How Russia Votes* is a splendid achievement. Combining the considerable talents of an area specialist, a psephologist, and methodologist, this well-written study is must reading for courses on Russian government, comparative politics, and election behavior."

1-56643-037-2 *paper*

Table of Contents

EDITOR'S NOTES vi

ARTICLES

Unequal Participation: Democracy's Unresolved Dilemma
Arend Lijphart 1

Capabilities, Perception, and Escalation
Bruce Bueno de Mesquita, James D. Morrow, and
Ethan R. Zorick 15

Separation-of-Powers Games in the Positive Theory of
Congress and Courts
Jeffrey A. Segal 28

Politics Matters: Political Events as Catalysts for Preadult Socialization
David O. Sears and Nicholas A. Valentino 45

Political Institutions and Satisfaction with Democracy: A Cross-National
Analysis of Consensus and Majoritarian Systems
Christopher J. Anderson and Christine A. Guillory 66

Institutional Arrangements and the Creation of Social Capital:
The Effects of Public School Choice
Mark Schneider, Paul Teske, Melissa Marschall, Michael Mintrom,
and Christine Roch 82

Alliances, Preferential Trading Arrangements, and International Trade
Edward D. Mansfield and Rachel Bronson 94

Beyond Quid Pro Quo: What's Wrong with Private Gain from
Public Office?
Andrew Stark 108

Legal Theory in the Collapse of Weimar: Contemporary Lessons?
David Dyzenhaus 121

Stability and Coordination in Duverger's Law: A Formal Model of
Preelection Polls and Strategic Voting
Mark Fey 135

FORUM

Trends in the Partisan Composition of State Legislatures:
A Response to Fiorina
Jeffrey M. Stonecash and Anna M. Agathangelou 148

Professionalism, Realignment, and Representation
Morris P. Fiorina 156

BOOK REVIEWS

Political Theory 163
American Politics 181
Comparative Politics 206
International Relations 227